NOTICES

OF THE

PROCEEDINGS

AT THE

MEETINGS OF THE MEMBERS

OF THE

Royal Institution of Great Britain

WITH

ABSTRACTS OF THE DISCOURSES

DELIVERED AT

THE EVENING MEETINGS

VOLUME XVI
1899—1901

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CONTENTS.

1899.

1900.

1901.

PLATES.

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 20, 1899.

Sir Frederick Bramwell, Bart., D.C.L. LL.D. F.R.S., Honorary Secretary and Vice-President, in the Chair.

Professor Dewar, M.A. LL.D. F.R.S. *M.R.I.*

Liquid Hydrogen.

From the year 1878, when the experiments of Cailletet and Pictet were attracting the attention of the scientific world, it became a common habit in text-books to speak of all the permanent gases, without any qualification, as having been liquefied, whereas these experimentalists, by the production of an instantaneous mist in a glass tube of small bore, or a transitory liquid jet in a gas expanding under high compression into air, had only adduced evidence that sooner or later the static liquid form of all the known gases would be attained. Neither Pictet or Cailletet in their experiments ever succeeded in collecting any of the permanent gases in that liquid form for scientific examination. Yet we meet continually in scientific literature with expressions which lead one to believe that they did. For instance, the following extract from the 'Proceedings' of the Royal Society, 1878, illustrates this point very well: "This award (Davy Medal) is made to these distinguished men (Cailletet and Pictet) for having independently and contemporaneously liquefied the whole of the gases hitherto called permanent." Many other quotations of the same kind may be made. As a matter of fact six years elapsed, during which active investigation in this department was being prosecuted, before Wroblewski and Olszewski succeeded in obtaining oxygen as a static liquid, and to collect liquid hydrogen, which is a much more difficult problem, has taken just twenty years from the date of the Pictet and Cailletet experiments.

Wroblewski made the first conclusive experiment on the liquefaction of hydrogen in January 1884. He found that the gas cooled in a capillary glass tube to the boiling point of oxygen, and expanded quickly from 100 to 1 atmosphere, showed the same appearance of sudden ebullition lasting for a fraction of a second, as Cailletet had seen in his early oxygen experiments. No sooner had the announcement been made, than Olszewski confirmed the result by expanding hydrogen from 190 atmospheres, previously cooled to the temperature given by liquid oxygen and nitrogen evaporating under diminished pressure. Olszewski, however, declared in 1884 that he saw colourless

drops, and by partial expansion to 40 atmospheres, the liquid hydrogen was seen by him running down the tube. Wroblewski could not confirm Olszewski's results, his hydrogen being always obtained in the form of what he called a "liquide dynamique," or the appearance of an instantaneous froth. Olszewski himself seven years later repeated his experiments of 1884 on a larger scale, confirming Wroblewski's results, thereby proving that the so-called liquid hydrogen of the earlier experiments must have been due to some impurity. The following extract from Wroblewski's paper states very clearly the results of his work on Hydrogen:—

"L'hydrogène soumis à la pression de 180 atm. jusqu'à 190 atm., refroidi par l'azote bouillant dans la vide (à la température de sa solidification) et détendu brusquement sous la pression atmosphérique présente une mousse bien visible. De la couleur grise de cette mousse, où l'œil ne peut distinguer des gouttelettes incolores, on ne peut pas encore deviner quelle apparence aurait l'hydrogène à l'état de liquide statique et l'on est encore moins autorisé à préciser s'il a ou non une apparence métallique. J'ai pu placer dans cette mousse ma pile thermo-électrique, et j'ai obtenu suivant les pressions employées des températures de $-208°$ jusqu'à $-211°$ C. Je ne peux pas encore dire dans quelle relation se trouvent ces nombres avec la température réelle de la mousse ou avec la température d'ébullition de l'hydrogène sous la pression atmosphérique, puisque je n'ai pas encore la certitude que la faible durée de ce phénomène ait permis à la pile de se refroidir complètement. Néanmoins, je crois aujourd'hui de mon devoir de publier ces résultats, afin de préciser l'état actuel de la question de la liquéfaction de l'hydrogène." *

It is well to note that the lowest thermo-electric temperature recorded by Wroblewski during the adiabatic expansion of the hydrogen (namely, $-211°$) is really equivalent to a much lower temperature on the gas-thermometer scale. The most probable value is $-230°$, and this must be regarded as the highest temperature of the liquid state, or the critical point of hydrogen, according to his observations. In a posthumous paper of Wroblewski's on 'The Compression of Hydrogen,' published in 1889, an account appears of further attempts which he had made to liquefy hydrogen. The gas compressed to 110 atmospheres, was cooled by means of liquid nitrogen under exhaustion to $-213 \cdot 8°$. By suddenly reducing the pressure, as low a temperature as $-223°$ on his scale was recorded, but without any signs of liquefaction. This expansion gives a theoretical temperature of about 15° absolute in the gas particles. The above methods having failed to produce static hydrogen, Wroblewski suggested that the result might be attained by the use of hydrogen gas as a cooling agent. From this time until his death in the year 1888, Wroblewski devoted his time to a laborious research on the iso-

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* Compt. Rend. 1885, 100, 981.

thermals of hydrogen at low temperatures. The data thus arrived at enabled him, by the use of Van der Waal's formulæ, to calculate the critical constants, and also the boiling point of liquid hydrogen.

Olszewski returned to the subject in 1891, repeating and correcting his old experiments of 1884, which Wroblewski had failed to confirm, using now a glass tube 7 mm. in diameter instead of one of 2 mm. as in the early trials. He says: "On repeating my former experiments, I had no hope of obtaining a lower temperature by means of any cooling agent, but I hoped that the expansion of hydrogen would be more efficacious, on account of the larger scale on which the experiments were made." The results of these experiments Olszewski describes as follows: "The phenomenon of hydrogen ebullition, which was then observed, was much more marked and much longer than during my former investigations in the same direction. But even then I could not perceive any meniscus of liquid hydrogen." Further, "*The reason for which it has not hitherto been possible to liquefy hydrogen in a static state, is that there exists no gas having a density between those of hydrogen and of nitrogen, and which might be for instance* 7–10 (H = 1). Such a gas could be liquefied by means of liquid oxygen or air as cooling agent, and be afterwards used as a frigorific menstruum in the liquefaction of hydrogen."

Professor Olszewski, in 1895, determined the temperature reached in the momentary adiabatic expansion of hydrogen at low temperatures, just as Wroblewski had done in 1885, only he employed a platinum-resistance thermometer instead of a thermo-junction. For this purpose he used a small steel bottle of 20 or 30 cc. capacity, containing a platinum-resistance thermometer; in this way, the temperatures registered were regarded as those of the critical and boiling points of liquid hydrogen, a substance which could not be seen under the circumstances and was only assumed to exist for a second or two during the expansion of the gaseous hydrogen in the small steel bottle.

The results arrived at by Wroblewski and Olzewski are given in the following table, and it will be shown later on that Wroblewski's constants are nearest the truth.

	Wroblewski, 1885.	Olszewski, 1895.
Critical temperature	−240°	−234°
Boiling point	−250°	−243°
Critical pressure	13 atm.	20 atm.

The accuracy of Wroblewski's deductions regarding the chief constants of liquid hydrogen following from a study of the isothermals of the gas is a signal triumph for the theory of Van der Waals and a monument to the genius of the Cracow physicist. From these results we may safely infer that supposing a gas is hereafter discovered in small quantity four times more volatile than liquid hydrogen, having a boiling point of about 5° absolute, and

therefore incapable of direct liquefaction by the use of liquid hydrogen, yet by a study of its isothermals we shall succeed in finding out its most important liquid constants, although the isolation of the real liquid may for the time be impossible.

In a paper published in the 'Philosophical Magazine,' September 1884, "On the Liquefaction of Oxygen and the Critical Volumes of Fluids," the suggestion was made that the critical pressure of hydrogen was wrong, and that instead of being 99 atmospheres (as deduced by Sarrau from Amagat's isothermals) the gas had probably an abnormally low value for this constant. This view was substantially confirmed by Wroblewski finding the critical pressure of 13·3 atmospheres, or about one-fourth of that of oxygen. The 'Chemical News,' September 7, 1894, contains an account of the stage the author's hydrogen experiments had reached at that date. The object was to collect liquid hydrogen at its boiling point, in an open vacuum vessel, which is a much more difficult problem than seeing it in a glass tube under pressure and at a higher temperature. In order to raise the critical point of hydrogen to about $-210°$, from 2 to 5 per cent. of nitrogen or air was mixed with it. *This is simply making an artificial gas containing a large proportion of hydrogen which is capable of liquefaction by the use of liquid air.* The results are summed up in the following extract from the paper: "One thing can, however, be proved by the use of the gaseous mixture of hydrogen and nitrogen, namely that by subjecting it to a high compression at a temperature of $-200°$ and expanding the resulting liquid into air, a much lower temperature than anything that has been recorded up to the present time can be reached. This is proved by the fact that such a mixed gas gives, under the conditions, a paste or jelly of solid nitrogen, evidently giving off hydrogen, because the gas coming off burns fiercely. Even when hydrogen containing only some 2 to 5 per cent. of air is similarly treated, the result is a white solid matter (solid air) along with a clear liquid of low density, which is so exceedingly volatile that no known device for collecting it has been successful." This was in all probability the first liquid hydrogen obtained, and the method is applicable to other difficultly liquefactible gases.

Continuing the investigations during the winter of 1894, and the greater part of 1895, the author read a paper before the Chemical Society in December of that year entitled, "The Liquefaction of Air and Research at Low Temperatures," * in which occasion was taken to describe for the first time the mode of production and use of a Liquid Hydrogen Jet. At the same meeting Professor William Ramsay made an announcement of a sensational character, which amounted to stating that my hydrogen results had been not only anticipated but bettered. The statement made to the Society by Professor Ramsay, reads as

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* 'Proceedings' of the Chemical Society, No. 158, 1895.

follows: "*Professor Olszewski had succeeded in liquefying hydrogen, and from unpublished information received from Cracow, he (Ramsay) was able to state that a fair amount of liquid had been obtained, not as a froth, but in a state of quiet ebullition, by surrounding a tube containing compressed hydrogen by another tube also containing compressed hydrogen at the temperature of oxygen boiling at a very low pressure. On allowing the hydrogen in the middle jacket suddenly to expand, the hydrogen in the innermost tube liquefied, and was seen to have a meniscus. Its critical point and its boiling point, under atmospheric pressure, were determined by means of a resistance thermometer.*" *

This announcement of Professor Ramsay's had from its very specific and detailed experimental character the merit of the appearance of being genuine, although it was never substantiated, either by the production of the Cracow document, or by any subsequent publication of such important results by Professor Olszewski himself. My observation at the time on Professor Ramsay's communication was that quotations had been made in my paper from the most recent publications of Professor Olszewski in which he made *no mention of getting* "*Static Hydrogen or of seeing a meniscus*" *or of getting as Professor Ramsay alleged* "*a fair amount of liquid, not as a froth, but in a state of quiet ebullition.*" To achieve such a result would require a very different scale of experiment from anything Professor Olszewski had so far described. Naturally an early corroboration of the startling statement made by Professor Ramsay as to this alleged anticipation was expected, but strange to say Professor Olszewski published no confirmation of the experiments detailed by Professor Ramsay in scientific journals of date immediately preceding my paper or during the following years 1896, 1897 or up to May 1898. The moment the announcement was made by me to the Royal Society in May 1898 that, after years of labour, hydrogen had at last been obtained as a static liquid, Professor Ramsay repeated the story to the Royal Society that Olszewski had anticipated my results (basing the assertion solely on the contents of the old letter received some two and a half years before), in spite of the fact that during the interval he, Professor Ramsay, must have known that Professor Olszewski had never corroborated in any publication either the form of the experiments he had so minutely described or the results which were said to follow. Challenged by me at the Royal Society Meeting on May 12, 1898, to produce Olszewski's letter of 1895, he did not do so, but at the next meeting of the Society, Professor Ramsay read a letter he had received during the interval from Professor Olszewski, denying that he had ever stated that he had succeeded in producing static liquid hydrogen. This oral communication of the contents of the new Olszewski letter (of which it is to be regretted there is no record in the published proceedings of the Royal Society) is the only kind of retraction Professor Ramsay has thought fit to make of his

* 'Proceedings' of the Chemical Society, No. 195, 1897–1898.

published mis-statements of fact. No satisfactory explanation has yet been given by Professor Ramsay of his twice-repeated categorical statements made before scientific bodies of the results of experiments which, in fact, had never been made by their alleged author. The publicity that has been given to this controversy makes it imperative that the matter should not be passed over, but once for all recorded.

The report of a Friday Evening Discourse on "New Researches on Liquid Air" * contains a drawing of the apparatus employed

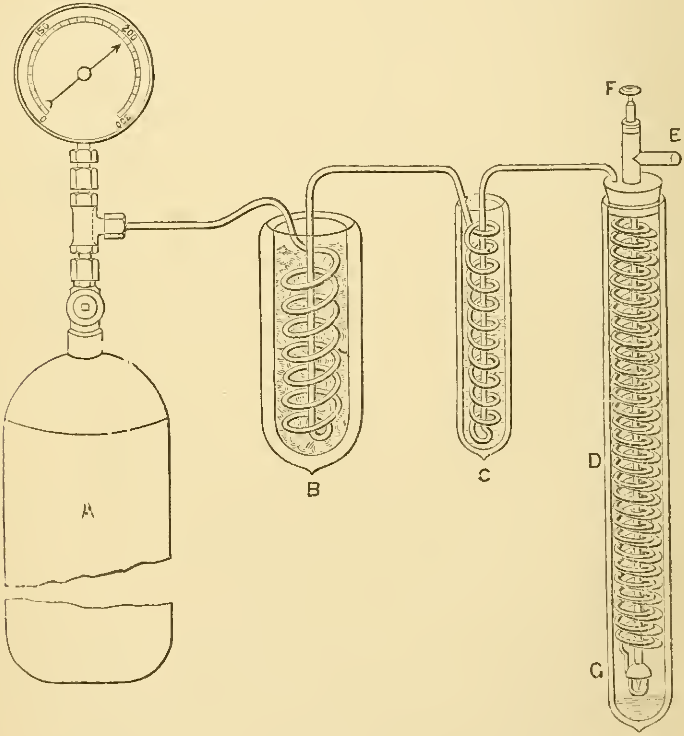

Fig. 1.

for the production of a jet of hydrogen containing visible liquid. This is reproduced in Fig. 1. A represents one of the hydrogen cylinders; B and C, vacuum vessels containing carbonic acid under exhaustion and liquid air respectively; D is the coil, G the pin-hole nozzle, and F the valve. By means of this hydrogen jet, liquid air can be quickly transformed into a hard solid. It was shown that

* 'Proceedings' of the Royal Institution, 1896.

such a jet could be used to cool bodies below the temperature that it is possible to reach by the use of liquid air, but all attempts to collect the liquid hydrogen from the jet in vacuum vessels failed. No other investigator improved on my results,* or has indeed touched the subject during the last three years. The type of apparatus used in these experiments worked well, so it was resolved to construct a much larger liquid-air plant, and to combine with it circuits and arrangements for the liquefaction of hydrogen. This apparatus took a year to build, and many months have been occupied in the testing and preliminary trials. The many failures and defeats need not be detailed.

On May 10, 1898, starting with hydrogen cooled to – 205°, and under a pressure of 180 atmospheres, escaping continuously from the nozzle of a coil of pipe at the rate of about 10 to 15 cubic feet per minute, in a vacuum vessel doubly silvered and of special construction, all surrounded with a space kept below – 200°, liquid hydrogen commenced to drop from this vacuum vessel into another doubly isolated by being surrounded with a third vacuum vessel. In about five minutes, 20 cc. of liquid hydrogen were collected, when the hydrogen jet froze up, from the accumulation of air in the pipes frozen out from the impure hydrogen. The yield of liquid was about 1 per cent. of the gas. The hydrogen in the liquid condition is clear and colourless, showing no absorption spectrum, and the meniscus is as well defined as in the case of liquid air. The liquid must have a relatively high refractive index and dispersion, and the density appears at first sight to be in excess of the theoretical density, namely 0·18 to 0·12, which we deduce respectively from the atomic volume of organic compounds, and the limiting density found by Amagat for hydrogen gas under infinite compression. A preliminary attempt, however, to weigh a small glass bulb in the liquid made the density only about 0·08, or half the theoretical. My old experiments on the density of hydrogen in palladium gave a value for the combined element of 0·62. Not having arrangements at hand to determine the boiling point other than a thermo-junction which gave entirely fallacious results, experiments were made to prove the excessively low temperature of the boiling fluid. In the first place if a long piece of glass tubing, sealed at one end and open to the air at the other, is cooled by immersing the closed end in the liquid hydrogen, the tube immediately fills where it is cooled with solid air. A small glass tube filled with liquid oxygen when cooled in liquid hydrogen is transformed into a bluish white solid. This is a proof that the boiling point of hydrogen is much lower than any temperature previously reached by the use of liquid nitrogen evaporating *in vacuo*, seeing oxygen always remains liquid under such conditions. A first trial of putting liquid hydrogen under exhaustion gave no appearance of transition into the solid state. When the vacuum tube containing liquid hydrogen is immersed in liquid air so that the external wall

* 'Proceedings of the Chemical Society' (No. 158), 1895.

of the vacuum vessel is maintained at about $-190°$, the hydrogen is found to evaporate at a rate not far removed from that of liquid air from a similar vacuum vessel under the ordinary conditions of temperature. This leads me to the conclusion that, with proper isolation, it will be possible to manipulate liquid hydrogen as easily as liquid air.

The boiling point of liquid hydrogen at atmospheric pressure in the first instance was determined by a *platinum-resistance thermometer.* This was constructed of pure metal and had a resistance of 5·3 ohms at 0° C., which fell to about 0·1 ohm when the thermometer was immersed in liquid hydrogen. The reduction of this resistance to normal air thermometer degrees gave the boiling points $-238 \cdot 2°$ and $-238 \cdot 9°$ respectively by two extrapolation methods, and $-237°$ by a Dickson formula.* The boiling point of the liquid seems therefore to be $-238°$ C. or 35° absolute, and is thus about 5° higher than that obtained by Olszewski by the adiabatic expansion of the compressed gas, and about 8° higher than that deduced by Wroblewski from Van der Waal's equation. From these results it may be inferred that the critical point of hydrogen is about 50° absolute, and that the critical pressure will probably not exceed 15 atmospheres.

If we assume the resistance reduced to zero, then the temperature registered by the electric thermometer ought to be $-244°$ C. At the boiling point of hydrogen, registered by the electric-resistance thermometer, if the law correlating resistance and temperature can be pressed to its limits, a lowering of the boiling point of hydrogen by 5° or 6° C. would therefore produce a condition of affairs in which the platinum would have no resistance, or would become a perfect conductor. Now we have every reason to believe that hydrogen, like other liquids, will boil at a lower temperature the lower the pressure under which it is volatilised. The question arises, how much lowering of the temperature can we practically anticipate? For this purpose we have the *boiling point given by the hydrogen gas thermometer,* and critical data available, from which we can calculate an approximate vapour pressure formula, accepting 22° absolute as about the boiling point, 33° absolute as the critical temperature, and 15·4 atmospheres as the critical pressure; then, as a first approximation—

$$\log. p = 6 \cdot 410 - \frac{77 \cdot 62}{\mathrm{T}} \text{ mm.} \quad . \quad . \quad . \quad (1)$$

If, instead of using the critical pressure in the calculation, we assume the molecular latent heat of hydrogen to be proportional to the absolute boiling point, then from a comparison with an expression of the same kind, which gives accurate results for oxygen tensions below one atmosphere, we can derive another expression for hydrogen vapour

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* See Phil. Mag., 45, 525, 1898.

pressures, which ought to be applicable to boiling points under reduced pressure.

The resulting formula is—

$$\log. p = 7 \cdot 0808 - \frac{88}{T} \text{ mm.} \quad . \quad . \quad . \quad . \quad (2)$$

Now formula (1) gives a boiling point of 14·2° absolute under a pressure of 25 mm., whereas the second equation (2) gives for the same pressure 15·4° absolute. As the absolute boiling point under atmospheric pressure is about 22°, both expressions lead to the conclusion that ebullition under 25 mm. pressure ought to reduce the boiling point some 7° C. For some time experiments have been in progress with the object of determining the temperature of hydrogen boiling under about 25 mm. pressure, by the use of the platinum thermometer; but the difficulties encountered have been great, and repeated failures very exasperating. The troubles arise from the conduction of heat by the leads; the small latent heat of hydrogen volume for volume as compared with liquid air; the inefficiency of heat isolation; and the strain on the thermometer brought about by solid air freezing on it and distorting the coil of wire. In many experiments, the result has been that all the liquid hydrogen has evaporated before the pressure was reduced to 25 mm., or the thermometer was left imperfectly covered. The apparatus employed will be understood from Fig. 2. The liquid hydrogen collected in the vacuum vessel A was suspended in a larger vessel of the same kind B, which is so constructed that a spiral tube joins the inner and outer test-tubes of which B is made, thereby making an opening into the interior at C. The resistance thermometer D and leads E pass through a rubber cork F, and the exhaustion takes place through C. In this way the cold

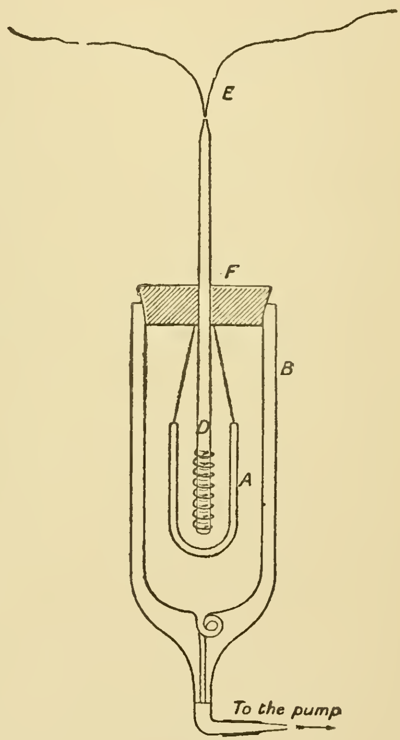

Fig. 2.

vapours are drawn over the outside of the hydrogen vacuum vessel, and this helps to isolate the liquid from the convective currents of gas. To effect proper isolation, the whole apparatus ought to be immersed in liquid air under exhaustion. Arrangements of this kind add to the complication, so in the first instance the liquid was used as described. The liquid hydrogen evaporated quietly and steadily under a diminished pressure of about 25 mm. Naturally the liquid does not last long, so the resistance has to be taken quickly. Just before the reduction of pressure began, the resistance of the thermometer was 0·131 ohm. This result compares favourably with the former observation on the boiling point, which gave a resistance of 0·129 ohm. On reducing the pressure,the resistance diminished to 0·114 ohm, and kept steady for some time. The lowest reading of resistance was 0·112 ohm. This value corresponds to $-239{\cdot}1°$ C., or only one degree lower on its own scale, than the boiling point at atmospheric pressure, whereas the temperature ought to have been reduced at least 5° under the assumed exhaustion according to the gas thermometer scale. The position of the observation on the curve of the relation of temperature and resistance for No. 7 thermometer is shown on the accompanying diagram (Fig. 3). As a matter of fact, however, this platinum thermometer was, when placed in liquid hydrogen, cooled at starting below its own temperature of perfect conductivity, so that no exhaustion was needed to bring it to this point. The question arises then as to what is the explanation of this result? Has the platinum resistance thermometer arrived at a limiting resistance about the boiling point of hydrogen, so that at a lower temperature its changes in resistance become relatively small—the curve having become practically asymptotic to the axis of temperature? That is the most probable supposition, and it further explains the fact that the temperature of boiling hydrogen obtained by the linear extrapolation of the resistance temperature results in values that are not low enough.

As the molecular latent heats of liquids are proportional to their absolute boiling points, the latent heat of liquid hydrogen will be about two-fifths that of liquid oxygen. It will be shown later, however, that we can reach from 14° to 15° absolute by the evaporation of liquid hydrogen under exhaustion. From analogy, it is probable that the practicable lowering of temperature to be obtained by evaporating liquid hydrogen under pressures of a few mm. cannot amount to more than 10° to 12° C., and it may be said with certainty that, assuming the boiling point 35° absolute to be correct, no means are at present known for approaching nearer than 20° to 25° to the absolute zero of temperature. The true boiling point is in reality about $-252°$ C., in terms *of the gas-thermometer scale*, and the latent heat of the liquid is therefore about two-ninths that of an equal volume of oxygen, or one-fourth that of liquid nitrogen. The platinum-resistance thermometer had a zero point of $-263{\cdot}2$ platinum degrees, and when immersed in boiling liquid hydrogen, indicated a temperature of $-256{\cdot}8°$ on the same scale, or 6·4 platinum degrees

from the point at which the metal would theoretically become a perfect conductor. The effect of cooling platinum from the boiling point of liquid oxygen to that of liquid hydrogen is to diminish its resistance to one-eleventh.

The difficulties in liquefying hydrogen caused by the presence of air in the gas have been referred to,* and later experiments had for their object the removal of this fruitful source of trouble. This is by no means an easy task, as quantities amounting to only a fraction of one per cent. accumulate in the solid state, and eventually choke the nozzle of the apparatus, necessitating the abandonment of the opera-

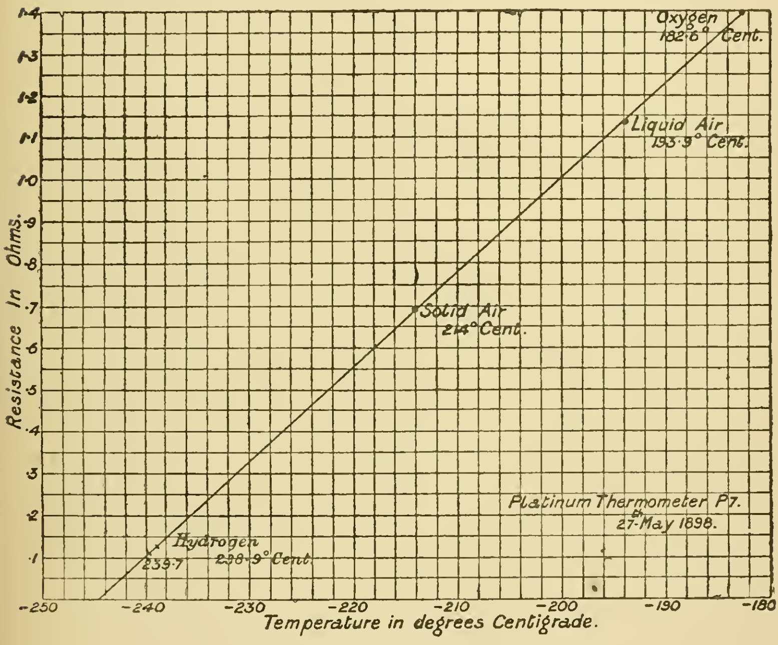

Fig. 3.

tion. Later experiments enabled me to procure a larger supply of liquid hydrogen with which the determination of certain physical constants has been continued. The first observations made with a pure platinum-resistance thermometer had given − 238° as the boiling point. A new thermometer, constructed of platinum from a different source, gave practically the same value. As these results might be affected by some constant error, the determination was checked by employing a thermometer constructed from an alloy of rhodium and

* 'Proceedings,' 1898, 14, 130.

platinum, containing 10 per cent. of the former. Alloys had been shown by Professor Fleming and the author to differ from pure metals in showing no sign of becoming perfect conductors at the absolute zero of temperature, and a study of the rhodium-platinum alloy had shown that the change in conductivity produced by cooling from 0° to the boiling point of liquid air is regular and may be represented by a straight line. As determined by the rhodium-platinum thermometer, the boiling point of hydrogen was found to be − 246° or some 8° lower than the platinum thermometer gave. Two ways of explaining the discrepancy between the two values suggested themselves. Pure platinum, although its resistance may be represented by a straight line almost down to the solidifying point of air, shows signs of a departure from regularity at about this point, and the curve may become asymptotic at lower temperatures. On the other hand, the resistance of the rhodium-platinum alloy diminishes less rapidly at these lower temperatures and is much higher than that of pure platinum under similar conditions. It follows that its resistance curve, in all probability, deviates less from a straight line than is the case with platinum. Either cause would explain the differences observed, but the lower boiling point (−246° or 27° absolute) seemed to be the more probable as it agreed very fairly with the value for the boiling point calculated by the author from Wroblewski's results. As the use of other pure metals or alloys was not likely to lead to more satisfactory results, the problem had to be attacked in a different way, namely, by means of an "air" thermometer containing hydrogen under diminished pressure.

A first attempt has been made at determining the boiling-point by a constant-volume hydrogen thermometer working under diminished pressure. This thermometer, which gave the boiling point of oxygen as 90·5° absolute or − 182·5°, gave for hydrogen 21° absolute or −252°. The three determinations that have been made are then as follows: (1) pure platinum resistance thermometer, 35° absolute; (2) rhodium-platinum resistance thermometer 27° absolute; (3) hydrogen thermometer, 21° absolute. From this it appears that the boiling point of hydrogen is really lower than was anticipated, and must range between 20° and 22° absolute. Further experiments will be made with thermometers filled with hydrogen prepared from different sources. A hydrogen thermometer filled with the gas obtained from the evaporation of the liquid hydrogen itself must be employed.

The approximate density of liquid hydrogen at its boiling point was found by measuring the volume of the gas obtained by evaporating 10 cc. of the liquid, and is slightly less than 0·07, or about one-sixth that of liquid marsh-gas, which is the lightest liquid known. It is remarkable that, with so low a density, liquid hydrogen is so easily seen, has so well defined a meniscus, and can be so readily collected and manipulated in vacuum vessels. As hydrogen occluded in palladium has a density of 0·62, it follows that it must

be associated with the metal in some other state than that of liquefaction.

The atomic volume of liquid hydrogen at its boiling point is about 14·3, the atomic volumes of liquid oxygen and nitrogen being 13·7 and 16·6 respectively at their boiling points. The weight of a litre of hydrogen gas at the boiling point of the liquid is about the same as that of air, at the ordinary temperature. The ratio of the density of hydrogen gas at the boiling point to that of the liquid is approximately 1 : 60, as compared with a ratio of 1 : 255 in the case of oxygen under similar conditions.

The specific heat of hydrogen in the gaseous state and in hydrogenised palladium is 3·4, but may very probably be 6·4 in the liquid substance. Such a liquid would be unique in its properties; but as the volume of one gramme of liquid hydrogen is about 14–15 cc., the specific heat per unit volume must be nearly 0·5, which is about that of liquid air. It is highly probable, therefore, that the remarkable properties of liquid hydrogen predicted by theory will prove to be less astonishing when they are compared with those of liquid air, volume for volume, at corresponding temperatures.

With hydrogen as a cooling agent we shall get to from 13° to 15° of the zero of absolute temperature, and its use will open up an entirely new field of scientific inquiry. Even so great a man as James Clerk Maxwell had doubts as to the possibility of ever liquefying hydrogen.* He says: "Similar phenomena occur in all the liquefiable gases. In other gases we are able to trace the existence of attractive force at ordinary pressures, though the compression has not yet been carried so far as to show any repulsive force. In hydrogen the repulsive force seems to prevail even at ordinary pressures. This gas has never been liquefied, and it is probable that it never will be liquefied, as the attractive force is so weak." In concluding his lectures on the non-metallic elements delivered at the Royal Institution in 1852, and published the following year, Faraday said †: "There is reason to believe we should derive much information as to the intimate nature of these non-metallic elements, if we could succeed in obtaining hydrogen and nitrogen in the liquid and solid form. Many gases have been liquefied: the carbonic acid gas has been solidified, but hydrogen and nitrogen have resisted all our efforts of the kind. Hydrogen in many of its relations acts as though it were a metal: could it be obtained in a liquid or a solid condition, the doubt might be settled. This great problem, however, has yet to be solved, nor should we look with hopelessness on this solution when we reflect with wonder—and as I do almost with fear and trembling—on the powers of investigating the hidden qualities of these elements—of questioning them, making them disclose their secrets and tell their tales—given by the Almighty to man."

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* See Scientific Papers, 2, 412.

† See Faraday's Lectures on the Non-Metallic Elements, pp. 292–3.

Faraday's expressed faith in the potentialities of experimental inquiry in 1852 has been justified forty-six years afterwards by the production of liquid hydrogen in the very laboratory in which all his epoch-making researches were executed. The "doubt" has now been settled; hydrogen does not possess in the liquid state the characteristics of a metal. No one can predict the properties of matter near the zero of temperature. Faraday liquefied chlorine in the year 1823. Sixty years afterwards Wroblewski and Olszewski produced liquid air, and now, after a fifteen years' interval, the last of the old permanent gases, hydrogen, appears as a static liquid. Considering that the step from the liquefaction of air to that of hydrogen is relatively as great in the thermodynamic sense as that from liquid chlorine to liquid air, the fact that the former result has been achieved in one-fourth the time needed to accomplish the latter proves the greatly accelerated pace of scientific progress in our time.

The efficient cultivation of this field of research depends on combination and assistance of an exceptional kind; but in the first instance money must be available, and the members of the Royal Institution deserve my especial gratitude for their handsome donations to the conduct of this research. Unfortunately its prosecution will demand a further large expenditure. It is my duty to acknowledge that at an early stage of the inquiry the Hon. Company of Goldsmiths helped low-temperature investigation by a generous donation to the Research Fund.

During the whole course of the low-temperature work, carried out at the Royal Institution, the invaluable aid of Mr. Robert Lennox has been at my disposal, and it is not too much to say that, but for his engineering skill, manipulative ability and loyal perseverance, the present successful issue might have been indefinitely delayed. My thanks are also due to Mr. J. W. Heath for valuable assistance in the conduct of the experiments.

[J. D.]

WEEKLY EVENING MEETING,

Friday, January 27, 1899.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

The Right Hon. Sir Mountstuart E. Grant Duff, G.C.S.I. F.R.S.

Epitaphs.

When we remember that nearly all churches and churchyards contain a great variety of epitaphs and that they were in use long before churches or churchyards existed, we may well feel some surprise that so extensive a department of literature has received such scant attention from competent critics. It is true that there are many collections of epitaphs, but the most uncritical spirit has almost always guided those who have collected them. Now and then a great writer has produced an essay on the subject. Samuel Johnson, for instance, contributed one to the 'Gentleman's Magazine,' which will be found in his collected works; but it is far indeed from being one of its author's more felicitous compositions, and is, sooth to say, a singularly poor piece of work, only redeemed from insignificance by the praise which it gives to the memorable epitaph of Zosimé, then less known, I presume, than it is now:—

> Zosimé, ne'er save in her flesh a slave,
> E'en for her flesh finds freedom in the grave.

Wordsworth, too, wrote a paper upon epitaphs in the 'Friend,' but it is a very unsatisfying performance. The philosophical and critical part of it, indeed, is exceedingly jejune, although when the author forgets that he is a philosopher, and remembers only that he is a poet, he rises very high. The following is surely a noble paragraph:—

"As in sailing upon the orb of this planet, a voyage towards the regions where the sun sets, conducts gradually to the quarter where we have been accustomed to behold it come forth at its rising; and in like manner a voyage towards the East, the birth-place in our imagination of the morning, leads finally to the quarter where the sun is last seen when he departs from our eyes; so the contemplative soul, travelling in the direction of mortality, advances to the country of everlasting life; and, in like manner, may she continue to explore those cheerful tracts, till she is brought back, for her advantage and benefit, to the land of transitory things—of sorrow and of tears."

I need not say that I am not going to attempt a dissertation upon epitaphs when two such eminent men have failed. All I shall attempt to do is to bring together as many quite first-rate epitaphs as time will permit, avoiding some of those which are best known, and connecting those I shall cite with each other as well as I can. If by that means I can give, to those who have honoured me by their presence, an agreeable hour, my highest ambition will be satisfied.

In most collections of epitaphs a great many pages are given to comic ones. Such things are quite harmless when they are merely written to pass from mouth to mouth and with no intention of engraving them on a tomb, but those persons who spend their time in painfully collecting and carefully publishing in books the rubbish which is often to be found in country churchyards do a serious disservice to such of their fellow-creatures as have the misfortune to read them. They should be condemned to employ a sort of Old Mortality Reversed to go through the land chipping off the stones the trash which they have copied, paying all the fines their agent incurs in the process.

Perhaps the most amusing of comic epitaphs is one which circulates as that of Lady O'Looney, and is commonly said to have been copied from a tomb in Pewsey churchyard. That, however, is not the case, for in a work on epitaphs by Mr. Ravenshaw, who dates from Pewsey Rectory, I find that it is a version, mutilated for conversational purposes, of a long epitaph from St. George's burying ground in London on a certain Mrs. Jane Molony. The original is, Heaven knows, sufficiently absurd, and nearly all the current version has been picked out of it, but it contains a great deal of additional matter chiefly of a genealogical character.

Lord Holland said to Mr. Charles Greville in 1830, that "there is hardly such a thing in the world as a good house or a good epitaph, and yet mankind have been employed in building the former and writing the latter since the beginning almost."

I propose to deal exclusively with those epitaphs which deserve to be covered by the word "hardly" in this judgment.

When I determined to address you on this subject, my first endeavour was to find out whether the great ancient civilisations of Assyria, Babylonia or Egypt had bequeathed anything to us in the shape of epitaphs. After applying to the best authority I have not been able to find that the two first mentioned have done so. From a paper, however, published under the title of 'Egyptian Stelæ principally of the Eighteenth Dynasty,' by Mr. Budge of the British Museum, and kindly lent to me by him, I gather that "the custom of the Ancient Egyptians of erecting sepulchral *stelæ* in honour of their deceased kings, nobles, persons of rank, relatives and friends, has proved a most valuable aid to the modern student of the Egyptian language, and has enabled him to learn much of the social life of the Egyptian which would otherwise have passed away in oblivion."

No doubt this is so, and the specimens which Mr. Budge gives are

very curious; but although a striking expression occurs here and there, much of their language is, to the ear of a modern, in the highest degree grotesque. Such phrases as "May his memorial abide in the seat of Eternity," or "May he be granted the breath of the North wind," seem appropriate enough on a funeral monument, but aspirations like those to be found in the ninth and tenth paragraphs of the first inscription quoted, "May I attain the field of peace, may one come with jugs of beer and cakes, the cakes of the Lords of Eternity," "May I receive many slices from the joint upon the table of the great God," are less attractive.

I do not remember that the Old Testament, filled though it is with passages which have been and will be used as epitaphs, contains anything that was intended as such. I have met, however, with one exceedingly fine Phœnician epitaph which makes me doubt whether there were none amongst the inhabitants of Southern Palestine. It is on a sarcophagus in the Louvre, brought from Sidon, a place which, if it was as beautiful in early days as it is now, might well have made poets of its rulers.

In the month of Bul, the fourteenth year of my reign, I, King Ashmanezer, King of the Sidonians, son of King Tabnith, King of the Sidonians, spake King Ashmanezer, King of the Sidonians, saying: "I have been stolen away before my time—a son of the flood of days. The whilom Great is dumb; the son of Gods is dead. And I rest in this grave, even in this tomb, in the place which I have built. My adjuration to all the Ruling Powers and all men: Let no one open this resting place, nor search for treasure, for there is no treasure with Us; and let him not bear away the couch of my Rest, and not trouble Us in this resting place by disturbing the couch of my slumbers. . . . For all men who should open the tomb of My rest, or any man who should carry away the couch of My rest, or anyone who trouble me on this couch: Unto them there shall be no rest with the departed; they shall not be buried in a grave, and there shall be to them neither son nor seed. . . . There shall be to them neither root below nor fruit above, nor honour among the living under the sun. . . .

To find many examples of anything really good done by the early world in this department, we must, as is so often the case, turn to Greece. There we shall find a rich harvest from which, however, the limit wisely set to your lectures will allow me only to glean a very few specimens. Most of these have been treasured up for the world by the admirable persons who compiled the various editions of the 'Anthologia,' a work to which this century has done more justice than its predecessor. Chesterfield's judgment of it is, next to his low standard in one branch of morality, the greatest blot on the fame of that wise man.

The briefest of selections from the epitaphs of which we have the good fortune to have excellent translations in English verse, is all I can attempt. First then may come the immortal distich on Leonidas and his three hundred:—

Go Stranger and to Lacedæmon tell
That here, obeying her commands we fell.

We may next take that upon Aster, ascribed (scholars, I believe, think rightly) to Plato :—

Thou wert the morning Star among the living
Ere thy fair light had fled;
Now having died, thou art as Hesperus giving
New splendour to the dead.

To Plato likewise is attributed the wonderfully touching epitaph on the Eretrians who were transported to Ecbatana and died there. I have never seen a metrical translation of this which succeeds in rendering the concentrated pathos of the original. Mr. Symonds' version runs :—

We who once left the Ægean's deep-voiced shore,
Lie 'neath Ecbatana's champaign, where we fell.
Farewell Eretria, thou famed land of yore,
And neighbour Athens, and loved sea, farewell.

There is nothing to be said against this save that the gifted writer has not succeeded in performing an impossibilty. "Loved sea farewell!" is of course perfectly literal, but the last three words of the original χαῖρε θάλασσα φίλη fall on the ear like a sigh, and those of the translation do not.

We may pass to the epitaph on Plato himself, of which, as of his lines on Aster, we have a translation by no less a personage than Shelley :—

Eagle! why soarest thou above that tomb?
To what sublime and star-y-paven home
Floatest thou?

I am the image of swift Plato's spirit,
Ascending heaven: Athens does inherit
His corse below.

The next I shall cite is by Callimachus, supremely translated by the late Mr. Cory :—

They told me, Heracleitus, they told me you were dead;
They brought me bitter news to hear and bitter tears to shed.
I wept, as I remembered, how often you and I
Had tired the sun with talking and sent him down the sky.

And now that thou art lying, my dear old Carian guest,
A handful of grey ashes, long, long ago at rest,
Still are thy pleasant voices, thy nightingales, awake,
For death, he taketh all away, but them he cannot take.

A very large number of the Greek epitaphs which have been preserved deal, as might be expected, with the innumerable accidents incident to a seafaring life. Here, for instance, is one :—

Ask not, Oh Sailor, what my name might be,
But may Heaven grant to you a kinder Sea.

The following is said to have been taken from the tomb of an Athenian at Meröe on the Upper Nile :—

> Fear not in death far from thy home to be,
> 'Tis one—all one—Athens or Meröe.
> Since from each country whatsoe'er its name
> The wind that blows to Hades is the same.

Although it is upon a city, and not upon an individual, I must not pass by the epitaph on Corinth so happily translated by Goldwin Smith :—

> Where Corinth, are thy glories now,
> Thy ancient wealth, thy castled brow,
> Thy solemn fanes, thy halls of state,
> Thy high-born dames, thy crowded gate?
> There's not a ruin left to tell
> Where Corinth stood, how Corinth fell.
> The Nereids of thy double sea
> Alone remain to wail for thee.

No one disapproves more strongly than I do of the monstrous loss of time involved in setting boys and young men, most of whom are absolutely destitute of the slightest poetical talent, to write Latin and Greek verses; but every now and then this atrocious custom leads to the production of something of value, and I have always thought that the Greek epitaph on the Admirable Crichton, written by the late Mr. George Butler, elder brother of the Master of Trinity, and published in the 'Anthologia Oxoniensis,' deserved a place amongst the best inscriptions of a similar kind by the writers of Ancient Greece.

I am addressing, no doubt, a good many people who know vastly more about Greek epigrams in general and Greek epitaphs in particular, than I can pretend to do. To those, however, who do not chance to have given attention to these subjects, and have a mind to do so, I should like to recommend an excellent chapter in the volume on the Greek Poets by the late Mr. Symonds, and the not less delightful book, by Mr. Mackail, published under the name of 'Select Epigrams from the Greek Anthology.' It is high time, however, to pass from what is, after all, only a section of my subject, and to turn from Greek to Latin.

Although the language of Rome was destined to be pre-eminently that of epitaphs, and to supply the wants of the speakers of other tongues, in that behalf, for many generations, the earlier Latin epitaphs had no alliance with any of the muses save that of history. Gradually they became a little more copious, and we find such expressions as: "Rogo ut discedens terram mihi dicas levem"—"I ask thee as thou departest to pray that the earth may lie lightly upon me." The four most remarkable early Roman epitaphs, in verse, are, I think, well known, but I am not aware that any of them was ever inscribed upon a monument. They are those of Nævius, Pacuvius, Ennius and Plautus. The first three are said to have

been written by the poets themselves, the fourth apparently not by the great comedian but by an admirer:—

> Mortalis immortalis flere si foret fas,
> Flerent divæ Camœnæ Naevium poetam.
> Itaque postquam est Orcino traditus thesauro
> Oblitei sunt Romæ loquier Latinâ linguâ.
>
> If it were fitting that immortals should weep for mortals,
> The Muses themselves would weep for Nævius.
> For since he has gone to the Treasure House of Orcus
> Men have forgotten at Rome to speak the Latin tongue.
>
> Adolescens, tamen etsi properas, hoc te saxum rogat,
> Utei ad se aspicias: deinde quod scriptu'st legas:
> Hic sunt poetæ Pacuvei Marcei sita
> Ossa, hoc volebam nescius ne esses, vale.
>
> Youth, albeit thou art in haste, this stone entreats thee
> To look upon it and to read the words with which it is inscribed:
> Here lie the bones of Marcus Pacuvius the poet,
> I wished thee to know this, and so farewell.

That of Ennius is finer, especially the two last lines:—

> Nemo me lacrumis decoret nec funera fletu
> Faxit, cur, volito vivu per ora virum.
>
> Let no one weep or raise funeral lamentations for me.
> Why? Because still alive I flit from mouth to mouth of men.

The fourth, that on Plautus, regrets that after his death Comedy mourns, the stage is deserted, while Laughter, Jest and Verse all weep together.

> Postquam morte datu'st Plautus, comœdia luget;
> Scena est deserta, dein Risus, Ludu', Jocusque!
> Et numeri innumeri simul omnes collacrumarunt.

Goldwin Smith mentions a suggestion that the famous elegy of Propertius upon Cornelia was intended to be inscribed upon her tomb. I should much doubt this, but if it had been it certainly would have been amongst the most remarkable epitaphs of the world. He has translated it very well in his 'Bay Leaves,' and there is another version even more beautiful in a small volume of poems by the late Sir Edmund Head. This last is indeed one of the best translations or paraphrases in English of a Latin poem to be found anywhere.

Morcelli cites two lines from another poem by Propertius which is, in effect, an epitaph and a very graceful one.

> Hic Tiburtinâ jacet aurea Cynthia terrâ;
> Accessit ripæ laus Aniene tuæ.
>
> Here in the soil of Tibur lies the golden Cynthia;
> Anio! a new honour has been added to thy banks.

The oldest Christian epitaphs in the Catacombs are of the greatest simplicity, bearing no trace of the definite dogmatic beliefs which were later imported into inscriptions of this kind. They are chiefly brief outpourings of natural affection or such expressions of non-

dogmatic devotion as: "In Pace," or "Vivas in Deo," or "Vivas in pace et pete pro nobis"; or in Greek, "Mayest thou live in the Lord, and Pray for us."

Dean Stanley, in his excellent 'Christian Institutions,' remarks—"In a well-known work of Strauss, entitled 'The Old and New Belief,' there is an elaborate attack on what the writer calls 'The Old Belief.' Of the various articles of that 'Old Belief' which he enumerates, hardly one appears conspicuously in the Catacombs. Of the special forms of belief which appear in the Catacombs, hardly one is mentioned in the catalogue of doctrines so vehemently assailed in that work."

We may be permitted then to feel ourselves, if we so please, in full communion with the Christians of at least the first two centuries—with the "Church in Cæcilia's House" as it is described in an exquisite chapter of Mr. Pater's book, 'Marius the Epicurean,' and nevertheless fully to admit that Strauss was a very great man. We may agree, without receding from our position, that he did a notable piece of work for the world, although that work was diametrically opposite in its tendency to the equally valuable work for England which began at Oxford, just about the time when he first appeared upon the scene.

I would almost venture to assert that more really fine epitaphs have been produced in Latin since it became the language only of the Church and of the learned than was the case while it was still the language of the civilised Western World.

Assuredly in modern times Latin has been constantly used as the language of epitaphs with the most brilliant success in every part of Europe, and in commemoration of the most diverse characters. I may cite first the epitaph of St. Benedict and his sister Santa Scholastica at Monte Cassino.

> Benedictum et Scholasticam
> Uno in terris partu editos,
> Unâ in Deum pietate coelo redditos
> Unus hic excipit tumulus
> Mortalis depositi pro immortalitate custos!
>
> Benedict and Scholastica
> Born into this world at the same birth
> Restored to Heaven by the same piety towards God
> This same tomb receives
> The Guardian for immortality of a mortal deposit.

Then we may take one from Southern Spain which has a certain family resemblance to the last, though in honour of a very different personage—Gonzalez of Cordova, the Great Captain.

> Gonzali Fernandez de Cordova, qui propriâ virtute Magni Ducis nomen proprium sibi fecit, ossa perpetuæ tandem luci restituenda huic interea loculo credita sunt, gloriâ minime consepultâ.
>
> The bones of Fernandez of Cordova, who by his valour won for himself the distinctive name of the Great Captain, bones to be one day restored to perpetual light, are in the meantime entrusted to this little niche—his glory being by no means buried with them.

Excellent is the epitaph on Trivulzio, General of Francis I., and, for that matter, of many other lords :—

> Johannes Trivulzius, qui nunquam quievit, hic quiescit. Tace.
>
> Johannes Trivulzius, who never rested, rests here. Be silent.

Hardly less good is the epitaph on Mercy :—

> Sta viator heroem calcas.
>
> Stop, traveller, thou treadest on a hero.

The modern Florentines missed their mark, by altogether overshooting it, when they put on the tomb of Machiavelli,

> Tanto nomini nullum par elogium.
>
> No eulogium is sufficient for so great a name.

But the epitaph, if better deserved, would have been a grand one.

Admirable is the epitaph on Sheffield, Duke of the County of Buckingham, written by himself, and to be found in Westminster Abbey. The stone originally bore two additional words " Christum adveneror," but the foolish bigotry of Atterbury suppressed them.

> Dubius sed non improbus vixi;
> Incertus morior, non perturbatus.
> Humanum est nescire et errare.
> Deo confido
> Omnipotenti benevolentissimo:
> Ens entium, miserere mei.
>
> I lived a doubtful but not an evil life;
> I die uncertain, but not dismayed.
> It is the lot of man to be ignorant and to err.
> I trust in God the Omnipotent, the most Benevolent.
> Being of Beings, have mercy upon me.

The magnificent epitaph on Colin Maclaurin, Professor of Mathematics in Edinburgh, can be read at length in Boswell's ' Johnson.' It was placed on the tomb by his son, not, as he says, to provide for his father's fame, for it wants no such assistance, but in order that in this unhappy field where fear and sorrow reign, mortals should not be left absolutely without consolation, for turn over his writings and be sure that a mind capable of such things must outlast the perishable body :—

> Non ut nomini paterno consulat,
> Nam tali auxilio nil eget;
> Sed ut in hoc infelici campo,
> Ubi luctus regnant et pavor,
> Mortalibus prorsus non absit solatium.
> Hujus enim scripta evolve,
> Mentemque tantarum rerum capacem,
> Corpori caduco superstitem crede.

The same modesty led Buffon's son to describe himself on his monument to his father as the humble column of a lofty tower: "Excelsæ turris humilis columna."

So graceful a turn of phrase ought by itself to have prevented his having been described as "le plus mauvais chapitre de l'histoire naturelle de son père!"

One of the most delightful of all epitaphs, to my thinking, is in a place very familiar to me, the grey City of Aberdeen, but I learnt it first from Pennant, who, in his tour last century, was fortunate enough to observe it.

Si fides, Si humanitas
Multoque gratus lepore candor
Si suorum amor amicorum caritas
Omniumque benevolentia
Spiritum reducere possent
Non hic situs esset
Johannes Burnet a Elrick.

If fidelity, if humanity and candour, made pleasant by an abundance of wit, if the love of his kindred, the affectionate regard of his friends, and the kindly feeling of all could bring back the breath—John Burnet of Elrick would not lie here.

There are two good epitaphs on dogs by Lord Grenville in the 'Anthologia Oxoniensis'—one of them extremely beautiful. Its last two lines are:—

Jamque vale! Elysii subeo loca læta piorum
Quæ dat Persephone manibus esse canum.

And now! Farewell. I depart to those happy seats of the good which Persephone reserves for the *manes* of dogs.

I may refer those who would like to see a reasoned defence of the dog's view of his future, to a very remarkable passage in a most interesting book, the late Mr. Greg's 'Enigmas of Life.'

One of the most happily conceived of epitaphs is the line of Ovid inscribed over the gate of the cemetery at Richmond, where so many of those who fell on the southern side in the American Civil War are buried:—

Qui bene pro patriâ cum patriâque jacent.

Those who lie here in honour having died for and with their country.

They were more fortunate than the noble of the Eastern Empire, who died shortly before the capture of Constantinople by the Turks, and whose epitaph is thus translated by Bland. I do not know the original.

Oh thou who sleep'st in brazen slumber, tell,
—(Thy high descent and noble name full well
I know—Byzantium claims thy birth—) but say!
"A death, unworthy of my high estate—
This thought is keener than the stroke of fate,
I bled not in the ranks of those who fell
For glorious, falling Greece—no more—Farewell!"

An epitaph was repeated to me once by the late Mr. Charles Pearson, the author of 'National Life and Character,' as having been placed or proposed to be placed on the tomb of one who was like himself a Fellow of Oriel, Mr. Charles Marriott, so well known in connection with the earlier part of the Oxford Movement. It seemed to me very striking in spite of its peculiar Latinity:—

> Exutus morte
> Hic licet in occiduo cinere
> Aspicit eum
> Cujus nomen est Oriens.
>
> Freed from death
> Tho' in ashes that vanish away,
> He looketh upon him
> Whose name is "the Rising."

Very beautiful and very characteristic of the man at his best, is the epitaph which Newman composed for himself:—

> Ex umbris et imaginibus in veritatem.
>
> Out of shadows and images into the Truth.

Dr. Johnson, as is well known, had the most rooted objection to English epitaphs, and insisted, in spite of the respectful remonstrances of a most distinguished group of friends, in writing the epitaph upon Goldsmith in Latin. His obstinacy in a bad cause had, however, the incidental effect of giving us the happy phrase which many suppose to have come down from classical antiquity:—

> Nihil tetigit quod non ornavit.
>
> He touched nothing which he did not adorn.

To another writer of the last century, to Shenstone, we owe the equally famous words which formed part of the epitaph of a young lady:—

> O quanto minus est
> Cum aliis versari
> Quam tui meminisse.
>
> O how much less it is to live with others than to remember thee.

Again, however, the clock warns me to pass to another branch of my subject, but before doing so I should like to say that I wish some one who had eyes, leisure and enthusiasm would go through Mommsen's inscriptions and the far less gigantic but still huge work of Morcelli, and Murray's handbooks (which have swept into their pages so much that is interesting and that cannot easily be found elsewhere), with a view to giving us a small volume containing only the most beautiful Latin epitaphs.

We may turn now to our own language. The last Lord de Tabley, one of the most accomplished of men, used to remark on the extraordinary difficulty of writing an epitaph in English about a commonplace life, "like those touching ones of commonplace life which, as he said, draw one's very heart out in Latin in the first few centuries."

There are many volumes containing hundreds of epitaphs which seem to have been collected chiefly to prove the correctness of this remark.

Yet English, when the life you have to deal with is not commonplace, is far from being a bad language for epitaphs either in prose or verse. I am indeed not at all sure that there is any poetical epitaph in any language superior to that wonderful "amende honorable" of Macaulay's, the Epitaph on a Jacobite :—

To my true king I offered, free from stain,
Courage and faith—vain faith and courage vain.
For him I threw lands, honours, wealth away,
And one dear hope that was more prized than they;
For him I languished in a foreign clime,
Grey-haired with sorrow in my manhood's prime;
Heard in Lavernia Scargill's whispering trees,
And pined by Arno for my lovelier Tees;
Beheld each night my home, in fevered sleep,
Each morning started from that dream to weep,
Till God, who saw me tried too sorely, gave
The resting-place I asked—an early grave.
O thou whom chance leads to this nameless stone,
From that proud country which was once mine own,
By those white cliffs I never more must see,
By that dear language which I spake like thee:
Forget all feuds, and shed one English tear
O'er English dust—a broken heart lies here.

Some of the best of English epitaphs are so well known, as not to be worth quoting : such, for instance, as Shakespeare's, said to have been written by himself, and much in the spirit of that of King Ashmanezer, already mentioned, which assuredly he never saw ; or Milton's upon his great predecessor ; or Pope's upon Sir Isaac Newton ; or, far superior to all of them put together, the lines usually attributed to Ben Jonson, but probably written by William Browne, lines which it is impossible to avoid repeating whenever one thinks of them :—

Underneath this marble hearse
Lies the subject of all verse,
Sydney's sister, Pembroke's mother.
Death ! ere thou hast slain another
Fair and learned and good as she
Time shall throw a dart at thee.

One of the finest of English epitaphs is undoubtedly the famous one at Melrose, which is rarely quite correctly quoted, but of which the correct version runs as follows. Correct, I say, for I copied it from the stone :—

The Earth goes on the Earth,
  Glist'ning like Gold,
The Earth goes to the Earth
  Sooner than it wold;
The Earth builds on the Earth
  Castles and towers;
The Earth says to the Earth
  All shall be ours.

There is an exceedingly beautiful Greek version of this in the 'Anthologia Oxoniensis' from the pen of Mr. James Riddell, the same who is described in Principal Shairp's far too-litttle-known poem on "The Balliol Scholars from 1840 to 1843," which contains such admirable sketches of Clough, Matthew Arnold and Coleridge.

This noble epitaph, or variants of it, are found in several places, but, so far as I know, its original author has not been discovered. He was surely no mean poet.

Herrick's epitaph is characteristically graceful:—

Weep for the dead, for they have lost this light,
And weep for me, lost in an endless night,
Or mourn, or make a marble verse for me
Who writ for many, Benedicite.

On Shelley's grave in the new Protestant cemetery near the pyramid of Caius Cestius at Rome, Trelawney put the lines from Shakespeare:—

Nothing of him that doth fade
But doth suffer a sea-change
Into something rich and strange.

On his monument at Christchurch, in Hampshire, they have very appropriately put his own lines:—

He hath outsoar'd the shadow of our night;
  Envy and calumny, and hate and pain,
And that unrest which men miscall delight,
  Can touch him not, and torture not again,
  From the contagion of the world's slow stain
He is secure, and now can never mourn
  A heart grown cold, a head grown grey in vain;
Nor, when the spirit's self has ceased to burn
With sparkless ashes load an unlamented urn.

On the monument of the Wesleys in Westminster Abbey, Dean Stanley placed not less appropriately the words "God buries his workmen but carries on his work."

Excellent too is a sort of general epitaph which he hung up in the Great Abbey:—

Here's an acre sown indeed,
With the richest royallest seed,
Which the earth did e'er suck in,
Since the first man died for sin.

Very good is the epitaph by Lord Houghton, in the same place, upon Charles Buller:—

Here, amidst the memorials of maturer greatness
This tribute of private affection and public honour
Records the talents, virtues, and early death of
The Right Honourable Charles Buller;
Who, as an independent Member of Parliament
And in the discharge of important offices of State,

United the deepest human sympathies,
With wide and philosophic views of government and mankind,
And pursued the noblest political and social objects,
Above party spirit and without an enemy.
His character was distinguished by sincerity and resolution,
His mind by vivacity and clearness of comprehension;
While the vigour of expression and singular wit,
That made him eminent in debate and delightful in society,
Were tempered by a most gentle and generous disposition,
Earnest in friendship and benevolent to all.

The British Colonies will not forget the statesman
Who so well appreciated their desires and their destinies,
And his country, recalling what he was, deplores
The vanished hope of all he might have become.
He was born August 1806. He died November 29, 1848.

I think that this is far the best long epitaph in the Abbey. Dean Stanley said to me, with much truth, that there were very few good ones there, either long or short.

Two of the best English epitaphs which I have come across, written in our times, are from the hand of the Archbishop of Armagh. The first, which is in Derry Cathedral, is good throughout, and contains two specially good lines:—

'Twas but one step for those victorious feet
From their days' walk into the golden Street.

Excellent, too, is the other on a lady of the Nathalie Narischkin type:—

Proudly as men heroic ashes claim
We asked to have thy fever-stricken frame
And lay it in our grass beside our foam
Till Christ the Healer call his Healers home.

An epitaph on Lord Hugh Seymour and his wife, quoted by Pettigrew (whose collection of English epitaphs, though containing many hundreds of no value, is much the best I have seen), is little known and worth quoting. He died on the Jamaica Station; she in England, but they were buried together:—

Parted once—the fair and brave,
Meet again—but in their grave.
She, was Nature's brightest flower,
Struck before its drooping hour:—
He, was Britain's Naval pride;
Young—but old in fame, he died.
Love, but with a Patriot's tear
Mourns, and consecrates them here.

On the same page, and by the same author, is to be found a long but rather feeble epitaph on Lord Cornwallis. It would have been better to have placed on his monument the very striking paragraph by Sir James Mackintosh.

"He expired at Gazeepore, in the province of Benares, on the 5th

of October, 1805, supported by the remembrance of his virtue, and by the sentiments of piety which had actuated his whole life. His remains are interred on the spot where he died, on the banks of that famous river, which washes no country not either blessed by his Government, or visited by his renown; and in the heart of that province so long the chosen seat of religion and learning in India, which under the influence of his beneficial system, and under the administration of good men whom he had chosen, had risen from a state of decline and confusion to one of prosperity probably unrivalled in the happiest times of its ancient princes. His body is buried in peace, and his name liveth for evermore."

When I was passing through Bombay in the autumn of 1874 an epitaph was repeated to me which I thought extremely good. It was on Major D'Oyley, an artillery officer who died in the Mutiny, and ran as follows:—

> Here lies the body of Major D'Oyley of The Bengal Artillery
> Whose last wish: "When I am dead, put
> A stone over me and write upon it that
> I died fighting my guns," is thus fulfilled.

Later the exact words were sent to me, but they were not quite so few nor so good.

Over Campbell in Westminster Abbey they put—they could not have done otherwise, his own fine lines:—

> This spirit shall return to Him
> Who gave its heavenly spark;
> Yet, think not, sun, it shall be dim
> When thou thyself art dark!
> No! it shall live again and shine
> In bliss unknown to beams of thine,
> By Him recall'd to breath,
> Who captive led captivity,
> Who robb'd the grave of victory
> And took the sting from death.

Not less happily inspired were those who wrote on the tomb of Mrs. Hemans in St. Ann's, Dublin.

> Calm in the bosom of thy God,
> Fair spirit rest thee now;
> E'en while with us thy footsteps trod,
> His seal was on thy brow.
> Dust to its narrow house beneath,
> Soul to its place on high!
> They that have seen thy look in death,
> No more may fear to die.

They might have added the lines in which Wordsworth described the poetess who, somewhat overrated in her own time, has been quite absurdly underrated in ours, but who will probably have a return of fame when people get tired of the clotted nonsense or the harmonious

words without any thought at all, which at present divide into two equally deluded schools of poetry a large section of our contemporaries:—

> Mourn rather for that holy Spirit
> Sweet as the Spring, as Ocean deep;
> For Her who, ere her Summer faded,
> Has sunk into a breathless sleep.

Mrs. Hemans was only forty when she died.

Among English epitaphs expressing nothing but tender domestic feeling, one of the best I have met with is in St. Giles's, Cripplegate, in memory of a young lady belonging to the Lucy family. I will not quote it because by not doing so, I may conceivably lead some one in my audience to visit that most interesting church in which Cromwell was married and Milton buried.

There is a very pretty epitaph, a little too long to quote, in Shepperton churchyard, which has been published by Dr. Garnett, on a child of Mr. Peacock, of Crochet Castle celebrity—a man of many and strangely diverse gifts, novelist and naval constructor, examiner of correspondence at the India Office and operatic critic, poet in the most approved manner of the later eighteenth century, and in the most approved manner of the earlier nineteenth century—equally successful in such compositions as his very beautiful 'Love and Age,' and in describing a whitebait dinner at Blackwall in Homeric Greek. I remember his once presenting me with such a curious *tour-de-force.*

Very striking, and in the highest degree characteristic, is the epitaph to be read at Mentone on the grave of Mr. Green the historian:—

> He died learning.

It carries one's thoughts to the admirable *motto* cited, amongst many not less good, by a man who, whatever we may think of his political activity, certainly lived up to it—the Prussian General Radowitz:—

> Disce ut semper victurus
> Vive ut cras moriturus.
>
> Learn as if you were to live for ever,
> Live as if you were to die to-morrow.

One of the best epitaphs of the perfectly simple kind which I ever chanced to light upon is, or was a generation ago, in a churchyard adjoining the ruined church of Gamrie in the extreme north-east of Scotland. It was on one of those large slabs which were once much affected by the wealthier peasantry in that district and consisted of simply two lines. At the top of the stone were the words:—

> The night is far spent,

And at the bottom:—

> The day is at hand.

Highly characteristic of a quite different frame of mind from that which inscribed the stone I have mentioned, was another, placed over some rough-handed, but faithful vassal, which was repeated to me many years ago :—

Ill to his freen
Waur to his foe
True to his Macker *
In weal or in woe.

To the Jews we owe at least one epitaph, the "In Pace" of the Catacombs already alluded to, which was obviously suggested by the word "Shallum" or "Peace," which seems to have been frequently placed on the graves of the early Jewish settlers in Rome. I do not know whether there are many remarkable modern ones. I have only chanced to meet with one. This was inscribed in memory of a man whom many I am addressing must have known, Mr. Deutsch of the British Museum. It seems to me extremely fine :—

"Here is entombed the well-beloved whose heart was burning with good things, and whose pen was the pen of a ready writer. Menahem, Son of Abraham Deutsch, whom the Lord preserve! He was born at Neisse on the 1st Masheshwau 5590 A.M., and departed from this world in Alexandria on Monday the 9th Iyar in the year 'Arise, shine, for thy light is come.' May his soul be bound up in the bond of life."

Punning epitaphs, so common in English churchyards, are usually beneath contempt, but one is very nearly good—that proposed by Douglas Jerrold for the publisher Charles Knight—"Good Night."

An epitaph on a dog by the first Lord Lytton, may be remembered with Lord Grenville's, and with a very beautiful Greek one quoted in Mr. Mackail's book already mentioned :—

Alas, poor Beau,
Died February 28th, 1852.
It is but to a dog
That this stone is inscribed
Yet what now remains
In the House of thy Fathers,
Oh! solitary Master,
Which will sigh for thy departure,
Or rejoice at thy return?

Some other rather striking English epitaphs, hardly, however, striking enough to quote, may be read in Pettigrew, such as that on the great musician Purcell in Westminster Abbey, or one on Atterbury by Pope in the form of a conversation between the Bishop and his daughter, who died suddenly in the arms of her father, whom she had gone to visit in his exile.

I might add those on Prior and Gay, which are, however, familiar to every one.

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* i.e. his feudal lord.

I have only time to cite two or three striking French epitaphs. One of these reached me from Rhodes and belongs to the twelfth century :—

> Ci-gît très-haut et très-puissant Seigneur
> Baudoin de Flandre, Comte de Courtenay.
> J'ai aimé, j'ai pêché, j'ai souffert.
> Ayez pitié de moi, ô mon Dieu.

"Does n't it," said the gifted friend who found and who sent it to me, "resume all the anguish of mankind ?"

The following was written for himself by the first husband of Madame de Maintenon :—

> Passants, ne faites pas de bruit,
> De crainte que je ne m'éveille
> Car voilà la première nuit
> Que le pauvre Scarron sommeille.

Another of which I am fond is :—

> Seule à mon Aurore
> Seule à mon couchant
> Je suis seule encore ici.

It may be remembered with the enigmatic 'Miserrimus' of Worcester Cathedral.

Every one knows the epitaph which Piron, disgusted at his exclusion from the Académie Française, wrote for his own monument :—

> Ci-gît Piron qui ne fût rien
> Pas même Académicien.

But not so many have met with the incomparably superior epitaph which he suggested for the Marshal de Belleisle when that commander, taking a far too favourable view of his own merits, desired to be buried close to Turenne :—

> Ci-gît le glorieux à côté de la gloire !

I must spare five minutes for one or two German epitaphs. I remember having been particularly struck with one in the military Friedhof at Berlin. Over the entrance of a tomb, which when I saw it forty-five years ago looked appropriately forlorn, were the simple words :—

> Here is extinguished the old line of the House of Arnim Frederwalde.
>
> Hier erlischt die alte Linie des Hauses Arnim Frederwalde.

On the tomb of the great Alexander von Humboldt, they have placed an inscription to the effect that as he had learnt and comprehended all that is to be seen in our upper air he had descended also into the night to continue his researches :—

> Da er alles umfasst und erkannt was in Licht sich bewegt hier,
> Stieg er nun auch in die Nacht weiter zu forschen hinab.

Very different in character but extremely beautiful in its simplicity is one which was found by Mr. Hughes, an English clergyman and yachtsman, in the Aland Isles and reproduced in a book which he published under the title of 'The Log of the Pet.' The epitaph proper consisted of seven words only :—

Gott sei dir gnädig, O meine Wonne.

God be gracious to thee, oh my delight!

but under it were some verses which if they are as good in the original as they are in Mr. Hughes' translation, must be well worth recovering :—

Bright, bright was the soft and tender light
Of her eye,
And her smile's vanescent play
Like some truant sunbeam's ray,
Flitting by.

Clear, clear and passing sweet to hear
Was the sound
When her laugh's light melody
With quiet sparkling glee,
Rang around.

Fleet, fleet and oh! too deadly sweet
Sped the hour,
When those locks I loved to twine,
Flowed interlaced with mine
In her bower.

Fold, fold her tenderly around
Thou tomb!
Cold, cold lies the dank and sodden ground
In the gloom.

Roll, roll thy deep and solemn swell
Thou wave!
Toll, toll thy sad and endless knell
O'er her grave.

Admirably good was the epitaph by Ferdinand Gregorovius upon a German historian who died in Rome :—

Hier ruht der Geschichtschreiber,
Im Staube der Geschichte.

Here rests the Historian amid the dust of History.

Longfellow's charming 'Hyperion' has introduced to innumerable English and Americans the striking epitaph at St. Gilgen :—

Look not mournfully into the Past; it comes not back again. Wisely improve the present. It is thine. Go forth to meet the shadowy future. Without fear and with a manly heart.

Blicke nicht traurend auf die Vergangenheit,
Sie kommt nicht wieder: nütze weise die Gegenwart;
Sie ist dein: der luftigen Zukunft
Geh' ohne Furcht mit männlicher Sinne entgegen.

It would be hard to beat an epitaph in the great cemetery at Delhi, belonging to a religion which we do not generally associate with the gentler virtues:—

> Let no rich marble cover my grave
> This grass is sufficient covering
> For the tomb of the poor in spirit
> The humble, the transitory Jehanara
> The disciple of the holy men of Cheest
> The daughter of the Emperor Shah Jehan.

Many of my hearers will remember that the Emperor Shah Jehan was the builder of the Taj, beyond all comparison the most beautiful monument ever raised by the hand of the architect in memory of the departed. The thought of it takes me to Boury in Normandy, where most of those lie interred whose lives formed the subject of the 'Récit d'une Sœur,' the only literary monument to those who have passed away which quite deserves to rank with the marvellous creation of the Mogul. The epitaphs on those graves are not particularly striking, mostly texts from the Vulgate. Over all of them stands up the great marble cross, erected by Princess Lapoukyn, who, strange to say, survived her daughter by about a quarter of a century. It bears the inscription:—

> Jenseits ist meine Hoffnung.

I do not know what epitaph they have put in Paris over the grave of Ernest Renan, but they certainly could not have put a more appropriate one than that which he suggested for himself in the noble passage in which he expressed the wish that he could be buried in the cloisters of the Cathedral of Tréguier:—

> Veritatem dilexi.

I wonder whether before the year 2000 the Great Church will have come to the conclusion that he was not so far wrong when he said, "that his criticism had done more to support religion than all the Apologists." If such ideas are dreams, they are at least agreeable ones.

I see, however, that the sands of my hour are nearly run out, and I will conclude with two epitaphs, the one concentrating the deepest religious feeling, the other expressing the most legitimate pride in unequalled earthly achievement.

Chiabrera, after publishing many volumes of poems, summed up the experience of his long life, for he lived I think to over eighty, in his epitaph, still to be read in S. Giacomo at Savona. This was the epitaph which so much struck Frederick Faber, who saw in it, I apprehend, a prophecy of his own later years, for when he determined to devote himself to the Church, Wordsworth wrote to him: "I cannot say that you are wrong, but England loses a poet." It runs as follows:—

Amico, io vivendo cercava conforto
Nel Monte Parnaso;
Tu meglio consigliato, cercalo
Nel Calvario.

Friend, I when living sought for comfort
On Mount Parnassus;
Do you, better counselled, seek for it
On Calvary.

The other is in Spanish, the grand words on the tomb of the son of Columbus in the Cathedral of Seville:—

A Castilla y a Leon
Mundo nuevo dió Colon!

To Castile and to Leon
Columbus gave a new world!

WEEKLY EVENING MEETING,

Friday, February 3, 1899.

Sir Henry Thompson, Bart., F.R.C.S. F.R.A.S., Vice-President, in the Chair.

Victor Horsley, Esq., M.B. B.S. F.R.S. F.R.C.S. *M.R.I.*

Roman Defences of South-East Britain.

It is stated, on the authority of Dickens, that at Dr. Blimber's academy for young gentlemen, which was an establishment upon the south coast, where, as Mrs. Pipchin said, "there was nothing but learning going on from morning to night," the Doctor, at the end of dinner, "Having taken a glass of port and hemmed twice or thrice, said: 'It is remarkable, Mr. Feeder, that the Romans—' At the mention of this terrible people, their implacable enemies, every young gentleman fastened his gaze upon the Doctor with an assumption of the deepest interest." It is of these terrible people that I desire to speak to you to-night, and especially with reference to their defences—the Roman camps on the south-east coast of Britain—against the continual piratical attacks of *their* implacable enemies, the Scandinavian and Germanic tribes living on the shores of the North Sea. The number of these sea-coast camps is considerable, and possesses not only an archæological interest, but is a practical example of general political science; and owing to their being naval headquarters, they are actually illustrative of the present position of Continental politics so far as they affect this country. For to use what is perhaps now a hackneyed phrase, the interest of these camps lies in the tenure of sea power and the command of the English Channel, as I hope to show later in discussing the historical aspect of the subject.

As regards the occupation of Britain by the Romans, it is now of course well understood that Cæsar's invasions had no lasting effects—that in fact his two expeditions were little more than reconnaissances in force. The permanent occupation of Britain was only effected by Agricola, the able general of the Emperor Claudius, and his campaign was fortunately recorded for us by his son-in-law Tacitus. There is nothing to show that at that time the Scandinavian and North German tribes who ultimately invaded England were causing any appreciable trouble to the Romans, who had command of the sea, and who had conquered the Continental coast as far as Friesland.

During the campaigns of Hadrian and Severus, walled towns had been constructed all over the country; but about 230 A.D. it became necessary to re-organise the defence of the south-eastern coast against the aggression of the Baltic tribes, and this the authorities proceeded to effect by enlarging and reconstructing the walled defences of the

camps and towns of the south-eastern third of England. At this date (about 230 A.D.) our present subject therefore practically commences, and only finishes with the exodus of the Roman garrison in the year 404 A.D., in the time of the Emperor Honorius.

We shall see how during this period of about 200 years, which corresponds to the second half of the Roman occupation of Great Britain, the fate of the kingdom entirely depended upon him who held the command of the sea.

There is no doubt from evidence which I have not time to lay before you, but which is quite conclusive in several of the instances, that each of the great walled camps that I am about to describe to you had as a precursor a Roman station of smaller dimensions. This is an entirely historical fact, and it is also reasonable that those towns on the coast which afforded the best harbours for the navigation of that period, of necessity became the chief channels of trade, and being consequently the chief centres for the mercantile population, called for an expansion of their boundaries and re-fortification.

The outlines of the plan of the existing camps is worth noting, being for the most part square, but the conformation was occasionally altered to suit the ground. This is well shown at Lympne and Pevensey, and is indicated in the very early drawings which accompany the well-known texts of the Roman land surveyor Hyginus, which I here reproduce.

Further, the masonry points to the present camps having been built in the third century, and it is of course certain that none of the original stationary camps (*castra stativa*) were built before the end of the first century, because there was no occupation of the country worthy of the name until the first campaign of the Emperor Claudius.

The earliest thorough establishment of Roman walled fortifications in Britain, like so much of the organisation of the Roman Empire, was accomplished by the Emperor Hadrian about the year 120 A.D. It is, by the way, universally agreed that it is to Hadrian we are indebted for much of the construction of the great Northumbrian wall.

The fortification of the sites of our south-eastern camps at this time was evidently effected by the sailors or marines of the British squadron of the Roman Navy, for the tiles of the walls at Dover, of the original walls at Lympne and at Boulogne and Étaples are marked with the stamp C L B R, which stands for either Classis Britannica or Classiarii Britannici: the former of course indicating the Fleet, the latter the men who manned it, but the first is doubtless the right reading, and harmonises with the military title in which the inscription is always of the legion itself, and not of the men composing it.

This question of the foundation of the camps has a special interest to me, because, like Mr. Roach Smith, I found in the area of the castrum, at Lympne, yellow tiles marked C L B R, but the tiles in the wall are red, or even darkly burnt, and are not stamped at all. Further, in the foundation of the chief gate an altar, erected

by an Admiral of the name of Aufidius Pantera, was found built in, and, as can also be seen in the examination of the original at the British Museum, it is marked with barnacles, having been clearly at one time under the sea, the suggestion being that the original camp (? Hadrian's) was overwhelmed by an incursion of the sea over Romney level.

All this points to the fact, that the original station—at any rate, as far as Lemanus was concerned—was destroyed early in the Roman occupation of Kent, and that the present existing castrum was built later, when it became necessary, in the third century, to provide a series of forts to protect the coast from the Saxons. During the course of some excavations I carried out in 1893 at the Portus Lemanus, I obtained new confirmation of this fact and of the relatively late foundation of the present castrum. This consisted in the evidence of the coins discovered. I found in the concrete-boulder foundation of the south wall of Lympne a coin of Maximinus, who flourished 237 A.D. This was the earliest coin I discovered, and the only one actually in intimate relation with the foundation. Situated, as it were, in chronological order, I found at the foot of the wall, on the inner side, a Gaulish coin of Tetricus the elder, of a date about 260, and finally in the black soil of the camp, i.e. in the most recent and superficial layers, numerous coins of the Constantine family were brought to light. Thus we have a distinct series of dates which harmonise entirely with the previous conclusion.

The state of things at the opening of our story was as follows:—Walled towns or camps were established by the Roman Imperial Government holding the sea at the following places along the coast, beginning with the harbour creeks of Southampton, and following the line of Sussex, Kent, Essex, Suffolk and Norfolk, round to the Wash.

Of these originally, Richborough was the chief, but on the French coast Gessoriacum, which, just at the period under discussion, was rechristened Bononia, and is now called Boulogne, was, together with the Sambrican port (probably the modern Étaples), the headquarters of the fleet, according to M. Vaillant, our guide in the localisation of the squadrons of the Roman fleet. The following is a *résumé* of the fortifications:—

Clausentum	Bittern-Southampton.
Portus Magnus . . .	Portchester.
?	Rowlands Castle-Stanstead.
Regnum	Chichester.
Portus Adurni	?
Anderida	Pevensey.
?	Newenden.
Portus Lemanus (Portus Novus)	Lympne.
Dubris	Dover
Rutupis	Richborough.
Regulbium	Reculver.
Othona	(Ithan) Chester.
Camulodunum	Colchester.
Garrianonum	Burgh Castle (Yarmouth).
Branadunum	Brancaster.

We must now consider the fleet which occupied these fortified harbours. The British squadron was probably established by the Emperor Claudius, and was mentioned definitely in the year 70 expedition. We know that it formed part of Agricola's expedition, and at that time made a circular tour of the British Isles, in the year 83. It is not mentioned again until towards the end of the third century. The British fleet, however, does not actually appear prominently in history until it was re-organised by Caurausius, towards the end of the third century.

Squadrons of the Roman Navy and their Stations:—

Classis	Aegentensium.	
„	Anderetianorum.	
„	Britannica	Boulogne and South-East Coast of Britain.
„	Samarica	Mouths of Somme.
„	Germanica	Friesland Coast.
„	Mœsica	Danubian Mouths.
„	Pannonica	North Adriatic.

When the Roman document, the 'notitia dignitatorum' was compiled, which is the Whitaker of the Roman Empire towards the end of the fourth century, it shows that during that century a further re-organisation of the fleet had occurred, and that it was divided into squadrons which, for the most part, consisted of what we should now call coast-defence vessels, and these were stationed in the great estuaries evidently to stop the special methods of attack adopted by the Vikings and the Baltic tribes, who penetrated the country, especially the North of France, by ascending the rivers.

The fleet was officered just as ours is with admirals, or, as they were called, prefects, and captains of the triremic battleships, who were called trierarchs. Of these officers, the Museum at Boulogne contains several epigraphs. The admirals seem to have been of much the same status as the admirals of our seventeenth century Commonwealth, for they were military as well as naval officers, and the whole force was under the command of an officer called the Comes or Count of the Saxon shore, whose head-quarters were at Boulogne or Bononia.

Each of the walled castra stands at the head of a harbour creek. They are most of them of a quadrilateral figure, two being an exception, viz. Anderida and Lemanus, in which instances the outline of the camp was traced so as to accommodate itself to the contour of the ground. Their foundation and general details of structure are remarkably alike in each case, and these we will now consider.

Structure.—These walled towns were almost all built close to the water, and in one case at least (Porchester) the sea washes the wall. The foundations were dug deeply, and were composed of solid concrete made of flints and cement, upon which, at the level of the ground, was laid the first set of masonry, consisting, as a rule, of a course of large, rough, flattened stones. On these were erected concrete walls, faced

with square stones, and containing bonding courses of tiles. In the best built camps, e.g. Richborough, these bonding courses occurred at frequent intervals—e.g. in a wall 30 feet high there would be four or five, in the others there would be fewer. The tops of the walls all terminated in the same way, viz. by a flat platform and a crenellated parapet.

The facing stones are smaller as we ascend the wall, and the parapets are constructed of quite small stones. Roman parapets are, of course, rare, because of the subsequent mediæval occupation of these castra, nor do the remarkable parapets at Pompeii, each with its little traverse, help to make us better acquainted with their aspect, because they are more of a modified Greek pattern; but on Trajan's column, in which, of course, a style of architecture 100 years earlier is depicted, the battlements of a walled camp under construction are represented as being square, and somewhat mediæval in appearance. I would suggest that the crenellated intervals on the parapet at Portchester are the structural bases of the Roman battlement. The walls were supported at intervals by towers, which are in many respects interesting. Thus in the majority of the cases of the camps now under consideration the towers were solid—at any rate, they are solid in the portions which still exist. In the case of Anderida and Richborough, they were solid throughout, and fulfilled the function of buttresses, but where this cannot be absolutely established, as in the camps mentioned, it is possible that the plan frequently adopted by the Roman may have been executed, viz. that the tower was solid up to the platform of the wall, and then had a chamber at the top, such as is seen at present in the main walls of Rome, which were built by Aurelian about 270 A.D., i.e. shortly after ours. In cross-section the towers are, as a rule, round, but may also be strongly U-shaped; this is particularly the case at Anderida and at Portchester. The Romans employed such U-shaped towers for the purpose of better flanking the walls, and on the solid platforms on the tops of the towers they placed catapults and scorpions.

Apropos of the artillery just referred to, it is worth noting that the Romans had both fortress artillery and field artillery, the latter consisting of small balistæ, or catapults and scorpions, for firing large darts, mounted on small carts and drawn by mules. As to the efficacy of their weapons, you may remember the tragic fate of an officer of the Goths, who at the siege of Rome, when that city was defended by Belisarius, climbed a tree opposite the gate to reconnoitre, but was transfixed by a dart fired from a catapult on one of the towers, and nailed to the tree thereby.

The round towers which are so remarkable at Anderida and Portchester, are similar to those found in many Continental Roman castra. For instance, in France the towers of the outer wall of Carcassonne are of the U-shaped variety, even if perhaps but Visigothic copies of the original; those of Dax and of Bourges, are round, and so, too, those of Jublains, which last named castrum is in many particulars identical

with our south-coast examples. In the remarkable town of Troesmis, founded in the third century, near Trajan's wall at the mouth of the Danube, these U-shaped towers are particularly well seen.

The arrangements within the walls.—These fortified towns, of course, enclose an area with numerous buildings, but so far very little systematic excavation has been made. I have found myself that the arrangement depended very much on the condition of the outside wall; and I may here digress for a moment to take up the question of apparent deficiencies in the outside wall, which have always excited much controversy among antiquarians. Thus at Rutupius, Lemanus, Regulbium, Anderida, and Garriononum, one wall is wanting, but as regards Regulbium, there is extant a seventeenth century map, which I now show you, exhibiting the fact that the castrum in question was a perfect quadrangle, and the missing north wall has simply been washed away by the sea. At Rutupia ruins of the wall have been found on the cliff. At Anderida the missing part is obviously just beneath the turf and has been proved to be there by excavation; it may have been thrown down by the undermining processes of the mediæval siege which the place underwent. At Garriononum also the foundations have been traced. We come then to the single instance of Lemanus. This castrum, the walls of which it is admitted on all hands have been dislocated and ruined by a landslip, stands on the slope of a hill, and at its lower border, i.e. the south side, there is a slightly elevated piece of ground which was taken by Mr. Roach Smith, who made the first excavation here, to be a portion of the landslip. Thinking one day that I could detect in the bank some of the flat foundation cross stones of an ordinary wall, I instituted a systematic excavation of this portion of the circumference of the camp, and found that so far from its being an elevation merely due to landslip, it was really a remarkably interesting example of a revetted harbour wall. It consisted of a mass of layers of boulders embedded in concrete, on the top of which the ordinary wall was built. At this particular point the concrete was carried backwards over the area of the camp for forty feet, so as to form a large and immovable platform. It seems to me that this was possibly the platform of a battery for defending the ships moored by the wall, as represented on one of Trajan's coins.

To return to the question of the buildings within these castra; as is shown in Trajan's column, there were numerous houses covered with pent-house roofs and the well-known roofing tiles with imbricated edges, such as are seen to be extensively used in many parts of this country, especially in East Anglia, and which we certainly owe to the Romans, as our Belgic and Celtic forefathers roofed their huts with straw and thatch, such as may be also seen in many parts of the country, especially well exemplified again in East Anglia.

There were, of course, a forum and pretorian building, the residence of the Governor, and barracks for the troops, basilica for the Court of Justice, and private houses, markets, baths, etc. All

these are shown on excavation of the areas of these walled towns and castra, and are well exhibited in the plan (Daremberg's Encyclopædia) of the town of Troesmis.

The buildings in the castra now under consideration have only to a slight degree been ascertained, and have moreover in three striking cases been confused by the erection of mediæval buildings within them, such as the Norman and later works in Portchester, including not only the keep and allied buildings, but also, in the further corner of the castrum, a Norman church, at Pevensey again a Norman keep, and buildings including a chapel. At Richborough there is the most remarkable formation in the centre, an enormous platform of concrete, 30 feet thick, with a cruciform block of masonry in the middle, which was occupied by a chapel in mediæval times. This may have been preceded by an early Roman British church, or the platform may have formed the base of a lighthouse tower, analogous to the two which stood on each side of the valley at Dover, only one of which now remains next the church within the accessory * Roman Camp now included in the enceinte of the mediæval castle.

You may remark that the Church of Reculver stands within the castrum, and that the construction of its chancel arch with two Roman pillars shows the very early origin at least of the Augustinian epoch, if not earlier.

At Lympne I found that the site of what apparently was the Governor's house, which had already been discovered by Mr. Roach Smith, exhibited round the outside wall an original cobble stone paving, which we may conclude was generally employed for the side streets in such walled castra.

The same camp Lympne also shows the foundations of some large buildings, which may be taken to have been store-houses, and have been termed barracks, but do not resemble the barracks, gladiatorial or otherwise, at Pompeii at all, where a large number of rooms opened on to a court-yard. The windows of such buildings are extant at Lympne, being built into the wall of an out-house there. As Mr. Roach Smith has shown, and other archæologists have shown, such openings were rounded at the top, with embrasured sides, and the window sill bevelled outwards as a rain drip. The walls were plastered, and then painted in distemper. These buildings were warmed by the usual hypocausts.

Besides these official buildings, there were, of course, numerous small houses and shops, which are set forth in the Troesmis plan, and are analogous to the buildings now being so carefully explored at Silchester.

We now come to the historical stories, which, so far as we know, are closely associated with these fortified stations, and, as I have already suggested, the interest really begins towards the end of the

* Accessory because the walled town of Dubris lay below in the valley.

third century, viz. 286 A.D. It will be remembered that the then Emperor Diocletian (who like Hadrian was a great administrator) found that the size of the Empire, and the very personal system of government of the Romans, rendered it necessary to subdivide the imperial power, and he therefore constituted a government of himself and Maximianus. The latter, having command of Gaul and Britain, appointed over the British fleet a Belgian named Carausius, who was already in the Roman Navy as a pilot or Gubernator, a position which was analogous to the Master in a man-of-war in our own navy in the beginning of this century—an officer on whom great responsibility in navigation devolved.

This man Caurausius seems to have been a person of great individual energy and ability; his headquarters were at Boulogne, the ancient Gessoriacum, but which we must now call Bononia. Finding that his reputation and influence were greatly increasing, he began to pave the way for seizing power over the fleet and Britain.

For this purpose, he evidently sent over agents into Britain to work public feeling in his favour, and, to support it further, had coins struck with a portrait of himself on one side, and an inscription which said, "Come, oh expected one." Having thus cleared the ground, he duly came, and apparently made his headquarters for a time at Rutupia, and then at Clausentum. Having done this, he next intimated to Diocletian and Maximinius that he had assumed the imperial status, and the title of Augustus. Maximinius, upon whom the responsibility of dealing with this insurrection devolved, found his hands were too full in Gaul and North Africa, and consequently with Diocletian, made a virtue of necessity and recognised him, upon which Caurausius, with a certain sense of humour, struck coins in which he represented himself with a diadem and his colleagues without, and put an inscription, "Caurausius and his two brothers." When, however, his hands were less hampered, his nearest brother Maximinius despatched a very able general named Constantine Chlorus, who forthwith opened the campaign against Caurausius by laying siege to Bononia, and ultimately capturing it. Caurausius had withdrawn into Britain, and there was murdered, at one of these camps we are now discussing, by his chief subordinate in command, Alectus, who also set up a claim for imperial rank, and commenced a coinage, of which numerous examples are to be found in the south-eastern camps. Constantius sent against Alectus a general named Ascupuladorius, who secured the province to Rome by taking his attacking fleet over in two divisions, crossing under shelter of a fog, and defeating Alectus who was waiting to receive him.

At this time the Saxons and North Germanic tribes increased their piratical incursions on both sides of the Channel, and the removal of Caurausius no doubt rendered their attacks much safer to them and more profitable. From this time, at any rate to the beginning of the fifth century, there can be little doubt that the Saxons steadily increased in force, and the Picts and Scots combined to render

the existence of the Romano-British people very hard. The best evidence of this state of things is really given by the manuscript list of officers under the empire which is known as the Notitia diguatotorum. This accompanying reproduction of the map of the Notitia, from Horsley's 'Britannia Romana,' shows that the defences of the country had been gradually re-organised, and the troops concentrated along the line of the Roman wall and the camps in relation to it, and the remainder along the south-east and south coast. The second legion, for instance, which had been in the north 150 years, was brought down and posted in Richborough. The sixth legion, which had been in England for 200 years, was taken to Rutupia and then removed altogether. The Notitia is an extremely valuable document, because it gives all the details of the staff-command of the South Eastern coast. The composition of the staff is very interesting, and the Count, as he was called, of the Saxon shore, seems to have always held a very prominent position. The last matter of interest and importance on this subject is the question of the fate of those fortifications and, concurrently, the explanation why, as in some places, e.g. London, they remain as they are. All the southern part of England was studded over with Roman villas, and these always seem on excavation to have been burnt down and not inhabited afterwards, and woodland allowed to spring up around the ruins, in many cases burying them. The reason for this given by some is, that for a certain time the villas, having been inhabited by large families and resident subordinates, were built for comfort but not for defence, and after their destruction by the piratical and the superstitious Saxons, were believed to be occupied by the ghosts of their former owners. Terrible for this cause, and useless to a people needing fortresses rather than homes, they were left in undisturbed ruin. This does not appear to have been the case with walled towns, for obvious reasons: these camps remained inhabited to a certain extent during the mediæval period, but not for long, except in the case of Porchester, Richborough, and the large towns like Dover, &c., which have been continuously inhabited since their Roman construction.

I should like to finish this sketch by showing you two examples illustrative of the period of which it treats: one of the fate of a large walled station such as we have been considering, and of which we have an actual record in a Saxon chronicle still existing; the other of the destruction of a Roman dwelling house.

The first example is the storming of Anderida (Pevensey). Here we are told in the Saxon chronicle that the Saxons, who had landed in Portsmouth harbour, and who no doubt had already taken Portus Magnus (Porchester) and Clausentum, marched eastward along the Roman Road until they ultimately came to Anderida, which they besieged for a long time without any success. The Saxons were not skilled in sieges, and it is especially noted that during the attack their lines of circumvallation were continually being assailed by the Britons moving out from the forest and falling upon the rear of the

Saxon forces. Ultimately the place was carried by storm, and all its inhabitants massacred.

The second example, that of the destruction of a villa, is remarkably shown by the following instance, for which I have to thank Mr. Storry, the curator of the Cardiff Museum. This gentleman had observed in a field which was known in the neighbourhood as "the battlefield," that there was some indication of a Roman foundation in its centre, and on examining this found the customary rough masonry upon which the timber and plaster walls of a Roman villa were erected. On following up the passage, which was the first part of the villa opened into, he found it led into a large room with a good pavement, the tesseræ of which were broken and the surface indented with horses' hoofs. The floor was covered by numerous human skeletons and those of horses, while in the corner of the room were the skeletons of two children, and across, in front of them, that of a woman. Further, in three graves dug in the floor, were found the skeletons of men of a larger and more powerful build than those whose remains were left unburied where they fell. The evidence is circumstantial but complete. The whole story is told. The unfortified dwelling house, the attack by the stronger invaders, the retreat of the household along the passage to its inmost room, the last stand of the little garrison, the slaughter of the men, the murder of the woman, and last of all the massacre of the children, in front of whom she had thrown herself in a final desperate effort to save them from the inevitable destruction. There are those who find the study of old walls dull, and wonder that some can pore for hours over a jaw from a cave, a flint from a field, or a bit of Roman mortar, but these things are the keys to the unwritten history of man, and when we find how vivid a page can be restored to us from the floor of a single house, one's wonder should rather be that so much still remains uncared-for and unread.

[V. H.]

GENERAL MONTHLY MEETING,

Monday, February 6, 1899.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

George C. Cathcart, Esq. M.A. M.B. C.M.
Hon. Alan de Tatton Egerton, M.P.
Sir Francis Henry Evans, K.C.M.G. M.P.
John George Glover, Esq.
John William Gordon, Esq.
John Gretton, Esq.
John Gretton, Esq., Jun. M.P.
Charles Edward Groves, Esq. F.R.S. F.C.S.
Alexander Ritchie, Esq. J.P.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned for the following Donation to the Fund for the Promotion of Experimental Research at Low Temperatures:—

Professor Dewar	£100
Professor Frank Clowes	£10 10*s.*

The following resolution was proposed from the chair by Sir James Crichton-Browne, M.D. F.R.S. the Treasurer, seconded by Sir Frederick Bramwell, Bart. F.R.S. the Honorary Secretary, and unanimously adopted:—

"The Members of the Royal Institution of Great Britain in General Meeting assembled desire to express to his Grace the Duke of Northumberland their sympathy with him and his family in the loss which they, as well as the Royal Institution and the country at large, have sustained by the death of his father, the late Duke. They desire also to record their grateful sense of the invaluable services rendered to the Royal Institution by the late Duke during his membership of 48 years' duration; during seven terms of office as Visitor and Manager, and as President for 26 years; by his active and unfailing interest in its affairs, and in the progress of science, which it is its object to promote; by his generous benefactions to its funds; and by the true nobility of his character, which has lent distinction to its proceedings during an eventful period of its history."

The Special Thanks of the Members were returned to C. E. Melchers, Esq. for his donation of £50 for alterations in the Lecture Theatre.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Annual Progress Report of the Archæological Survey Circle, N.W. Provinces and Oudh, for year ending June 1898. fol.
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The Mogul Architecture of Fathpur-Sikri. By E. W. Smith. Part 3. 4to. 1897.
Monograph on Buddha Sakyamuni's Birthplace in the Nepalese Terai. By A. Fuhrer. 4to. 1897.
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Atti, Serie Quinta: Rendiconti. Classe di Scienze Fisische, &c. 2° Semestre, Vol. VII. Fasc. 10–12; 1° Semestre, Vol. VIII. Fasc. 1. 8vo. 1899.
Agricultural Society of England—Journal, Vol. IX. Part 4. 8vo. 1898.
American Geographical Society—Bulletin, Vol. XXX. No. 5. 8vo. 1898.
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Analyst for Dec. 1898 and Jan. 1899. 8vo.
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Astrophysical Journal for Dec. 1898. 8vo.
Athenæum for Dec. 1898 and Jan. 1899. 4to.
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Education for Dec. 1898 and Jan. 1899. 8vo.
Electrical Engineer for Dec. 1898 and Jan. 1899. fol.
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Engineering for Dec. 1898 and Jan. 1899. fol.
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Editors—continued.
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Invention for Dec. 1898 and Jan. 1899. 8vo.
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New Church Magazine for Dec. 1898 and Jan. 1899. 8vo.
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Science of Man for Nov. 1898.
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Travel for Dec. 1898 and Jan. 1899. 8vo.
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Zoophilist for Dec. 1898 and Jan. 1899. 4to.

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Philologisch-Historische Classe—
Berichte, 1898, No. 4. 8vo.
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Berichte, 1898, No. 5. 8vo.

Scottish Society of Arts, Royal—Transactions, Vol. XIV. Part 4. 8vo. 1898.

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Tacchini, Prof. P. Hon.Mem.R.I. (*the Author*)—Memorie della Società degli Spettroscopisti Italiani, Vol. XXVII. Disp. 9, 10. 4to. 1898.

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Wagner Free Institute of Science of Philadelphia—Transactions, Vol. III. Part 4. 8vo. 1898.

Wisconsin Geological and Natural History Survey—On the Instincts and Habits of the Solitary Wasps. By G. W. Peckham and S. G. Peckham. 8vo. 1898.
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WEEKLY EVENING MEETING,

Friday, February 10, 1899.

The Hon. Sir James Stirling, M.A. LL.D., Vice-President, in the Chair.

Professor H. S. Hele-Shaw, LL.D. M. Inst. C.E.

The Motion of a Perfect Liquid.

If we look across the surface of a river, we cannot fail to observe the difference of the movement at various points. Near one bank the velocity may be much less than near the other, and generally, though not always, it is greater in the middle than near either bank. If we could look beneath the surface and see what was going on there, we should find that the velocity was not so great near the bottom as at the top, and was scarcely the same at any two points of the depth. The more we study the matter, the more complex the motion appears to be; small floating bodies are not only carried down at different speeds and across each other's paths, but are whirled round and round in small whirlpools, sometimes even disappearing for a time beneath the surface. By watching floating bodies we can sometimes realise these complex movements, but they may take place without giving the slightest evidence of their existence.

You are now looking at water flowing through a channel of varying cross section, but there is very little evidence of any disturbance taking place. By admitting colour, although its effect is at once visible on the water, it does not help us much to understand the character of the flow. If, however, fine bubbles of air are admitted, we at once perceive (Fig. 1) the tumultuous conditions under which the water is moving and that there is a strong whirlpool action. This may be intensified by closing in two sides (Fig. 2), so as to imitate the action of a sluice gate, through the narrow opening of which the water has all to pass, the presence of air making the disturbed behaviour of the water very evident.

Now you will readily admit that it is hopeless to begin to study the flow of the water under such conditions, and we naturally ask, are there not cases in which the action is more simple? Such would be the case if the water flowed very slowly in a perfectly smooth and parallel river bed, when the particles would follow one another in lines called "stream-lines," and the flow would be like the march of a disciplined army, instead of like the movement of a disorderly crowd, in which, free fights taking place at various points may be supposed to resemble the local disturbances of whirlpools or vortices.

The model (Fig. 3) represents on a large scale a section of the channel already shown, in which groups of particles of the water are indicated by round balls, lines in the direction of flow of these groups (which for convenience we may call particles) being coloured alternately. When I move these so that the lines are maintained, we imitate "stream-line" motion, and when, at any given point of the pipe, the succeeding particles always move at exactly the same velocity, we have what is understood as "steady motion."

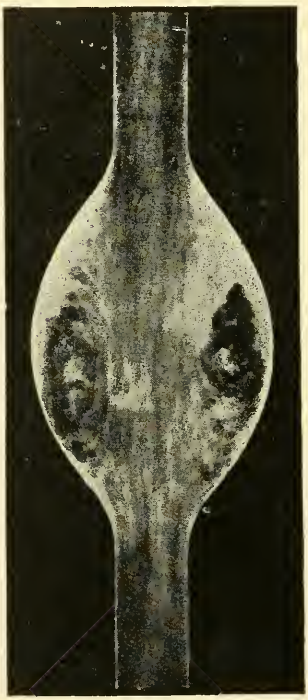

Fig. 1.

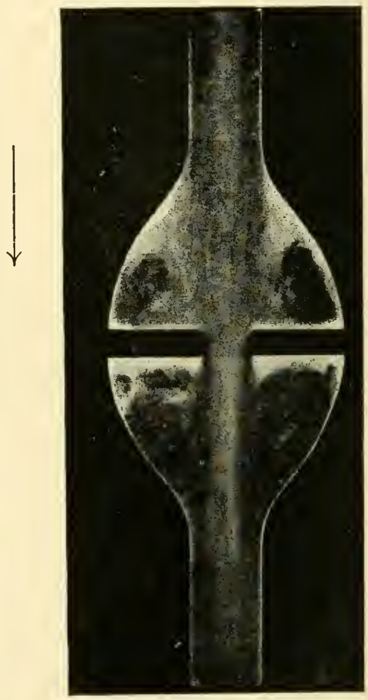

Fig. 2.

As long as all the particles move in the straight portion of the channel, their behaviour is easy enough to understand. But as the channel widens out, it is clear that this model does not give us the proper distribution. In the model, the wider portions are not filled up, as they would be, with the natural fluid; for it must be clearly understood that the stream-lines do not flow on as the balls along these wires, passing through a mass of dead water, but redistribute themselves so that every particle of water takes part in the flow.

Perhaps you may think that if these wires were removed, and the wooden balls allowed to find their own positions, they would group themselves as with an actual liquid. This is not the case; and, for reasons that you will see presently, no model of this kind would give us the real conditions of actual flow. By means of a model, however, we may be able to understand why it is so absolutely essential we should realise the correct nature of the grouping which occurs.

First look at the two diagrams on the wall (Figs. 4 and 5), which you will see represent channels of similar form to the experimental one. The same number of particles enter and leave in each under apparently the same conditions, so that the idea may naturally arise in your minds, that if the particles ultimately flow with the same speed whatever their grouping in the larger portion of the channel, it cannot much matter in what particular kind of formation they actually pass through that wider portion. To understand that is really very important. Let us consider a model (Fig. 6) specially made for the purpose. You will see that we have two lines of particles which we may consider stream-lines, those on the left coloured white, and those on the right coloured red. The first and last are now exactly 18 inches apart, there being eighteen balls of 1 inch diameter in the row. If I move the red ones upward, I cause them to enter a wider portion of the channel, where they will have to arrange themselves so as to be three abreast (Fig. 7). It is quite clear to you, that as I do this their speed in the wider portion of the channel is only one-third of that in the narrow portion, as you will see from the relative positions of the marked particles. Now, directly the first particle entered the wider channel, it commenced to move at a reduced speed, with the result that the particles immediately behind it must have run up against it, exactly in the same way that you have often heard the trucks in a goods train run in succession upon the ones in front, when the speed of the engine is reduced; and you will doubtless have noticed that it was not necessary for the engine actually to stop in order that this might take place. Moreover, the force of the impact depended largely upon the suddenness with which the

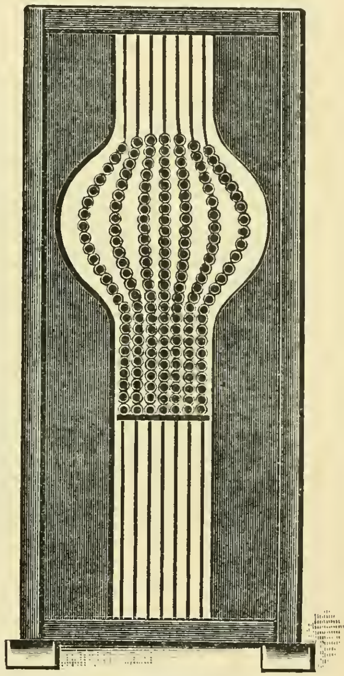

Fig. 3.

speed of those in front was reduced. Applying this illustration to the model, you will see that the impact of these particles in the wider portion would necessarily involve a greater pressure in that part. Turning next to the white balls, I imitate, by means of the left-hand portion, the flow which will occur in a channel six times as large as the original one, and you now see (Fig. 7) that as the particles have placed themselves six abreast, and the first and last row are 3 inches apart instead of 18 inches, the speed in the wider portion of the channel must have been one-sixth of that in the narrow portion. Evidently, therefore, the velocity of the particles has been reduced more rapidly than in the previous case, and the pressure must consequently be correspondingly greater.

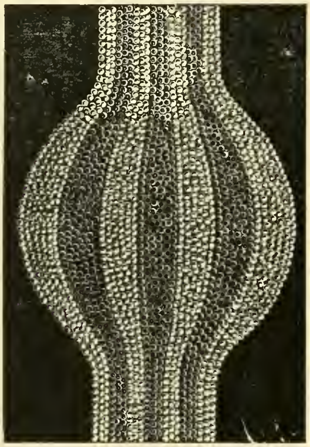

Fig. 4.

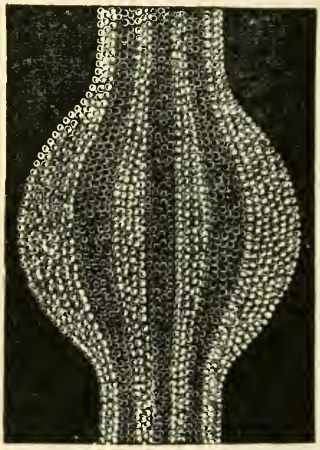

Fig. 5.

We may now take it as perfectly clear and evident, that the pressure is greater in the wider portion and less in the narrower portion of the channel. Turning now to the two diagrams, we see that the pressure is in each case greater in every row of particles as in the wider portions of the channel, but that instead of being suddenly increased, as in the model, it is gradually increased. The width of the coloured bands, that is, rows of particles, or width apart of stream-lines, is a measure of the increased pressure. Thus you will now regard the width of the bands, or what is the same thing, the distance apart of the stream-lines, as a direct indication of pressure, and the narrowness or closeness of the stream-lines as a direct indication of velocity.

Next notice the great difference between the two diagrams. In one diagram (Fig. 4), the change of width is uniform across the entire

section. In diagram (Fig. 5), however, this is not the case. In the narrowest portion of the channel in each diagram, there are seven colour bands of little balls each containing three abreast, but we find that in one diagram (Fig. 4) they are equally spaced in the wider part six abreast throughout. In the other diagram (Fig. 5), the outer row is spaced eight abreast, the second row rather more than six, and the inner rows rather more than four abreast, and the middle row less than four abreast, making in all forty-two in a row, as in the previous case. One diagram (Fig. 5), therefore, will represent an entirely different condition to the state represented by the other diagram (Fig. 4), the pressure in the wide part of the latter varying from a maximum at the outside to a minimum in the middle, while the corresponding velocity is greatest in the middle and least at the outside or borders.

Now, when we know the pressure at every point of a liquid, and

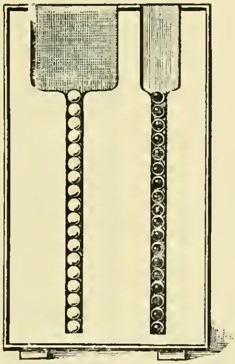

Fig. 6.

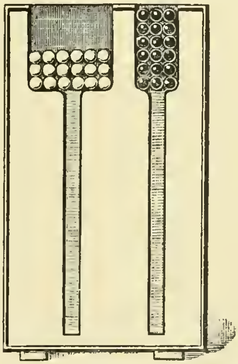

Fig. 7.

also the direction in which the particles are moving, together with their velocity at every point, we really know all about its motion, and you will see how important the question of grouping is, and that, in fact, it really constitutes the whole point of my lecture to-night. How then shall we ascertain which of the two groupings (Fig. 4 or 5) is correct, or whether possibly some grouping totally different from either does not represent the real conditions of flow?

Now, the model does not help us very far, because there seems to be no means of making the grouping follow any regular law which might agree with fluid motion. In whatever way we improve such a model, we can scarcely hope to imitate by merely mechanical means the motion of an actual liquid, for reasons which I will now try to explain.

In the first place, apart from the particles having no distinguishing characteristics, either when the liquid is opaque or transparent, they are so small and their number is so great as to be almost beyond our

powers of comprehension. Let me try, by means of a simple illustration, to give some idea of their number, as arrived at by perfectly well recognised methods of physical computation. Lord Kelvin has used the illustration that, supposing a drop of water were magnified to the size of the earth, the ultimate particles would appear to us between the size of cricket balls and footballs. I venture to put the same fact, in another way, that may perhaps strike you more forcibly. This tumbler contains half a pint of water. I now close the top. Suppose that, by means of a fine hole, I allow one and a half billion particles to flow out per second—that is to say, an exodus equal to about one thousand times the population of the world in each second, —the time required to empty the glass would be *between* (for of course we can only give certain limits) seven million and forty-seven million years.

In the next place, we have the particles interfering with each other's movements by what we call "viscosity."

Of course, the general idea of what is meant by a "viscous" fluid is familiar to everybody, as that quality which treacle and tar possess in a marked degree, glycerine to a less extent, water to a less extent than glycerine, and alcohol and spirits least of all. In liquids, the property of viscosity resembles a certain positive "stickiness" of the particles to themselves and to other bodies; and would be well represented in our model by coating over the various balls with some viscous material, or by the clinging together, which might take place by the individuals of a crowd, as contrasted with the absence of this in the case of no viscosity as represented by the evolutions of a body of soldiers. It may be accounted for, to a certain extent, by supposing the particles to possess an irregular shape, or to constantly move across each other's paths, causing groups of particles to be whirled round together.

Whatever the real nature of viscosity is, it results in producing in water the eddying motion which would be perfectly impossible if viscosity were absent, and which makes the problem of the motion of an imperfect liquid so difficult and perplexing.

Now, all scientific advance in discovering the laws of nature has been made by first simplifying the problem and reducing it to certain ideal conditions, and this is what mathematicians have done in study- the motion of a liquid.

We have already seen what almost countless millions of particles must exist in a very small space, and it does require a much greater stretch of the imagination to consider their number altogether without limit. If we then assume that a liquid has no viscosity, and that it is incompressible, and that the number of particles is infinite, we arrive at a state of things which would be represented in the case of the model or the diagram on the wall, when the little globes were perfectly smooth, perfectly round and perfectly hard, all of them in contact with each other, and with an unlimited number occupying the smallest part of one of the coloured or clear bands. This agrees with

the mathematical conception of a perfect liquid, although the mathematician has in his mind the idea of something of the nature of a jelly consisting of such small particles, rather than of the separate particles themselves. The solution of the problem of the grouping of the little particles, upon which so much depends, and which may have at first seemed so simple a matter, really represents, though as yet applied to only a few simple cases, one of the most remarkable instances of the power of higher mathematics, and one of the greatest achievements of mathematical genius.

You will be as glad as I am that it is not my business to-night to explain the mathematical processes by which the behaviour of a perfect liquid has been to a certain extent investigated. You will also understand why such models as we could actually make, or any analogy with the things with which we are familiar, would not help us very much in obtaining a mental picture of the behaviour of a perfect liquid. If, for instance, we try to make use of the idea of drilled soldiers, and move the lines with that object in view, we see that instead of the ordinary methods of drill, the middle rank soon gains on the others, and enters again the parallel portion of the channel in a very different relative position to the opposite lines, although the stream-lines would all have the same actual velocity when once again in the parallel portion. Since, then, we cannot use models or any simple analogy with familiar things, or follow—at any rate this evening—the mathematical methods of dealing with the problem, what way of understanding the subject is left to us?

If we take two sheets of glass, and bring them nearly close together, leaving only a space the thickness of a thin card or piece of paper, and then by suitable means cause liquid to flow under pressure between them, the very property of viscosity, which as before noted, is the cause of the eddying motion in large bodies of water, in the present case greatly limits the freedom of motion of the fluid between the two sheets of glass, and thus prevents not only eddying or whirling motion, but also counteracts the effect of inertia. Every particle is then compelled by the pressure behind and around it to move onwards without whirling motion, following the path which corresponds exactly with the stream-lines in a perfect liquid.

If we now, by a suitable means, allow distinguishing bands of coloured liquid to take part in the general flow, we are able to imitate exactly the conditions represented in the diagrams (Figs. 4 and 5). You are now looking at a projection on the screen (Fig. 8, Plate I.) of liquid, which, in flowing through the gradually enlarging and contracting channel, is obeying the conditions I have described. Such is the steadiness of its motion, that it is scarcely possible to believe at first that the figure does not consist of fixed bands of colour painted in perfectly smooth curves. By varying the flow of the coloured liquid however, you will realise at once that the painting is done by nature and not by the hand of a human artist.

Now you will notice that the bands widen out as they approach the

wider portion of the channel, afterwards contracting to their original width; but I have already prepared you for the fact that they do not do this uniformly, and, in spite of the fact that they were all equally spaced in the narrower portion of the pipe, they are very unequally spaced in the wider portion—in this you will see the resemblance to the model, Fig. 3, and the case given in Fig. 5.

You will not, I trust, now fall into the very natural mistake of thinking that the nature of the substance is more attenuated because the band has become wider, but will realise that the particles are in the wider portion just as close together as in the narrower. I have already explained that as more particles are required to fill the greater width, and can only be supplied from the same band behind, the band at that part cannot possibly be moving as fast as the narrow bands at the same cross-section, that is, on the line drawn across at right angles to the central line of the stream.

This I will now prove to you by a very simple but conclusive experiment, for by opening and closing the tap regulating the colour bands, we can start a fresh supply exactly at the same instant in each of the bands—in the same way as the starter attempts, though usually not with the same success, to carry out his duties on the racecourse. (Fig. 9, Plate I.) shows the different position of various colour band fronts which were all started in line, and gives a good idea of the relative changes of velocity. You will see that the straight formation of the row is not maintained, even in the parallel portion of the channel, the middle row gaining on the sides, which is not because of any resistance on the sides, but because the influence of the enlargement is felt before that is actually reached. Then, you see, the middle portion slows down considerably, and, for an instant, of course, the portions which lag behind on the sides appear to be overtaking it; they in turn, however, have to occupy so much more space on the sides, that they fall rapidly behind and the once straight row of particles becomes, in leaving, more and more curved. This curve is so drawn out as to leave no doubt in your minds as to which band of particles wins the race; and, although ultimately these particles are again flowing along the narrow channel at the same velocity, whether in the middle or at the sides, the particles which started at the middle, at the same time as the particles at the sides, have obtained the lead in finally entering the channel again. This lead they will continue to maintain, unless they should encounter an obstacle in the middle of the channel, when, as I shall be able to show you in a subsequent experiment, their positions may possibly be reversed.

By now gradually closing in the slides, so as to reproduce conditions of a narrow diaphragm or channel with ordinary flow, instead of the turbulent or whirlpool motion which then resulted, the colour bands at once respond to the altered conditions (Fig. 10), and, like a perfectly drilled body of troops, perform the required evolutions immediately, even though the defile through which they are compelled to pass involves almost incredibly rapid change of speed. So great, indeed, is

Fig. 8.

Fig. 9.

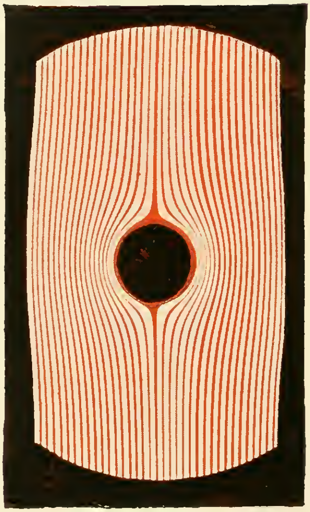

Fig. 12.

Fig. 10.

Fig. 11.

Fig. 14.

Fig. 15.

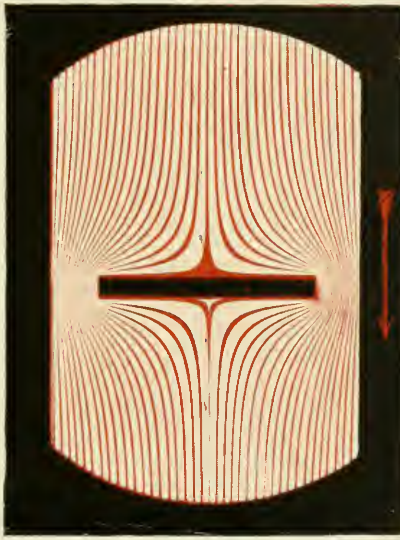

Fig. 16.

Fig. 17.

Fig. 18.

the confidence which we may place upon their behaviour, that the enlargement from the original channel, Figs. 1 and 2, as you see, has been much exaggerated in order to make the conditions as severe as possible, and intensify the effect. The greater pressure in the wider portion may be illustrated by the fact, that while the plugs remain at rest in the middle, where the narrow bands are, they are forced out, when removed to the sides, by the greater pressure, which there acts on the ends. This is where the bands were widest and the velocity slowest. This is quite contrary to what might have been expected, seeing that the liquid was forced so rapidly through the narrower channel, but it needs many illustrations to bring home to us this apparent contradiction of our ordinary experience. Fig. 11 shows the liquid now flowing out through the new channel thus made, as well as through the original place of exit.

But at this stage you may reasonably enquire how it is that we are able to state, with so much certainty, that the artificial conditions of flow with a viscous liquid are really giving us the stream-line motion of a perfect one; and this brings me to the results which mathematicians have obtained.

The view now shown represents a body of circular cross-section, past which a fluid of infinite extent is moving, and the lines are plotted from mathematical investigation and represent the flow of particles. This particular case gives us the means of most elaborate comparison; although we cannot employ a fluid of infinite extent, we can prepare the border of the channel to correspond with any one of the particular stream-lines, and measure the exact positions of the lines inside.

By means of a second lantern, the real flow of a viscous liquid for this case is shown upon the second screen, and you will see that it agrees with the calculated flow round a similar obstacle of a perfect liquid. The diagram shown on the wall is the actual figure employed for comparison, and upon which the experimental case was projected. By this means, it was proved that the two were in absolute agreement. If we start the impulses, as before, in a row, we at once see how the middle particles lag behind the outer ones, as indicated by the width of the bands, showing that it is not necessarily the side stream-lines that move more slowly. It may be more interesting to you to see, in addition to the foregoing case—in which for convenience, and as quite sufficient for measurement only, a semi-cylinder was employed—the case of a complete cylinder, and this is now shown (Fig. 12, Plate I.). In this case two different colours are used in alternate bands, and these bands are sent in, not steadily but impulsively, in order to illustrate what I have just pointed out. You will see how the greater width of the colour bands before and behind the cylinder indicates an increase of pressure in those regions. This in a ship-shape form accounts for the standing bow and stern waves, whereas the narrowing of the bands at the sides indicates an increase of velocity and reduction of pressure, and accounts for the depression of water level, with which you are doubtless familiar at the corresponding part of a ship.

I will now take a more striking case. If, instead of a circular body, we had a flat plate, the turbulent nature of the flow is evidently very great, as you will see from the view (Fig. 13), which is a photograph of the actual flow under these conditions, made visible by very

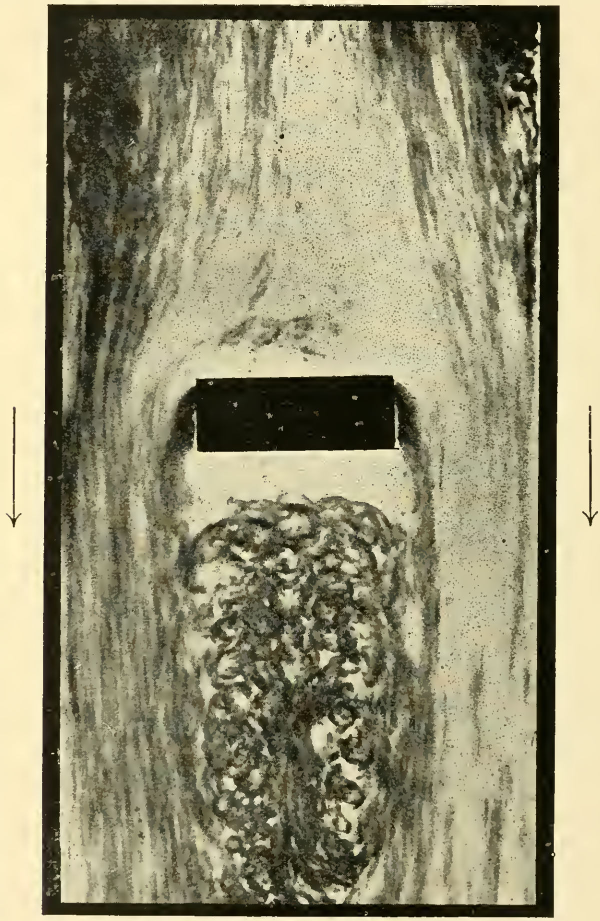

Fig. 13.

fine air bubbles, and showing water at rest in the clear space behind the obstacle.

We can, however, take steps to reduce this turbulence, and you now see on the second screen the flow by means of apparatus which time

does not permit me to describe, but which gives a slow and steady motion that it would be impossible to improve upon in actual conditions of practice, or even, I am inclined to think, by any experimental method. Instead of using air to make this flow clear, we now allow colour to stream behind the plate, and you will see (Fig 14, Plate II.) that the water still refuses to flow round to the back, and spreads on either side. We have so slow a velocity as not to induce vortex motion, but the inertia of the particles which strike the flat plate causes them to be deflected to either side, exactly as tennis balls in striking against a wall obliquely. The sheet of water is so thick, that is to say, the parallel glass plates are so far apart, that they do not enable the viscosity of the water to act as a sufficient drag to prevent this taking place.

To make the action of the water in front of the plate more visible, a different coloured liquid is allowed to enter from orifices in a small pipe placed across the slide in the thick sheet. You will now see that the general motion is steady enough to give a very clear idea of the deflecting action of the plate, and streaks of colour set themselves in such a way as to indicate the behaviour of the individual particles. This effect is practically what is called "discontinuity," for, although perfectly discontinuous motion can only take place when there is no viscosity, the effect of the general flow upon the nearly quiescent mass behind the plate is very slight.

If we send the flow in impulses, we produce vortex motion at the edge, due to viscosity, as shown in Fig. 15. This takes place in the thick sheet directly the velocity is sufficiently increased, though only at the edges of the plate, the motion being otherwise the same.

Mathematicians, however, predicted with absolute certainty, that with stream-line motion the water should flow round and meet at the back, a state of things that, however slow we make the motion in the present case, does not occur owing to the effect of inertia. They have drawn with equal confidence the lines along which this should take place. We could either effect this result with the experiment you have just seen, by using a much more viscous liquid, such as treacle, or, what comes to the same thing, bringing the two sheets of glass nearly close together; and the flow which you are now witnessing (Fig. 16, Plate II.) shows the result of doing this. The colour bands in front of the plate no longer mix at all with the general body of flow, or are unsteady, as was the case in the last experiment, but flow round the plate and flow so steadily, that, unless we jerk the flow of the colour bands, it is impossible to tell in which direction they are actually moving.

A still more extraordinary case is that of a plate at an angle of 45 degrees, the central line no longer striking the plate at the centre, but at a certain point which, together with the actual curves of flow, has been calculated and plotted by Professor Lamb. The calculations being made for an infinite fluid, we require the artificial border to be prepared, corresponding to the different stream-lines, and when

that is done, we find that the flow absolutely agrees with that predicted by Professor Lamb, the central line which meets the plate, leaving it exactly with the same form at a corresponding point on the back, the curves of each being hyperbolas. This effect is, of course, produced by the central line dividing on the plate, a portion flowing upwards and a portion downwards, reuniting at a corresponding point behind, from whence it flows away.

Such a state of things would be absolutely impossible to conceive by most of us, but by turning the plate at an angle in the lantern, we are able to approximately represent, even without artificial border lines, this condition of flow. You are thus able to see a striking example of the absolute accuracy of mathematical prediction, and to feel every confidence, that the original experiment in the channel, or indeed any others with thin viscous films, should give us indeed a correct picture of what we can never hope to see, viz. the motion of a perfect liquid.

It is satisfactory to know that the principle of the thin films has been examined by probably the greatest authority on the subject, and as a result, Professor Sir G. Gabriel Stokes states, that " they afford a complete graphical solution, experimentally obtained, of a problem which, from its complexity, baffles the mathematician except in a few simple cases."

Whilst I have been dealing with the stream-lines of a perfect liquid, your minds will doubtless have turned to the lines along which magnetic and electrical forces appear to act. We are possibly further from realising the actual nature of these forces, than from a correct conception of the real nature of a liquid. We have long agreed to abandon the old ideas of the electrical and magnetic fluids flowing along these lines, and to substitute instead the idea, that these lines represent merely the directions in which the forces act. Now we can easily see that this conception is quite a reasonable one, for in the case of the model it is not necessary to have the row of balls actually moving, in order that the effect may be transmitted along the different lines they occupy. If I attempt to raise the plate upon which they rest, the pressure is instantly transmitted through the whole row to the top ball along each line, whatever curve the line may take. In the same way, you will remember that it was not necessary to have the colour bands actually in motion, for, though apparently free to move in any direction, they retain their form for a considerable time, and the path along which they would influence each other as soon as the tap is opened, would be along those lines in which the liquid was flowing before it was brought to rest. Hence it is possible, with some suitable means, to cause a viscous liquid to reproduce exactly the lines of magnetic and electrical induction. In the case of magnetism and electricity, it is of course possible, by means of a small magnetic needle or a galvanometer, by exploring the whole surface through which magnetic induction or electrical flow is acting, to plot the lines of force for innumerable cases, where we can work in air or on the surface of the solid conductor.

But in this building it seems natural to take as an example the case first used by the great man to whom the conception of lines of magnetic force is due, for the first reference I have been able to find to such lines is in one of Faraday's earliest papers on the induction of electric currents,* in which he says, "By magnetic curves I mean the lines of magnetic forces, however modified by the juxtaposition of poles, which would be depicted by iron filings, or those to which a very small magnetic needle would form a tangent."

You are all familiar with the way in which iron filings set themselves, when shaken over the North and South poles of a magnet. The magnetic lines are then nearly, but not quite, circular curves between the two poles Now, the mathematics of the subject tells us that if the poles could be regarded as points, the lines of force between them would be perfect circles.

You are now looking (Fig. 17, Plate II.), at the colour bands, the edges—or indeed any portion—of which represent lines obtained by admitting coloured liquid from a series of small holes round a central small orifice, which admits clear liquid, and allows them to escape through another small orifice (called respectively in hydromechanics a *source* and *sink*), and I leave it to you to judge how far these curves deviate from the ideal form.

My assistant is now allowing the colour to flow, first steadily and then in a series of impulses, and the latter gives us the conception of waves or impulses of magnetic force, though of course the magnetic transmission force would be instantaneous. Regarded as a liquid, it is here again clear how absolutely the truth of our views concerning the slower movement in the wider portion is verified by this experiment.

A last experiment (Fig. 18) shows the streams admitted, not from a source but from a row of orifices in what corresponds to the slowest moving portion of the flow. The result is, that the colour bands are much narrower, and although the circular forms of the curves are, as in the previous experiment, preserved, the lines are so fine at the point of exit, which, as before, corresponds to the South Pole, as to really approximate to ideal stream-lines.

The same method enables us to trace the lines of force through solid conductors, for, as long as we confine ourselves to two dimensions of space, we may have *flat* conductors of any shape whatever. But it does something more, for by making the film rather deeper in some places than others, more particles arrange themselves there, and the lines of flow will naturally tend in the direction of the deeper portion. This will give the stream lines identically the same shape as the magnetic or electrical curves which encounter in their paths a body of less resistance, for instance, a para-magnetic body.

If, on the other hand, at these points the film is made rather

* 'Experimental Researches in Electricity,' vol. i. p. 32

thinner, less particles will be able to dispose of themselves in the shallow portion of the film, and hence the lines of flow will be pushed away from this portion, giving us exactly the same forms as magnetic lines of force in a magnetic field in proximity to a dia-magnetic body.

Here, again, mathematical methods have enabled lines of actual flow to be predicted, and you may compare the actual flow for the case of a cylindrical para-magnetic body, which was worked out some years ago.

You will doubtless not be inclined to question the practical value of stream-lines in the subject which we have just been considering, because, unlike the flow of an actual liquid, magnetic lines of force can never be themselves seen, and because there is no doubt as to the correspondence of the directions to the lines of a perfect liquid. It was the conception of these lines in the mind of Faraday, and more particularly their being cut by a moving wire, that enabled him to realise the nature of the subject more clearly than any other man at the time, and to do so much towards the rapid development of electrical science and its practical applications.

When we come to consider the relation of the study of the motion of a perfect liquid with hydromechanics and naval architecture, it must be admitted that the matter is a difficult one. Probably one of the most perplexing things in engineering science is the absence of all apparent connection between higher treatises on hydrodynamics and the vast array of works on practical hydraulics. The natural connection between the treatises of mathematicians and experimental researches of engineers would appear to be obvious, but very little, if any such connection exists in reality, and while at every step electrical applications owe much to the theories which are common to electricity and hydromechanics, we look in vain for such applications in connection with the actual flow of water.

Now the reason for this appears to be the immense difference between the flow of an actual liquid and that of a perfect one owing to the property of viscosity. A comparison of the various experiments which you have seen to some extent indicates this.

In the first place let us consider for a moment some of the things which would happen if water were a perfect liquid. In such a case, a ship would experience a very different amount of resistance, because, although waves would be raised, owing to the reasons which we have already seen, the chief causes of resistance, viz. skin friction and eddying motion, would be entirely absent, and of course a submarine boat at a certain depth would experience no resistance at all, since the pressures fore and aft would be equal. On the other hand, there would be no waves raised by the action of the wind, and there would be no tidal flow, but to make up for this rivers would flow with incredible velocity, since there would be no retarding forces owing to the friction of the banks. But the rivers themselves would soon cease to flow because there would be no rainfall such as exists at

present, since it is due to viscosity that the rain is distributed, instead of falling upon the earth in a solid mass when condensed. In a word, it may be said that the absence of viscosity in water would result in changes which it is impossible to realise.

We may now briefly try to consider the difference between practical hydraulics and the mathematical treatment of a perfect liquid. The earliest attempts to investigate in a scientific way the flow of water appears to have been made by a Roman engineer about 1800 years ago, an effort being made to find the law for the flow of water from an orifice. For more than 1500 years, however, even the simple principle of flow according to which the velocity of efflux varies as the square of the head, or what is the same thing, the height of surface above the orifice varies as the square of the velocity, remained unknown. Torricelli, who discovered this, did so as the result of observing that a jet of water rose nearly to the height of the surface of the body of water from which it issued, and concluded therefore that it obeyed the then recently discovered law of all falling bodies.

Though it was obvious that this law did not exactly hold, it was a long time before it was realised that it was the friction or viscosity of liquids that caused so marked a deviation from the simple theory. Since then problems in practical hydraulics, whether in connection with the flow in rivers or pipes, or the resistance of ships, have largely consisted in the determination of the amount of deviation from the foregoing simple law.

About 100 years ago it was discovered that the resistance of friction varies nearly in accordance with the simple law of Torricelli and also—although for a totally different reason—the resistances due to a sudden contraction or enlargment of cross section of channel or to any sudden obstructions appear to follow nearly the same law. Now it is extremely convenient for reasons which will be understood by students of hydraulics, to treat all kinds of resistance as following the same law, viz. square of velocity which the variation of head or height of surface has shown to do. But this is far from being exact, and an enormous amount of labour has consequently been expended in finding for all conceivable conditions in actual work tables of co-efficients or empirical expressions which are required for calculations of various practical questions. Such data are continually being accumulated in connection with the flow of water in rivers and pipes, for hydraulic motors and naval architecture. This is the practical side of the question.

On the other hand, eminent mathematicians since the days of Newton and the discovery of the method of the calculus, have been pursuing the investigation of the behaviour of a perfect liquid. The mathematical methods which I have already alluded to as being so wonderful, have however scarcely been brought to bear with any apparent result upon the behaviour of a viscous fluid. Indeed, the mathematician has not been really able to adopt the method of the practical investigator, and deal with useful forms of bodies such as

those of actual ships, or of liquid moving through ordinary channels of varying section, even for the case of a perfect liquid, but he has had to take those cases, and they are very few indeed, that he has been able to discover which fit in with his mathematical powers of treatment.

This brief summary may possibly serve to indicate the nature of the difficulties which I have pointed out, and will show you the vast field there yet lies open for research in connection with the subject of hydromechanics, and the great reception which awaits the discovery of a theoretical method of completely dealing with viscous liquids, instead of having recourse as at present principally to empirical formula based on the simple law already alluded to.

We may, however, console ourselves with the thought, that in the application of the laws of motion themselves to *any* terrestrial matters, the friction of bodies must always be taken into account, and renders it necessary, that we should commence by studying the ideal conditions. In this as in other matters the naval architect and engineer must always endeavour as far as possible to base their considerations and work upon the secure foundation of scientific knowledge, making allowances for disturbing causes, which then cease to be the source of perplexity and confusion. From this point of view, the study of the behaviour of a perfect liquid, even when no such form of matter appears to exist, has an interest for the practical man in spite of the deviation of actual liquids from such ideal conditions. If the truth must be told, it is such a deviation from the simple and ideal conditions that really constitute the work of a professional man, and it is only practical experience which, based upon sound technical knowledge, enables 50,000 tons of steel to be made to span the Firth of Forth, Niagara to be harnessed to do the work of 100,000 horses, or an 'Oceanic' to be slid into the sea with as little misgiving as the launch of a fishing boat.

I have, I am afraid, brought you only to the threshold of a vast subject, and in doing so, have possibly employed reasoning of too elementary a kind. After all, I may plead that I have followed the dictum of Faraday, who said, "If assumptions must be made, it is better to assume as little as possible." If I have assumed too little knowledge on your part, it is because of the difficulties I have found in the subject myself. If I have left more obscure than I have been able to make clear, it is consoling to think how many centuries were required to discover even what is known at the present time, and we may well be forgiven if we cannot grasp at once results which represent the life-work of some of the greatest men.

WEEKLY EVENING MEETING,

Friday, February 17, 1899.

Sir Frederick Bramwell, Bart., D.C.L. LL.D. F.R.S., Honorary Secretary and Vice-President, in the Chair.

Richard R. Holmes, Esq., M.V.O. F.S.A.

George the Third as a Collector.

The subject of this discourse I have chosen particularly because it is one which has in most histories either been passed over entirely, or treated with indifference. It is generally disposed of in a few curt phrases taken from the pages of contemporary diarists, repeated by every subsequent writer as containing everything necessary to be recorded of the King's tastes and epitomising his character with epigrammatic smartness, but seldom verified by examination or research. The stormy political quarrels at home and the complication of events abroad have combined to cast into oblivion the early cultivated tastes and pursuits of the King, as in later years the dark clouds of disease obscured the finer workings of his brain.

To appreciate fully the extent and value of the collections by which George III. has permanently enriched the possessions of the Crown, it is as well to consider briefly the condition in which His Majesty found the ancestral treasure of his Royal house when he succeeded his grandfather on the throne. Spoliation and robbery had played sad havoc among them, and it is only wonderful that anything of value was left at all. The nation perhaps may be congratulated that, on the foundation of the British Museum, the ancient library of the Kings of England was transferred there by George II., and so escaped the fate of many of the treasures of the Crown. In a note prefixed to a MS. catalogue of the pictures of Queen Anne by Horace Walpole, who once owned the volume, he says :—

"As several pictures mentioned in the following catalogue have not appeared in any of the palaces within my memory I imagine many were taken away by different persons between the death of Queen Anne and the arrival of George I. Henrietta Lady Suffolk told me that Queen Caroline never had any of Queen Anne's jewels but one pearl necklace. George I., who hated her and his son, might give what he found to the Duchess of Kendal. Her niece, Lady Chesterfield, certainly had several large diamonds. Catherine of Braganza, widow of Charles II., carried away several of the pictures of the Crown of Portugal. A Lord Chamberlain pawned the Vandyke hangings at Houghton to a banker, who, many years after, they not

being redeemed, sold them to my father. . . . When all the stores at Somerset House were brought by order of George III. to Kensington that His Majesty might choose some to replace what he had taken from Windsor and Hampton Court for the Queen's house in St. James's Park, he gave the residue, as they said, to Earl Gower, Lord Chamberlain, and his deputy, Sir Robert Wilmot, and the refuse they gave to Mr. Town, under-housekeeper at Kensington; but as he was servant to my sister, Lady Mary Churchill, then housekeeper, and as I had the care of that palace during her absence in France, I said, 'Mr. Town, the Lord Chamberlain may take or give what he pleases, but you are under my sister, take none, leave them here,' and they were left."

In another note Walpole goes on to say of one who has not generally been credited with much taste for art, "Frederick, Prince of Wales, was very desirous of reassembling all he could of the collection of Charles I. He bought many fine pictures, particularly the principal, of Mr. Humphry Edwin's collection;" and in mentioning the collections of George III. he proceeds to say, "he inherited from his father Prince Frederick's collection of miniatures, among which were Dr. Mead's admirable works of Isaac Oliver, namely, the whole-length of Sir Philip Sydney, and the heads of Queen Elizabeth, the Queen of Scots, Ben Jonson, and Oliver himself."

This gossip of Horace Walpole's, which is dated from 1783, I have quoted as an introduction to my subject, and to give an idea of the chaotic condition in which the collections were at the beginning of the reign.

When George III. succeeded to the throne he was still young, and had been brought up in comparative seclusion by his mother, the widowed Princess of Wales. He was then entirely under her influence and that of Lord Bute, who, whatever may be said of his political conduct, was a man of no mean intellect or culture, being devoted to literature and the fine arts, and passionately fond of botany. He was the constant friend and companion of the young Prince of Wales, and by him the tastes of the future king were in a great measure formed.

The dominant fashion of the time at the King's accession was the study of classical antiquity, led by the Society of Dilettanti, which had then been formed about a quarter of a century. George III. did not follow in the footsteps of this convivial club, though his agents secured for him a very important collection of works relating to antiquity, which will be mentioned later. His first thought was to form a library, which should replace the collection which, as I have already mentioned, his predecessor had given to the British Museum. But even before he had formulated this project, he had enriched the National Museum by the gift of the Thomason Collection—a curious and unique assemblage of English literature printed during the period of the Civil War—containing about 33,000 separate articles published between 1640 and 1662, and bound in over 2000 volumes. The collection had been valued at some thousands, and after having been for

sale for many years, was bought by the King for the trifling sum of 300*l.* and given to the British Museum, where its contents are known as "the King's Tracts or Pamphlets." This seems to have been the first purchase made by the King, who now began to form that splendid collection which has ultimately found its own resting place side by side with the earlier Royal Library in the great storehouse in Great Russell Street.

In 1762 the fine library of Consul Smith was bought for the King. Joseph Smith had settled in early life at Venice as a merchant, but he was principally a buyer and seller of works of art. He was omnivorous in his tastes, and his library was a mass of bibliographical treasures, which he had obtained by scouring all Italy in search of the rarest specimens of the early printers. For the collection George III. gave about 10,000*l.*, and he then proceeded to build upon this foundation that collection which has hitherto remained as the finest private library ever brought together.

In the preface to the Catalogue of the King's Library, my predecessor, Sir Frederick Barnard, gives an account of the manner in which its increase was developed, and the sage advice which contributed to its welfare. "Dr. Samuel Johnson was one of the earliest and most zealous promotors of its success; his visits to the library were frequent, during which he appeared to take pleasure in instructing youth and inexperience by friendly advice and useful information. At one of these visits he was surprised by the sudden and unexpected appearance of the King, and His Majesty was pleased to enter into a long conversation with him upon the library and various other subjects, which from recollection has been so frequently and minutely detailed that it is only necessary to add that the forcible impression which such a distinguished attention left upon his mind disposed him readily to embrace an opportunity of manifesting his zeal for the accomplishment of the plan upon which His Majesty had done him the honour to consult him." A part of this plan was the despatch of Mr. Barnard to the Continent to acquire further books, and he received from Dr. Johnson an admirable letter, also printed in the same preface, which may be read with advantage by every librarian.

In the half century which passed between the date of this letter and the death of the King, unremitting attention was paid to the increase of the library. Nor did the long indisposition of His Majesty suspend its progress; many large and choice acquisitions were made for it abroad, but perhaps the most valuable was that of the superb series of examples of the press of Caxton, which was added to the shelves by gift, bequest, or purchase. They numbered in all thirty-nine, including one 'The Doctrinal of Sapience,' at that time, and for many years after, the only book known printed by Caxton on vellum. This and some other volumes, personal gifts to the King, such as the sumptuous copy of the Mainz Psalter of 1437, the earliest printed book with a date, are still preserved in the Royal Library at Windsor.

The total number of volumes in the library at the time of the King's decease was between eighty and ninety thousand, all fine, in good condition, and splendidly bound. It was feared at one time that the whole might leave the country, as overtures for its acquisition were made by a great foreign potentate; but the wise counsels of the Ministry of the new Sovereign had their weight, and this noble collection is now worthily housed in the National Museum, where it is kept apart as the "King's Library."

Under the same roof is also preserved the great Numismatic Cabinet formed by the King, and presented to the nation in 1823 by George IV. It contained specimens of Greek, Roman, English and foreign coins and medals, many of singular beauty, and of the greatest rarity. These are no longer kept by themselves in one collection, as in the case of the books, but have been dispersed through the various cabinets of the Museum, according to their proper classification, but each one has a special ticket with it, showing the source from which it came. The value of the whole must have been very great, amounting to at least sixty thousand pounds.

At the same time that the King was laying the foundation of his library, he was making other and no less important additions to his treasures. In 1762, a gentleman writing from Rome says, "Nothing gives me more satisfaction than to find so many fine things purchased for His Majesty the King of Great Britain lately in Italy. He is now master of the best collection of drawings in the world, having purchased two or three capital collections in Rome, the last belonging to Cardinal Albani, for 14,000 crowns, consisting of 300 large volumes, one-third of which are original drawings by the first Masters, the others, collections of the most capital engravings; and lately there has been purchased for His Majesty all the museum of Mr. Smith, consisting of his library, prints, drawings, designs, &c. I think it is highly probable that the Arts and Sciences will flourish in Great Britain under the protection and encouragement of a monarch who is himself an excellent judge of merit and taste in the vertu." Sir Horace Mann tells the same tale in one of his letters from Florence to Horace Walpole. "Have you heard what a quantity of things have been bought and are buying for the King? Cardinal Albani's collection of drawings and prints were paid 14,000 crowns (about three thousand guineas). Mr. Smith's whole collection and library has been purchased at the price of 20,000*l.* sterling, and Mr. Dalton is now in Venice packing it up. Many expensive things of that sort were lost in a ship that took fire at sea some months ago, the crew of which saved their lives by becoming prisoners to the Spaniards at Carthagena. In short, I believe that there is no ship departs from any port in Italy that has not something for the King." It was at that time a splendid opportunity for a collector; the artistic treasures amassed in the seventeenth century by Princes and Cardinal nephews, by the Barberini, Giustiniani, Odescalchi, and others, were, owing to the increasing pecuniary embarrassments of the great families, being dispersed in every direction, though much, particularly of sculpture,

was secured by the Papal agents for the museums of Rome. Many marbles belonging to Cardinal Albani had gone to Dresden, but his collection of drawings was, by the agency of James Adam, one of the well-known brothers, secured for George III. The collection had been started in the previous century by the Commendatore Cassiano del Pozzo, and among its treasures was a series of volumes of particular value, as preserving at least, in the form of copies, many works of classic art which have since disappeared. Nine large volumes contain elaborate drawings of ancient bas-reliefs, and besides these are several volumes filled with the careful studies of Francesco and Pietro Santa Bartoli. Two volumes, also, are filled with drawings of the Christian Antiquities of Rome, including remains of mural paintings, and, what is of very much more value, careful drawings of the great mosaics of the churches, drawn with infinite elaboration before the time when these most valuable works were almost entirely ruined by neglect or restoration.

Valuable as these volumes are, the series of original drawings by old masters is far more precious, and with those which were already in the possession of the Crown, makes the Royal Collection at Windsor one of the most important in Europe. In number they amount to considerably over 20,000, and comprise many of the finest works of the greatest men.

Holbein was here already represented by the unique series of over eighty heads drawn by him from the life while he was painting at the Court of Henry VIII.; but these must be passed over, as they form no part of the collection of George III. The same also may be said in part of the drawings and MSS. of Leonardi da Vinci, which are the greatest pride of the Windsor Library, as these with the Holbeins had been unearthed from a closet at Kensington by Queen Caroline, wife of George II., and by her were well-known and appreciated. These precious works of Leonardo were largely increased by George III., whether from the Albani Collection just mentioned, or from the Bonfigoli Collection, which formed part of that of Consul Smith, or from one of the numerous additions made from time to time by Dalton, there is no means of ascertaining. Thus augmented, the Royal Library now contains far more of the work of the great master than the contents of all other collections put together. It is here a pleasure to note that these MSS. are now being published in *facsimile*, though some time must elapse before the work is completed.

The original number of volumes in which the mass of drawings of Old Masters was bound was about 250. Unfortunately for the present generation, whose taste is formed on different models from those of a century ago, thirty-four of these volumes are filled by the studies of Domenichino, a highly respectable craftsman, some scores more contain an interminable series of Masters of the Bolognese School, Caraccis, Guido, Guercino and the like, but of these it is useless to enter into particulars. Of the earlier and of the more important masters, the drawings have been removed from the volumes in which they were exposed to much injury by rubbing,

and by order of the late Prince Consort have been mounted so as to ensure their safety, and are kept in portfolios. Among these are drawings by Angelico, Perugino, Pinturicchio, Signorelli, Mantegna, and Fra Bartolommeo. The drawings by Raphael are numerous, and some may be classed among the finest specimens of his work now extant.

Second only in importance to the unique collection of drawings by Leonardo comes the series by Michael Angelo. In fact it is no exaggeration to declare that until the student has seen these purchases of George III. he can have no proper insight into the marvellous power of that extraordinary man, for in no other collection are to be found specimens so complete in design and of such minute and elaborate finish.

Great artists of other countries such as Durer, Claude, Poussin, with many of the Dutch School, are also represented by choice examples.

Of the pictures collected by or painted by order of the King, a large number remain in the Royal Palaces; the historical pictures painted by West for His Majesty commended themselves at the time to a few, but are now forgotten. The King's best bestowal of his patronage was upon the portrait painters who flourished during his reign, and were the money value of the portraits as they exist to be computed, the amount would be fabulous. The full-length portraits by Beechey and Reynolds are of great importance, supplemented by those by Romney and Hoppner; but the principal treasures are the portraits by Gainsborough, which are of exquisite purity and freshness, the series of ovals of the King and Queen, and their children in a series, being almost matchless in charm of execution.

Whilst encouraging the painters of his own day, George III., by his purchase from Consul Smith, enriched the galleries of the Crown by a number of the works of the Venetian painter Antonio Canal, commonly known as Canaletto. Those who only know this Master by his smaller works, have very little idea of the magnificent qualities which he could develop. Smith was the possessor of most of his finest pictures; he bought everything which came from the Master's easel. For the smaller works he seems to have had a ready sale, but the greater and finer pictures remained on his hands, and these came all into the possession of George III. Of the fifty pictures thus added, all are fine, but some twenty are of exceptional size, and of equal power and beauty. With the pictures came also a volume of drawings, 150 in number, many of them studies for these pictures.

The King was also a great admirer of miniatures, and added to his collection many of the works of Cosway and Ozias Humphry, to whom he gave many commissions, and their works are not an unworthy supplement to the great historical series which is one of the treasures of the library at Windsor. In the same room where these exquisite specimens of a lost art are preserved, is stored the vast group of engravings also collected by the King. Few public museums, and perhaps no private cabinet, can rival this in the number and value

of its engraved portraits. In its portfolios the Kings and Queens of the Empire, with their families, are represented by every known engraving which could be acquired, many of the utmost rarity and value, some perhaps unique, and all specially selected for beauty of impression. After these come in due order the Sovereigns of other Royal houses; nobles, statesmen and warriors, and others, all combine to swell this wonderful gallery, which embraces, or attempts to include, the likeness of every one of every country whose features were considered worthy to be handed down to posterity.

In addition to the engraved portraits, there was collected by His Majesty a vast number of engravings, which are arranged under the different schools of painters whose works they represent. Of these the most complete and important are the engravings by Hogarth, and those after Sir Joshua Reynolds. There is also a nearly complete collection of the 3000 plates engraved by Hollar.

This is a very hasty and imperfect list of the works of art which George III. collected, and has left behind him among the treasures of the Crown, but it by no means exhausts the subjects in which he took a keen and practical interest; such as his love of Botany, and the encouragement he gave to Sir Joseph Banks; his studies in scientific agriculture; and the introduction to this country of the breed of merino sheep. The foundation by him of the Royal Academy must on no account be omitted, nor the gift to it of over 5000*l.* from his privy purse, besides other grants and privileges.

From what has been said it must be acknowledged that the man who could devote so much attention and energy to the collection of so vast an accumulation of worthy objects, had tastes and aims of a high character; and here it may be mentioned that that generally admirable work the Dictionary of National Biography has in its notice of the King made a notable departure from its usual accuracy and impartiality in recording that "his taste was execrable." This sweeping condemnation is founded entirely on the report given by Miss Burney of one of the earliest conversations which she held with the King, who was then, as it seems, endeavouring to draw out a lady who was shortly to be introduced into the personal household of his Queen. It must be said here that a better and more appreciative opinion is given of the King by an authority far higher, Dr. Johnson, who, after his first interview, said to Mr. Barnard, "Sir, they may talk of the King as they will, but he is the finest gentleman I have ever seen." He reiterated to his friends his admiration of the King's talents and charms, and his testimony is more worthy of respect than the hastily jotted down notes of an exceedingly self-conscious and somewhat spiteful spinster. It is only bare justice to the memory of George III. that the facts which have been thus enumerated and not always remembered to his credit, should be even imperfectly put on record. [R. R. H.]

WEEKLY EVENING MEETING,

Friday, February 24, 1899.

Sir William Crookes, F.R.S., Vice-President, in the Chair.

Professor Oliver Lodge, D.Sc. LL.D. F.R.S.

Coherers.

A coherer is an instrument which responds to electric waves somewhat in the same manner as a microphone responds to sound waves.

A coherer is a light metallic contact or series of contacts introduced into an electric circuit of low voltage containing also a galvanometer or other signalling instrument. A steady current is normally unable to pass, or only very feebly, by reason of the high resistance of the bad joint, but under the influence of a sudden change of potential, or an electric jerk, the resistance of the joint suddenly diminishes, transmitting a considerable current, and signalling the arrival of the electric wave which may have caused the jerk. A slight shake or tap is sufficient to reduce the coherer to its former high resistance.

All metals do not behave in the same way, but the majority thus show an increase of coherence under electric influence, and a sudden decrease under mechanical influence. A few metals (e.g. silver) appear to behave in an opposite direction.

The earliest instances of electrical cohesion exhibited by the lecturer was the small vertical fountain issuing from a smooth orifice, which was found by Lord Rayleigh to scatter its drops by mutual collision except when they were under the influence of an electric field such as that due to a piece of sealing-wax held within a yard or two of the place where the jet breaks into drops. Another variety was the pair of horizontal jets, which, when they impinge, unite or rebound according as there is or is not a difference of potential between them of one or two volts. A pair of soap bubbles in contact were also shown by Mr. Boys to cohere and become one directly a stick of sealing-wax was produced in their neighbourhood.

The two halves of a mercury globule on a flat surface, cut into two with a greasy knife, and the parts connected to the poles of a battery, were found by Lord Rayleigh and by Mr. Appleyard to re-unite directly they were connected to the terminals of one or two Grove cells; a slight delay in the union suggesting that a film of foreign matter was being squeezed out from between the globules under the force of electrostatic attraction.

The electrified dust and smoke experiment, whereby a thick fog in a chamber can be cleared by the discharge from a point, as observed by Lodge and Clark in 1883, was also shown; and the lightning guard experiment with a couple of Leyden jars and a galvanometer and two surfaces in light contact, by which the lecturer observed electrical cohesion of metals in 1888, was exhibited, together with a couple of resonant Leyden jars, one of them charged and sparking, the other responding by closing the circuit of a local battery and ringing a bell; which bell, by its vibration, could effect the tapping back.

The discovery that this property of metals served as the best detector for Hertz waves was made by Monsieur Edouard Branly, Professor of Physics in the Catholic Institute of Paris; and some of Monsieur Branly's original apparatus was exhibited, especially a piece of ebonite smeared over with porphyrised copper so as to form a high resistance, which fluctuated in value between certain limits under the influence of alternate electric sparks and tapping. A Branly filings-tube connected to a speaking galvanometer was shown receiving signals from a Hertz emitter at a distance, the coherer being tapped back automatically in one of many alternative ways. A recent experiment of Signor Tomasina, displaying the effect of electric cohesion was projected on the screen:—A vertical wire about nine inches long had its end immersed in filings and was slowly raised. If a sphere of suitable size were sparking in the neighbourhood, between polished knobs, the electric jerks collected by the vertical wire would give, as Hertz showed, minute perhaps ultra-microscopic sparks to anything brought close to its end. The filings subjected to this action are found to cohere, and can be pulled up in a narrow string, of length depending on the steadiness of the movement.

(M. Tomasina has recently repeated this experiment under liquid, and obtained chains of filings several inches long.)

Another variety of the cohesion experiment under electrical influence, was shown by the lecturer in a form which suggested that electrostatic attraction played a considerable part. A very fine platinum wire was suspended in a glass box, with its lower end close to a flat and highly polished facet of a brass knob. On looking at this wire under a microscope, it and its image in the polished face could both be seen, a slight distance apart; or they could be projected with a strong lens upon a screen. Under the action of electric waves, the gap between the wire and its image sharply disappeared, and the wire was seen clinging to the knob until tapped back. A wire of this kind constitutes an extremely sensitive electroscope, but for this purpose the coherence which sets in (unless silver or some such non-cohering metal is employed) is inconvenient.

Finally a layer of filings on a horizontal glass surface, with tin foil electrodes, was projected on the screen, and subjected to strong electric influence, under which they were seen to move so as to close

up gaps, and were then found to have cohered; for if the superfluous filings were then gently removed, a continuous chain of irregular shape remained reaching from one terminal to the other. By suitably choosing the filings their motion when subjected to very slight electric sparks can readily thus be seen, and if subsequently a point be used to sweep them sideways, they are found to be quite in a different condition to ordinary unelectrified filings, for they are matted together into a sort of coherent mass just like the dust particles in a bell-jar under the influence of an electrified point, or somewhat like iron filings in a magnetic field. A stronger spark often destroys this cohesion, scattering the particles asunder, and producing somewhat the same effect as a mechanical tap, though for a different reason. Filings thus electrically disorganised are not usually in a sensitive condition. A set of large brass shavings on a flat surface, with sparks sent through them, at first show lines of spark in all directions, but gradually under the cohering influence are able to close up and presently conduct the discharge without visible manifestation.

Thus the process going on more or less in coherers, either the single-point or the multiple point kind, can be made to some extent apparent to the eye.

Some of the old apparatus used by the lecturer to send signals by Hertz waves and coherers over small distances (the now so-called wireless telegraphy) which had been exhibited to the Institution on Friday evening, June 1st, 1894, were once more exhibited, the receiver being carried about into different parts of the room: but this method of signalling has become well known and developed under the auspices of Signor Marconi, who has succeeded in telegraphing by its means across the sea over distances up to twenty or thirty miles.

[O. L.]

WEEKLY EVENING MEETING,

Friday, March 3, 1899.

Sir Henry Thompson, Bart., F.R.C.S. F.R.A.S., Vice-President, in the Chair.

Sir Frederick Pollock, Bart., M.A. LL.D. F.S.A. *M.R.I.*

King Alfred.

The position of King Alfred * in English history—one might almost say in European history—is unique. He is the first commanding figure in the roll of English princes after the Saxon conquest of South Britain, and after a thousand years there is still none greater. Other kings and statesmen have worked on a larger scale, with more powerful instruments, and for more brilliant immediate results. But none has wrought more strenuously or more successfully with the means at his command. Others, such as Henry II., have left the record of lives as full of activity and public zeal; others, again, like Simon de Montfort and Edward I., have worked long and valiantly for aims which on the whole were noble, with judgment which on the whole was wise, and by means which, if not always or altogether laudable in themselves, were no worse, or indeed better, than those in common use and allowance at the time. But very few, if any, have remained so free as Alfred from any kind of censure, or have actually stood higher and not lower in the esteem of later generations as their circumstances came to be more fully understood. This can be said of Alfred, and said without reserve. A blameless life, if we mean thereby a life not chargeable with definite wrongs or vices, or with culpable incompetence, is not necessarily a matter for great praise in a private citizen. Often it imports little more than the absence of temptation; sometimes nothing better than other men's usual ignorance of his intimate character and relations. It may even be the result of an unworthy shrinking from difficult or dangerous tasks which might have brought great temptations, but also great occasions, and in this case it may deserve only the faintest degree of external approbation, or rather should be ranked with the deceitful, so-called good works which have

* The contemporary form "Ælfred" is preserved even in Asser's Latin. Our modern literary form gives the correct pronunciation to modern readers, and I see no reason for departing from it; though, if English spelling is ever reformed, we certainly ought to restore the Anglo-Saxon notation 'æ' for our peculiar short vowel, and also þ and ð for the two distinct sounds expressed by 'th' (I am aware that they are used indiscriminately in A.-S. writings). On the other hand, I preserve Æthelwulf, &c., because those names have no accepted forms in modern English.

the nature of sin. But to live, work, and fight in the full light of day, a ruler and leader of men; to dare greatly for great ends, and accomplish them; and with all this to leave a fame so clear that no man dares lift a voice against it; this is not only good and of laudable example, but an evident mark of greatness. And this is how it stands with our King Alfred. He was tried in many ways and failed in none. He was neither a mere exemplar of negative virtues like Edward the Confessor, nor a speculative reformer with inopportune good intentions. Many things came to his hand to do, and every one of them was well done. He is not chargeable, so far as we know, with any one serious error of judgment. It is true that both the military and the political details of the time are in many ways obscure to us; and it is certain that Alfred had to suffer one great reverse. But if there had been any ground for supposing that want of any possible precaution on the King's part contributed to it, we may be sure that some record or tradition of it would be preserved. Even the best of rulers must make some enemies if he does his duty without fear. Alfred's enemies could find nothing to say against him, or, at the very least, nothing that was plausible enough to be remembered. We can have no stronger proof that there was really nothing of the kind to be said.

The bare chronicle and abridgment of King Alfred's deeds is much, but to see them in their full greatness we must try to realise in what manner of world a King of the West-Saxons had to reign in the ninth century. It was a world of hardship and peril; not occasional, but constant, such as had not been known in a great part of Europe within historical times. The old order of the Roman Empire had broken up. The new order of mediæval Christendom—itself to be swept away in the convulsions of religious wars when its work was done—was not yet come to the birth, and the new invasions of the heathen Northmen threatened to bring a worse chaos than the first. Only the Church, with such remnants of Roman political and official tradition as it had been able to preserve, was a stable power making for civilisation, and saving learning from total extinction. Hobbes's great epigram on the Papacy—"the ghost of the deceased Roman Empire sitting crowned upon the grave thereof"—is not fairly applicable to the early mediæval Church, which was really preserving the remnant of life till better times. The best that can be said on the whole for the dark ages, as they are commonly and justly called, is that there was still some light in the Church. But Hobbes's equally well-known description of what "is consequent to a time of war, where every man is enemy to every man," might be taken for a not highly exaggerated description of the dark ages at their worst:—

"In such condition, there is no place for industry, because the fruit thereof is uncertain, and consequently no culture of the earth; no navigation, nor use of the commodities that may be imported by sea; no commodious building; no instruments of moving, and removing, such things as require much force; no knowledge of the face of the

earth; no account of time; no arts; no letters; no society; and which is worst of all, continual fear and danger of violent death; and the life of man solitary, poor, nasty, brutish, and short."

England was still parcelled out into several kingdoms, whose dynastic intrigues and mutual jealousies blinded their rulers to the common danger already growing upon them from the Northmen. Some of these kingdoms had come out of heathendom at a time not very far back. Two hundred years before Alfred's birth the most powerful prince in Britain was a heathen, Penda of Mercia. This was, indeed, half a century after Augustine's mission to Kent. But Augustine's work had not run smooth, and the final triumph of Christianity came from the North. It was late in the seventh century when the Church was definitely established in England, mainly by the work of Theodore of Tarsus, under the supremacy of Rome.

Little more than a hundred years passed when the new civilisation of England was surrounded by fresh heathen enemies. In the second quarter of the ninth century a great Viking expedition, which had been preceded by sundry smaller and inconclusive raids, conquered all the northern part of Ireland. The Northmen could not, indeed, retain a permanent hold on more than the maritime stations of Dublin, Waterford and Limerick. But their power resting on these bases of operation was formidable for more than two centuries.* About the middle of the same century—soon after King Alfred's birth—there began systematic attacks on the coasts of Western Europe, which continued through a generation of alarm and disaster. The Vikings were settled at the mouths of the Rhine and the Scheldt; they harried the lands of the Seine and the Loire, and—until Alfred ordered things better—had full command of the Channel and the North Sea. Warning had not been wanting, for successive raids, apparently in considerable force (834–837), were repulsed by Alfred's grandfather, Egberht of Wessex. But, in Alfred's infancy, the Danes began to settle down for winter quarters in England. In 851 London was ruined; in 860—when Alfred was old enough to hear and understand the tale—Winchester was plundered. Æthelwulf, Alfred's father, was a well-meaning but feeble prince, it seems, a kind of pious founder born out of due time. He gave his thoughts to enlarging his offerings to the Church when the question was whether the Danes would leave any gifts for the King to offer, or any churches to receive them, and he talked of remitting dues when the kingdom needed all its resources in men and wealth. At best those resources were barely adequate for the need. Population was scattered; great parts of the country were still covered with uncleared forests; communication by land was so slow and precarious that the rude navigation of those days was better wherever it was possible; the warlike habits of the old

* Keary, 'The Vikings in Western Christendom,' c. vi. Even after the Norman Conquest, a Danish fleet from Ireland did much damage on the South Devon coast, which is not only recorded but estimated in Doomsday Book.

English invaders were forgotten; there was little skill and less discipline, and every kind of authority was weak. The Danish foemen, compact, mobile, seafaring, expert in the wars by which they lived, and trained by necessity to obey their chiefs at least in the day of battle, had the advantage at all points. It was Alfred's task to redress the balance—a task demanding both genius and perseverance.

In such a world, in the year 849, as we are told,* Alfred was born, the youngest son of his father Æthelwulf. In his boyhood he was sent to Rome in great state, and some ceremony took place which was afterwards magnified into the Pope having anointed him as king. As Alfred's claim to succeed his father in Wessex was then quite remote according to any known rule or custom, this cannot be accepted in its obvious meaning. Perhaps Æthelwulf meant him to be an under-king. The official Roman account was that Alfred was invested with the marks of consular rank. He may have been confirmed by the Pope at the same time. However this may be, Alfred was at Rome in 853, and again, this time with his father, in 855. Æthelwulf's choice of a season when the Danes had been wintering in force at Sheppey to make a pilgrimage to Rome seems to do more credit to his piety than to his judgment.†

The incident, many times retold, which illustrates Alfred's early love of learning, seems to come between these two journeys; there is some mistake or confusion ‡ which prevents us from being sure of the date, but the tale can hardly be a fiction. Alfred's memory was good, and he was fond of getting English ballads by heart, but he had no regular lessons in his early youth, as indeed few

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* The difficulties arising from Alfred's mission to Rome as a young child, and from the apparent want of a suitable date for the circumstantial story of his learning to read, are well known to students of the period. They have led, on the one hand, to doubts as to the life by Asser being genuine. The text as we have it is certainly not in the best condition, and there may be dislocations as well as corruptions; but the objections to any hypothesis of forgery are greater than any relief that it would give. On the other hand, there is a strong temptation to suppose a mistake of seven years in the date of Alfred's birth. If he were born in 842, the incidents would fall in very well. The Bishop of Oxford felt the temptation some years ago (Pref. to William of Malmesbury's 'Gesta Regum,' ii. xli.), but resisted it; Sir James Ramsay ('Foundations of England,' i. 247) has yielded to it. Asser's existing text makes various inconsistent statements, and so far we might pick and choose. But the Parker MS. of the Chronicle—representing a statement probably authorised by Alfred himself—says that Alfred was twenty-three years old when he came to the throne, thus confirming the date given at the opening of Asser's narrative. The same reading is found in another early fragment. I do not see how this can be got over without suppositions which, as much as that of forgery, would be a remedy more violent than the disease.

† Sir James Ramsay, i. 234.

‡ Asser, or an early copyist, is more likely to have blundered in dates than to have spoken of Judith, Æthelwulf's second wife, who was hardly older than Alfred's elder brothers, as Alfred's mother. *Mater sua* does not mean stepmother in Latin of any age. Neither can we, in my opinion, believe that Osburh, Alfred's own mother, was repudiated: see below, p. 80.

laymen then had. One day his mother was showing her sons a book of English verse—a MS. probably containing only a few short poems, or even one—and offered the book as a gift to the boy who would first learn its contents. Alfred was attracted by the ornamented initial letter of the MS., and though the youngest, was the first to say, "Do you really mean that the book is for the one who can soonest understand and repeat it to you?" On being assured it was so, Alfred carried off the book to a teacher, learnt the poem—perhaps also learnt to read the words, but this is uncertain *—and could say it to his mother when he brought the book to her again. It is a pretty story, but tells us nothing about the later progress or extent of Alfred's learning. The many duties and distractions of the king's office left him but little time to pursue letters for himself, though he did much to make them accessible to others; much less, certainly, than he wished for. He preferred, it seems, to have some one to read to him if possible; but this may be intended only of Latin. † There is nothing, so far as I know, to show that he could write with ease, or wrote much. He may have been but little better as a penman than Charles the Great. He learnt Latin, probably from Asser, in the later and more settled part of his reign; but he cannot have known it like a trained clerk. Our present Sovereign Lady, acting quite in the spirit of her great ancestor, is said to have attained a competent mastery of Hindustani. But Latin, apart from the difference of alphabet, is a much harder language than Hindustani, and the Queen, though her life is busy enough, is not called upon to administer all her departments and command her frontier expeditions in person. Let us imagine what chance a modern Prime Minister would have of learning Arabic or Russian during his tenure of office. In the translations of Latin books which bear Alfred's name, the English was, no doubt, largely dictated by him; but, as I read his preface to the earliest of them, ‡ he did not trust himself alone with the Latin. He got the sense—a "construe," in fact—from his learned men, and then put it into such English as he chose himself. Even if his own knowledge could have sufficed for the whole of the translator's business, his leisure would not. We may believe that the king could follow the work of his bishops and priests with intelligence; but it would be absurd to think of him

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* *Magistrum adiit et legit* does not, in the Latin of the time, necessarily imply that the learner read the text himself. Or he may have learnt to follow those particular words as one learns to recognise a few words in a foreign tongue or character.

† Asser's statements are quite inconsistent with Alfred having ever been a perfect or ready scholar. Unluckily the passage which ought to contain the most decisive information is so corrupt as to give no certain sense. It tells us that Alfred was always sorry for not being able to do more, but it is mere guesswork whether the point of his grievance was never having had time to learn to read properly, or only not having enough time for reading.

‡ Gregory's 'Cura Pastoralis.'

as a scholar like Henry II., who could not only read but criticise Latin charters. On the other hand, the copious English additions, often characteristic, were beyond question made by Alfred's personal direction, and they may well be in his very words.*

These details make no difference whatever to the greatness of the man. It matters comparatively little whether Alfred knew more or less Latin, or recovered more or fewer square miles of territory in his lifetime. What does matter is that he rescued the very existence of English civilisation from imminent danger, that he left after his day an England in which learning could take firm root, and an English nation so knit together that when, only a few generations later, Danish kings did come to reign here, they had to reign and govern not as Danes but as Englishmen. It is hardly needful to add that a mere cloistered scholar could not have done Alfred's work. His campaigns are evidence enough that he could be a man of his hands; but we are expressly told that he was a great hunter.

On the way back from Rome, in the summer of 856, Alfred and his father spent some time at Worms at the court of the Frankish King Charles the Bald. Ever since Charles the Great's time there had been friendly relations between the Frankish court, the most polished † in Western Europe, so that this was quite a natural stage in Alfred's education. Here he saw the leading statesmen and scholars of the day, such as Grimbald of Old Saxony and John the Scot, the most brilliant of the early schoolmen and first in the line of illustrious Irish divines and philosophers. Grimbald certainly, John the Scot probably,‡ came to Alfred in England later. Æthelwulf took this occasion to marry as his second wife § Charles' daughter Judith, a girl only twelve years old. Historians have spoken harshly of her, forgetting, I think, that at this time and for several years afterwards she can have had no free choice of her own. Charles the Bald was an arbitrary father even according to mediæval notions of parental power. || When Judith did ultimately get a husband of her own choice, Baldwin of Flanders, she seems to have had no more adventures. Her son married a daughter of Alfred's, and was an ancestor of William the Conqueror's wife, and thus

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* The translation of Bede's 'Ecclesiastical History' is now ascertained to be Anglian, not West-Saxon, in its language. This appears to exclude, as to that work, Alfred's personal authorship.

† Such terms are, of course, relative as applied to the Dark Ages.

‡ See Mr. Poole's article "Scotus" in 'Dict. Nat. Biog.'

§ Æthelwulf was not, as sometimes represented, an old man; he was probably between forty and fifty. Some historians suppose that Alfred's mother Osburh must have been alive, and therefore that Æthelwulf repudiated her: see Freeman's article "Ælfred" in 'Dict. Nat. Biog.' As a conjectural remedy for chronological confusion, this seems much too desperate. The thing is not impossible, but it seems incredible, if it were so, that neither Asser nor any other chronicler should mention it.

|| Keary, 'Vikings,' p. 368.

Alfred is among the lineal ancestors of the kings of England since the Conquest. It would be curious to know how Alfred liked finding so young a stepmother crowned as his father's queen, but we have no information whatever about their personal relations. There is no doubt that Alfred's elder brothers did not like it at all. After Æthelwulf's death Judith married—to the scandal of all good Christian men, and according to the ancient custom of the heathen Saxons *—his eldest surviving son Æthelbald, whether willingly or not we do not know. She left England at his death in 860.

There must have been supposed grounds of policy for Æthelwulf's marriage with Judith, but the immediate effect was a risk of civil war at home, which Æthelwulf avoided only by subordinating himself to his son Æthelbald. This Æthelbald, seemingly a masterful and violent man, continued to reign over Wessex for two years after his father's death; he died in 860, and was succeeded by his brother Æthelberht, who had already been under-king of Kent. In Æthelberht's time the Danes took Winchester. The next brother, Æthelred, followed him as king in 866.

Alfred was now a man, and fit to take his part in war and counsel. The course of events had brought him very near the throne. Just at this time the Danes—men of Denmark, not merely Northmen—established themselves in East Anglia, and made themselves masters of Northumbria. In 868 Æthelred and Alfred helped the Mercians to drive an invading Danish host back upon its base at Nottingham. There the Danes could defy an enemy who knew nothing of fortification or siege works. Englishmen had—not for the last time—failed to follow the progress made in the art of war on the Continent. Peace was made, apparently without any terms of indemnity or security. In that same year Alfred married; we have no particular account of his wife, Ealhswith, but we know that she was the mother of wise and valiant children. Meanwhile, we read of famine and sickness in Wessex. Next year the Danes rode—for great part of them were now mounted—to York; the year after they were at Thetford and overcame and slew Edmund, King of the East Angles, whose death won him a long posthumous life of glory as a martyr and saint.† Another movable army made spoil of Croyland, Peterborough and Ely.

In 871 came the attack on Wessex, which Æthelred and Alfred must have been expecting. Early in the year the Danes seized the stronghold of Reading, sent out plundering parties, and fortified themselves by running an earthwork across from the Thames to the Kennet. A foraging detachment was defeated at Englefield, a few miles to the south-west. A few days afterwards Æthelred and

* Kemble, 'Saxons in England,' ii. 407.

† What was it that made St. Edmund a popular hero, demanding, so to speak, prompt beatification? One suspects something amounting to deliberate self-sacrifice, for what immediate purpose is unknown.

Alfred came up with the full muster of Wessex, and prepared to sit down before the Danish fort. They cut off or drove in any stragglers left in the open, but had no other success. A furious sortie of the Danes drove back the English after a hard-fought combat. Æthelwulf, the earldorman of Berkshire, who had won the little fight of a few days before, was among the slain.

The events of the next few days are obscure.* But, whatever had been happening in the meantime, after those days we find both sides apparently in something like equal force, and the English in good order, on the part of the Berkshire Downs called Ashdown, overlooking Wantage or perhaps Moulsford. The Danes, under two leaders of kingly rank and many earls, were on higher ground; they formed in two divisions, the kings leading one half † and the earls the other. The English made their dispositions to meet them with a similar front, Æthelred against the kings and Alfred against the earls. It was to be a battle of hand-to-hand shock, the shield-wall of either line backed by a dense mass of men. Standing still on lower ground to receive the enemy's charge was an obvious disadvantage. The heathen were moving, but King Æthelred was hearing mass in his tent, and would not let the office be interrupted. Alfred was at his post, and took upon himself to order a counter-attack by the whole English power,‡ without waiting for his brother. He led his men "as it were a wild boar," says Asser, perhaps echoing some song made in the camp. There was a stubborn fight, which raged mainly round a certain stunted thorn tree: the tree was shown to Asser when he visited the ground. The Danes gave way at last, and fled with heavy losses, a king and five earls, and an unknown number of lesser men; and the English pursued through the night and all the next day. What was left of the Danish host took refuge in the fort at Reading. Tradition preserved the memory of the fight as the greatest slaughter known since the Saxon invasion of Britain. Yet the power of the Danes was checked, not broken. Fighting went on all the year, once as far to the west as Wilton,§ and Asser counts eight battles in the year besides untold skirmishes and onfalls. When the English had the better, which they had not always, they still lost more than they could afford. The Danes had, no doubt, as many losses, or more;

* The earliest authorities give no details, and those given by later writers are improbable. If the English had retreated upon Windsor, they could surely not have been in force at Ashdown within four days. The battle was somewhere on the Ridgeway, but the spot cannot be fixed. Probably it was towards the eastern end. The White Horse proves nothing.

† Asser's "*mediam partem*" = "*dimidiam*," like Fr. *mi-*, It. *mezzo*.

‡ So I read Asser. There would be nothing so remarkable if he had moved only his own division. I am glad to learn that Mr. Oman, in his chapter on "Alfred as Warrior," contributed to the volume published by the King Alfred Committee, takes the same view. Asser ascribes the victory as much to Æthelred's piety as to Alfred's valour.

§ It is said that Alfred could not be at this battle, where the English were defeated.

but they were reinforced from oversea. Meanwhile, Æthelred had died soon after Eastertide—possibly of wounds, possibly of sickness contracted in the campaign, but we know nothing of the cause. Alfred reigned alone over the whole of the southern kingdom. Long recognised, it is said, as the ablest of Æthelwulf's sons, he now came to the height of opportunity, responsibility, and—as no distant time was to show—of trial.

His first act of policy was to make peace with the Danes, on the terms of Wessex being evacuated. We do not know whether money passed or not; it may be that these Danes already found, as their successors did some years later, that there were more hard knocks than shillings to be got from Alfred's men.* But I fear the probability is the other way.

Wessex was not attacked again for a few years. The Danes completed at leisure their work of ruining the northern and eastern counties, formerly the centre of Anglo-Saxon civilisation. In 872 their headquarters were at London. After being bought off by the Mercians for two years they subdued Mercia in 874. Leicester, Nottingham, Derby, Stamford, Lincoln—afterwards known as the Five Boroughs—were now Danish towns. In 875, starting from Repton, which had been their winter quarters, one column marched upon Northumbria, while another, with Guthrum—a name to be remembered—as one of their kings, made for Cambridge. From this time dates the Danish settlement in North-Eastern England, marked by the Scandinavian ending of place-names in *by*, and others as characteristic, though less frequent. Meanwhile Alfred had put some ships in fighting order, met six or seven heathen ships,† and took one of them.

In 876 the host from Cambridge gained the coast, it seems unobserved, and sailed round into the Channel and to Poole. Thence they established themselves at Wareham in Dorsetshire, in an almost impregnable position. But Alfred was soon on the spot in such force as to be able to treat on better terms than before. This time the Danes undertook to quit Wessex forthwith, gave hostages at Alfred's discretion, and swore by their most binding oath, the holy bracelet, as well as on Christian relics. But the treaty was never kept, for a large part of the army made a dash for Exeter, and wintered there, and sent out an expedition to Mercia the next summer.‡

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* Keary, 'Vikings,' p. 414. Neither Mr. Keary nor Sir James Ramsay doubts that Alfred had to pay the Danes this time.

† This squadron may or may not have belonged to the Danes in England, and may or may not have been acting in concert with the land army. It may well have come from Ireland. Asser's way of talking about seafaring matters reminds one of Munro's humorous vituperation of Lachmann as a Berliner landlubber on a question about Catullus's yacht. The good Welsh bishop meant well, but he does write like a landlubber—entirely suppressing the voyage round to Poole, for instance, in the next following incident.

‡ It is not clear whether this was downright perfidy or only sharp practice. Exeter might still seem West-Welsh rather than West-Saxon to those whose

In the course of 877 Alfred raised a fleet, and manned it with foreign adventurers, to stop the constant reinforcements which came to the Danes. At the same time he sat down before Exeter. A great Danish fleet, a hundred and twenty sail, weakened by storms and foul weather, fell in with Alfred's ships and was destroyed off Swanage. But such were the enemy's numbers on land that this had no general effect. The Danes left Exeter for Gloucester, and then seized Chippenham in Wiltshire by surprise, having made a forced march in mid-winter, soon after Christmas. Here they may have been reinforced from Mercia, now in complete subjection to them. About this time an independent expedition was completely routed on the North Devon coast; the place is supposed to be Kenwith, near Bideford. This victory, again, though to all seeming brilliant, was merely local. The main army of the heathen was in as great strength as ever, and met with no serious resistance inland. Apparently the English power in the heart of Wessex was exhausted for the time. This collapse after a campaign in which the English on the whole had the best of it remains somewhat obscure; but there is no doubt that it was so.

Whatever the exact course of events may have been, the Danes were masters of the best part of Wessex in the early part of 878. Alfred was driven to abandon open war for a season, and fall back with a small personal following into the marshes of Somersetshire. To this time belongs the famous tale of Alfred burning the loaves in the neatherd's cottage. It does not rest on the best authority, but there is nothing incredible in it, and there is no obvious motive for invention. It does not require us to suppose that Alfred was in hiding, or flying for his life; only that he was for a short time alone in a house where the goodwife did not know him by sight. This might be accounted for in various ways—a surprise visit to outposts, for example. But the story of the king going to the Danish camp disguised as a harper is absurd. In the genuine account, as Freeman well says, there "is no forsaking and no hiding; Ælfred is reduced to extreme distress, but he never lays down his arms." After Easter, however, Alfred entrenched himself at Athelney, an eyot (as the name denotes) in the fenland at the junction of the Parret and the Tone. Here the well-known "Alfred jewel," now in the Ashmolean Museum at Oxford, was found; but perhaps it is due to Alfred's later establishment of a monastery, of which we have not room to speak, rather than to his encampment. In May the king was able to summon the levies of Somerset, Wilts and Hampshire—these last a mere remnant—to meet him on the eastern border of Selwood Forest, near Warminster. Within three days the English had occupied the

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interest it was to think so. The whole story of these two years is meagre and confused. Asser seems to say that there was a treacherous slaughter at Wareham.

hills above Ethandun, now called Heddington,* on the downs towards Chippenham, there faced the Danes, completely defeated them, and shut them up in their entrenched camp. After being beleaguered a fortnight, the Danes, cut off from all help of their kinsfolk, and pressed by cold and hunger, came to terms. At last the terms give clear evidence of an English victory. Not only the Danes were to retire to their possessions in East Anglia—for there was no chance of reconquest in that quarter—but Guthrum, their king, and his followers were to receive baptism. That is, they pledged themselves to live side by side with the English as peaceable and law-abiding neighbours. It was as if in India, nine centuries later, a Mahratta horde, after a series of battles with the Mogul power, should have submitted to become Moslems. The treaty was not a surrender to Alfred on the Danish part, but it was a frank recognition that they could not deal with Wessex as they had dealt with Mercia. Nay, more, a good part of Mercia itself was won back. The boundary between the English land and that of the Danes, the "Danelaw," as it was afterwards called, was drawn up the Thames, up the Lea from its junction with the Thames, then to Bedford, then up the Ouse to Watling Street (the great Roman road), by Watling Street to Chester.† English and Danish men of corresponding rank were to be counted of equal worth for the purposes of wergild and compensations.

This time the covenant was well kept. Guthrum and thirty of his chief men duly came to be baptised, and he took the name of Æthelstan. The ceremony of "chrism-loosing" and the attendant festivities were completed at Wedmore. Still it was two years before the Danes were fairly back in East Anglia. It was no longer the march of a flying column, but a deliberate migration of settlers, carried out with only such delays as were natural. At length it was all done, and men could now, for the first time for many years, honestly say that there was good peace in Wessex.

Not that Alfred had done with wars. More fighting was to come in later years, even hard fighting, and at least one breach of the treaty of Wedmore. But these were no longer fights for the life of the kingdom. That was assured when the East Anglian Danes became a defined and recognised State, owning that the king of the West-Saxons wielded, in some sort, a paramount power. I purposely use vague terms; there could be no talk in England, at this time, of definite feudal relations or commendation; nor is it clear that Alfred could have enforced any formal submission. The later campaigns of Alfred's reign were not critical; they are interesting partly as

* I take it as the more likely view that Alfred seized a commanding position, and forced the Danes to attack at a disadvantage. Details are wholly wanting: as to the identification of the place, see the note at the end of this paper.

† The text we have represents a later confirmation. It is possible that the terms of 878 were not so favourable to the English. Green, 'Conquest of England,' 151.

showing how much he had improved his military dispositions, partly as indications of the still unabated power of the Northmen on the Continent. These renewed incursions were really in the nature of overflows to the English coast from far greater storm-waves, the last of the plundering and desolating raids of the old Viking type, which now had their centres in Northern France and the Rhineland. So far as the present sketch is concerned, these episodes may be passed over with a rapid survey.

In 884 a division from the Danish host in France besieged Rochester, but was driven off, leaving many slaves and horses; they had brought their own horses from the mainland. They must have received aid from East Anglia, for Alfred sent a fleet to make reprisals on the east coast; his sailors captured thirteen Danish ships at the mouth of the Stour (that is, hard by the modern Harwich),* but were afterwards beaten by a fresh squadron. On the whole, Alfred was able to reassert his supremacy, as we shall presently see. In 892 the Vikings, who had been signally defeated in Flanders, turned to England; one fleet fell on the south and made Appledore their quarters,† another went up the Thames under Hæsten or Hasting, a renowned freebooter, and held Milton, near Sheppey. Now followed three seasons of fighting up and down the country; but this time Alfred commanded the powers not only of Wessex, but of Mercia outside the Danelaw, and even a North Welsh contingent joined him. Alfred's son, Edward, afterwards his worthy successor, won his spurs (to use the phrase of chivalry before its due time) by checking the host from Appledore at Farnham in Surrey. Then the East Anglian Danes became involved in the strife; a Northumbrian fleet came against Exeter; there were many fights and a leaguer on the Severn. We hear of Hasting after this at Chester,‡ in Wales and on the Lea. In 896 both sides had suffered much, but the Danes more. They found that on the whole nothing was to be gained in England, and dispersed. Some of them remained as settlers in the Danelaw, while others sought adventures in France. Nothing is recorded of any formal peace-making; probably there was no permanent ruler to make a treaty with. Meanwhile, Alfred had built ships of a new design, and larger than the Danish galleys, to keep the peace of the southern coast. Their power was gained at the expense of handiness, at least it seems so from the only account we have of their behaviour

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* Assuming that the East Anglian and not the Kentish Stour is meant by the chronicler, which seems on the whole the more likely view.

† This Appledore lies in Kent to the west of Romney Marsh, not far from the borders of Sussex.

‡ The details are to be elucidated, if at all, only by a special student of the mediæval art of war; and I collect from Mr. Oman's essay, already mentioned, that he too has found them obscure. The chronicles expect us to believe that Hasting made his way across from the Severn Valley to his ships in Essex, and then back again across Mercia to Chester. For the dates I follow the received correction of the chronicle years.

in action, when the king's new ships grounded with the ebb tide and could not get off with the flood in time to pursue the lighter and nimbler Danes. Two Danish vessels, however, were cast ashore and their crews sent to Alfred. He had them hanged as pirates, which, no doubt, they were. We may assume that all pretence of regular warfare was at an end, even if these rovers had been under any command at all. This is the only act of severity recorded of Alfred in the whole of his reign, and it appears to have been justified both in strict right and in policy.

The vastly increased efficiency of Alfred's forces in these latter campaigns is our best measure of his military reforms. Our direct accounts of them are not so clear as might be desired. But this much is certain, that he found mere tribal levies, which could be kept together only for a short time, and were useless for distant or prolonged operations, and left a system in which there were distinct provisions for a field army, garrisons and reserve. His personal staff and retinue (in which military and civil functions were, no doubt, combined) were also divided into three sections, which took the duty in turns, month by month.*

We now return to a memorable deed of Alfred's, which we passed over in its order of time rather than interpolate it among purely military incidents. London, it will be remembered, had been plundered and wasted by the Danes in the middle of the century. In 886, the year in which Paris was besieged and nearly taken by the Vikings, and they departed at last rather as victors than as vanquished, Alfred, now free for works of peace, turned his thoughts to London. "He restored it with all honour, and caused men to dwell therein, and gave it in charge to his son-in-law, Æthelred, Earl of Mercia; and to him as their king all the Angles and Saxons who had been scattered abroad, or had been led captive by the heathen, freely betook themselves and put themselves under his lordship"; † that is, the scattered English of the northern and eastern parts came and settled in London, now sure of Alfred's protection. Alfred could have no conception of what London was to be even in later mediæval times. None the less this was a master-stroke of policy. London, the first of Mercian cities, thus restored to her old estate, was a sign for all men of the new power of Wessex, a bulwark of Mercia, and a sure warden of the Thames valley against any future Danish invasions. Next after Winchester, London ought of right to honour Alfred as her second and greatest founder. This must have been very soon after the time when the Treaty of Wedmore, perhaps with some

* Chron. s.a. 894; Asser, p. 65, ed. Wise. There is an extremely obscure document, now known to scholars as the 'Tribal Hidage,' which may possibly have to do with a military census of Alfred's time. The suggestion that this is its real significance comes from my friend Prof. F. York Powell.

† Asser, *sub anno*. The incongruous Roman *dominium*—for the language aims at being classical—makes one feel the misfortune of Asser having written in Latin.

revision, was finally put on record.* Within six years events proved Alfred's wisdom. In 893 the Danes had a camp where Westminster now stands, but were kept in check by Æthelred and the garrison of London. In 894–5 Hasting was on the Lea, and held out against the men of London till Alfred came in person and baffled the Danes by diverting the course of the stream below their camp, and so cutting off their communications. But the old pirate had failed to gain any ground, whereas, if things had been as they were twenty years before, he might have worked his will far up the river. Alfred took care in other ways that good witness should not be lacking to the restoration of English power in Mercia. He put in exercise there an ancient and eminent attribute of sovereignty. We have coins of Alfred's bearing the name of Oxford, then a Mercian town. At this time of day one need hardly repeat that this is the only authentic connection of his name with Oxford. The story that he founded the University, or schools of any kind, at Oxford is a late and gross fiction, which it would be too polite to call a legend ; in its developed form it cannot boast even mediæval antiquity.

To return to Alfred's improvements, it is more pardonable to speak of him as the founder, or one of the founders, of the English navy than as the founder of a university. But here (while we rejoice that the Admiralty has decided to name a first-class cruiser the "King Alfred") we must beware of exaggeration. I do not mean merely that Alfred's ships were singly and collectively inferior to those of the smallest modern navy. They were as good as he could make them then, and it is quite possible that the warships of a century hence may be as superior to ours as one of Nelson's frigates to the vessels of an Anglo-Saxon or Danish flotilla. Such comparisons are of no historical value. It might be more useful to consider how little the art of war on land had improved (if it had not gone back) since Agricola commanded in Britain. Probably one of his legions was more formidable in every way than the whole muster of Christian and heathen men who fought at Ashdown. As to naval matters, what really has to be said is that Alfred had not, for aught that appears, nor can we see how he could have had, any conception of what we now mean by the command of the sea. Within two centuries William the Conqueror landed his army without opposition of any kind. One such fact is conclusive to show that, even if we could suppose Alfred to have had some inkling of a real naval policy, he did not succeed in leaving any sound doctrine on the subject after his own day. The importance of sea power to England did not begin to be realised till the time of Elizabeth, and it has been strangely possible to forget it even within living memory. The Anglo-Saxon and mediæval plan of a navy was merely to keep the narrow seas

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* Green, 'Conquest of England,' 150 (but the mention of a "war of 886' is an obvious slip ; there was no such war in England); Ramsay, 'Foundations of England,' i. 255.

against freebooters, so that trade might be tolerably safe in time of peace. As Chaucer says of his Merchant:

> "He wolde the see were kept for any thing
> Bitwixe Middleburgh and Orewell."

It was to be a long time before Sir Walter Raleigh made his magnificent and wholly justified boast that the Invincible Armada of Spain had not burnt so much as one sheepcote of this land; and longer still before we ourselves were to learn the secret of England's greatness over again, not from an Englishman born, but from one of our kin beyond the sea—Captain Mahan, of the United States Navy.

Great reformers hardly ever find their work run smooth, and we know that Alfred did not. Incompetence, jealousy, local and personal, self-seeking, and—perhaps worst of all—the complacent inertness of honest but stupid men who think what was good enough for their fathers good enough for them, had to be reckoned with then as now. Bishops, ealdormen, king's thanes, and sheriffs, even the best of them, had to be taught their duty, lectured, ordered about, rebuked, in the last resort punished. The King wore himself out in well-doing, and after all could not get many of his plans executed. Forts designed by him were never built, or were not finished in time, and those who had been in fault lamented too late that the Danes had taken their wives and kindred captives, harried their land, and spoiled their goods.* Bismarck is a far less noble and dignified figure than Alfred, though he wrought on a greater scale; but the picture now fresh before us of Bismarck striving for the union of Germany, and fretting under the pretensions of absurd princelets and the pedantries of shallow politicians, will help us to realise Alfred's troubles. If Alfred found a helper after his own heart it was his son-in-law, Æthelred of Mercia, whose wife Æthelflæd, the lady of the Mercians as she came to be called, stands out as the most brilliant and heroic woman in our early history.

Justice was among the first of Alfred's cares when there was peace in the land. Here, too, the difficulties were immense. An Anglo-Saxon county or hundred court must have been more like a disorderly public meeting than a modern court of any kind; there was no security for any one being learned, or knowing how to conduct business, unless the bishop was present; and there were no effectual means of putting judgments in force.† The king and his Witan could set an example: when the ordinary methods, cumbrous and slow as they were, had been exhausted, the king could at need compel an obstinate wrongdoer to submit; but the king's wise men were not a court of appeal. Indeed, there was no such thing as

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* Asser, ed. Wise, pp. 59, 60.

† In the county courts of the 13th century, as we learn from Bracton, there were certain notables who practically controlled the proceedings. This may have been so from a much earlier time; we do not know that it was.

appeal from a final judgment in the modern sense. Alfred informed himself how justice was administered; he could require his officers to give an account of the proceedings, and put pressure on them, by the threat of dismissal, to qualify themselves for their duties and redeem themselves from the too common reproach of being quite illiterate. Many of them set to work to learn to read, or at least get by heart, the rules recorded in the king's and his ancestors' dooms. But the time for direct over-riding royal interference, for the establishment of new courts and new procedure, was not yet. Alfred could not do the work of Henry II. or Edward I.* There is a later legend to the effect that Alfred was not swift enough to restrain the oppressions of his great men in the early part of his reign, and that his reverses were a punishment for this fault.† A likely time, indeed, for judicial reform, with the Danes holding Mercia and barely out of Wessex, and the local magnates chafing under the most obviously needful measures taken to strengthen the king's authority, and lessen the evils of divided command! But no twelfth-century chronicler could be happy without a moral.

King Alfred's laws are extant, the 'dooms' which were set before his Witan and approved by them towards the end of his reign. Anglo-Saxon law is a technical and thorny subject, and most of its actual contents are extremely remote from anything that is now administered as law in England, though the spirit of publicity and fairness, rough as were its forms in early times, is not.‡ Enough to say that Alfred, as a good son of the Church, prefixed to his dooms considerable extracts from the Book of Exodus—it is conceived as an edifying specimen of "written reason" rather than as rules to be actually followed by Englishmen. When he comes to practical business he consolidates and amends, as we should now say, the customs recorded in various earlier dooms of West-Saxon, Kentish, and Mercian kings, and annexes a revised version of the laws of his ancestor, Ine of Wessex. There is a humane provision for securing a certain number of holidays to hired labourers, which we may well believe to be Alfred's own. His modest and sensible statement of his policy has been often quoted, but must be quoted again:—

"Now I, King Alfred, gathered all this together and bade write it down: much of the dooms that our forerunners held by, such as

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* Asser, ed. Wise, pp. 69, 72, at the end of the Life. These rhetorical comments of a Welshman writing Latin about English customs which he probably did not more than half understand are confused, and to a lawyer exasperating. But real facts must underlie them. After some doubt, I think the passage genuine. A later addition would have been fuller and would have ascribed more power to the king.

† In the interpolation from the St. Neots Chronicle. Asser, ed. Wise, p. 31.

‡ See more in an article of mine on "English Law before the Norman Conquest." 'Law Quart. Rev.,' July 1898. A critical text of the Anglo-Saxon laws is being edited by Dr. F. Liebermann, of Berlin, for the Savigny-Stiftung: the first part (Halle a. S., Max Niemeyer, 1898) comes down to Eadmund's time, and therefore includes the whole of Alfred's and Ine's dooms.

liked me well; and many of those which misliked me I put away with my Witan's advice, and bade men observe them in other manner. For I dared not put much of my own in writing, for it was unknown to me how much of that would be to the liking of those who came after us."

Later stories which ascribe the invention of modern legal institutions to Alfred—including trial by jury, which is distinctly of Anglo-Norman and not Anglo-Saxon origin—are merely fictitious.

Alfred's love of learning and encouragement of letters and research have been more fully and more often described than any other part of his work. We see his court at Winchester frequented by clerks, nobles, and travellers of all nations, including those very Danes who had been fighting him a few years before. So in our own time the chiefs of wild frontier tribes throng to the durbar of a strong and popular political officer on the north-west marches of India. We can almost hear Ohthere, who dwelt northmost of all the Northmen, telling his adventures in pursuit of the horse-whales (walruses), who have right goodly bones in their teeth fit to bring to a king—yes, such as this which he offers his lord Alfred for a token—and Alfred bidding Plegmund the Mercian, or John the Monk of Old Saxony, set down the tale and work it into the English translation of Orosius' universal history. We may note Alfred economising time, measuring his hours by four-hour candles of standard weight, and guarding them in horn lanterns from the draughts that ranged as they listed in the ninth-century palace. We see him investing his grandson, Æthelstan, the future victor in the great fight at Brunanburh, with the weapons and garb of a man of war. We catch him walking with Asser, the Welshman invited from the uttermost west of Britain to be his secretary, learning what Latin he can from him, and delighting his teacher with a proposal to keep a note-book—(what would we give now for that note-book?).* And with all this the king is still a sportsman, and looks with a master's eye to his hawks and his kennel. Strangest of all, this man of boundless and yet ordered activities has borne up from his youth against a mysterious and harassing sickness—according to modern conjecture, some form of epilepsy—from which Asser tries to extract edifying reflections. But these things, as I said, are familiar.

Alfred enjoyed some years of peace before the end of his reign. He died, according to the common reckoning, in the autumn of 901, but it seems really in 900 † or 899 ‡; by no means an old man as we think of statesmen nowadays, but having done enough to fill a long life. Is this all? No, not even for the immediate future. The

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* Asser makes a far-fetched and not over-courtly comparison of the king to the penitent thief, as a late enterer into the kingdom by good will rather than works, and apologises for it in the next paragraph.

† Ramsay, 'Foundations of England,' i. 267.

‡ W. H. Stevenson in 'Athenæum,' July and August, 1898. The choice turns on points too minute to discuss here, but 901 is wrong in any case.

king had lived to see his son Edward a warrior, and his grandson Æthelstan a promising boy. Right well they both followed in Alfred's path as just and valiant kings, Edward in alliance with his no less valiant sister Æthelflæd. Glorious among the women of our race, the Lady of the Mercians drove back the Dane step by step for eighteen years more. Tamworth, Stafford, and Warwick are her work; Derby and Leicester were her conquests. Alfred and Ealhswith might well be proud of their children.

The English kingdom might not last, indeed, in such manner and form as Alfred established it. The Anglo-Saxon polity bore in it the seeds of decay. Danish conquest—but not heathen—was to come only a century after the great king's death; Norman conquest—which may be called Danish at one remove—after that. English was transformed with travail and violence, but, in the long run, for the better. Alfred's work also was transformed, but never broken. It lives still in his old England; it lives and waxes in the growth of new English commonwealths round the world.

NOTE *on Ethandun* (*p.* 85 *above*).—The opinion represented in the text, namely that the Danish head-quarters were still at Chippenham when Alfred marched from Selwood Forest, is that of almost all recent writers. On this assumption, Heddington (as it is now written), almost due south of Calne, not the Edington also in Wiltshire, seems the likeliest representative of Ethandun. But there is another Edington in quite another direction, at the foot of the Polden Hill, near Bridgwater. The late Bishop Clifford, of Clifton, maintained in 1875 ('Proceedings of Somersetshire Archæological and Natural History Society,' xxi. 1), that this Edington of Somerset is the real Ethandun, taking a quite different view of the campaign as a whole. His points, briefly summarised, are to this effect.

1. The Danish land army was acting in concert with the fleet commanded by Ubbo or Hubba. The fleet landed at the mouth of the Parret; Asser's "Cynwit" is the modern Combwich. Anything south of the Parret might then be called Devon. Guthrum's army from Chippenham joined these invaders.

2. Alfred watched the Danes from Athelney, and by feints and skirmishes led them to believe that he was collecting his strength on the left bank of the Parret, while he was really doing so on the far side of Selwood, beyond the Danish means of observation.

3. From Egbert's Stone (somewhere among the Brixtons, *qu.* Whit-Street-Castle?) Alfred and his host doubled back upon Polden Hill, and succeeded in capturing the key of the Danish position.

4. The fort to which the Danes fled was not Chippenham, but probably Bridgwater. If it was at or near Chippenham, why did not relief come to the Danes from Mercia?

5. As a minor point, Æcglea or Iglea, the place of Alfred's halt on the march from Egbert's Stone to Ethandun, is identified with Edgarlea, formerly Egerly, a hamlet of Glastonbury close under Glastonbury Tor. The distances are about right, and there is no other plausible identification of the name.

This theory is worked out with great ingenuity—an ingenuity which is excessive in trying to fix definite interpretations on the language, not only of Asser and the English Chronicle, but of later writers who probably knew nothing of the country, and very little of war, and were quite indifferent to topographical accuracy. I am informed that some competent students, with opportunities of examining the ground, are convinced that Bishop Clifford was right, but I am not acquainted with any published criticism of his hypothesis, favourable or unfavourable, and it has certainly not found any general acceptance; whether

because it has been considered and rejected, or because it has remained unknown, I am not able to say. The broad and obvious objection to it is that it attributes an improbable amount of skill in strategical combination to both parties at a time when the Roman art of war was forgotten in Western Europe, and the mediæval art of war was in its first infancy. The truth is that we have to deal with hopelessly uncritical and unmilitary narratives, written by men who not only had not our modern apparatus of maps, gazetteers, and so forth, but cared for none of these things, and were chiefly intent on edification, rhetoric, or personal anecdote; and we cannot even assume that chroniclers not known to have been familiar with the places mentioned had endeavoured to form any clear notion of their situation or relative distances and bearings. If there was a genuine local tradition it is long since lost; and, as we are reduced to guess-work, the simplest guess consistent with such facts as we have appears to be the safest. On the whole I do not feel justified in departing from the received account, though it is by no means free from difficulty.

Another possible view of the earlier part of this campaign is that the main body of the Danes who seized Chippenham came not from Exeter but from Mercia, where they had spent the first half of the winter season.

[F. P.]

GENERAL MONTHLY MEETING,

Monday, March 6, 1899.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

W. Bruce Bannerman, Esq. F.G.S. F.R.G.S.
Arthur Bellin, Esq. F.R.G.S.
Lady Currie,
Charles Henry Gatty, Esq. LL.D. F.R.S.E. F.L.S.
Arthur Christian Gibbons, Esq. B.A.
Edward James Gibbons, Esq. M.A.
Mrs. J. E. Horne,
Mrs. Edward Kraftmeier,
Sir George Henry Lewis,
Mrs. John List,
Frederick James Longton, Esq.
Oswell S. Macleay, Esq.
Mrs. Parker,
Herbert Pulford, Esq. M.A. M.B.
Colonel Euston Sartorius, V.C. C.B.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Sir Andrew Noble, K.C.B. for his donation of £100, and to Mr. Edward Kraftmeier for his donation of £52 10*s*., to the Fund for the Promotion of Experimental Research at Low Temperatures.

His Grace The Duke of Northumberland was elected President of the Royal Institution in the place of the late Duke his father.

The following letter from the Duke of Northumberland was read from the Chair:—

"Dear Sir Frederick Bramwell, "ALNWICK CASTLE, 16*th February*, 1899.

"I am deeply touched by the Resolution adopted by the Members of the Royal Institution which you have been so good as to forward to me. The appreciation exhibited by it of the late Duke of Northumberland's services to the Institution, and the kindly sympathy expressed for me and for my family in the heavy bereavement we have sustained, have afforded me great gratification, more especially because I know how highly my father valued the work of the Institution, and his connection with it as its President.

I am, dear Sir Frederick,

The Honorary Secretary, Yours truly,

Royal Institution. (Signed) NORTHUMBERLAND."

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—A Manual of the Geology of India, 2nd ed. Part 1, Corundum. By T. H. Holland. 8vo. 1898.
Palæontologia Indica: Ser. XV. Himalayan Fossils, Vol. I. Part 3. 4to. 1897.
Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. 1° Semestre, Vol. VIII. Fasc. 2, 3. 8vo. 1899.
American Academy of Arts and Sciences—Proceedings, Vol. XXXIV. Nos. 2–5. 8vo. 1898.
American Association—Proceedings, Forty-seventh Meeting at Boston. 8vo. 1898.
Astronomical Society, Royal—Monthly Notices, Vol. LIX. No. 3. 8vo. 1899.
Bankers, Institute of—Journal, Vol. XX. Part 2. 8vo. 1899.
Berlin, Royal Prussian Academy of Sciences—Sitzungsberichte, 1898, Nos. 40–54. 8vo. 1898.
Bion, W. A. Esq. (the Author)—Notes on the Meteorology of Vizagapatam, Part 1. Rainfall. 8vo. 1898.
Birmingham and Midland Institute—Report for 1898. 8vo.
Birt, Sir William—On the Broads. By A. B. Dodd. 8vo. 1896.
Selections from "Sisters by the Sea." By C. Scott. 16mo. 1898.
Boston Public Library—Annual List of Books added to Library, 1897–98. 8vo. 1899.
Monthly Bulletin, Vol. IV. Nos. 1, 2. 8vo. 1899.
British Architects, Royal Institute of—Journal, 3rd Series, Vol. VI. Nos. 7, 8. 4to. 1899.
British Association—Report for 1898. 8vo. 1899.
British Astronomical Association—Memoirs, Vol. VII. Parts 2, 3. 8vo. 1899.
Journal, Vol. IX. No. 4. 8vo. 1899.
Buenos Aires, Museo Nacional—Comunicaciones, Tomo I. No. 2. 8vo. 1898.
Camera Club—Journal for Feb. 1899. 8vo.
Chemical Industry, Society of—Journal, Vol. XVIII. No. 1. 8vo. 1899.
Chemical Society—Journal for Feb. 1899. 8vo.
Proceedings, Nos. 203, 204. 8vo. 1899.
Collective Index to Transactions, Proceedings and Abstracts, 1883–92. 8vo. 1898.
Cracovie, l'Académie des Sciences—Bulletin International, 1899, No. 1. 8vo.
Dax, Société de Borda—Bulletin, 1898, Troisième Trimestre. 8vo. 1898.
Editors—American Journal of Science for Feb. 1899. 8vo.
Analyst for Feb. 1899. 8vo.
Anthony's Photographic Bulletin for Feb. 1899. 8vo.
Athenæum for Feb. 1899. 4to.

Editors—continued.
Author for Feb. 1899. 8vo.
Aeronautical Journal for Feb. 1899. 8vo.
Astrophysical Journal for Jan. 1899.
Bimetallist for Feb. 1899. 8vo.
Brewers' Journal for Feb. 1899. 8vo.
Chemical News for Feb. 1899. 4to.
Chemist and Druggist for Feb. 1899. 8vo.
Education for Feb. 1899.
Electrical Engineer for Feb. 1899. fol.
Electrical Engineering for Feb. 1899. 8vo.
Electrical Review for Feb. 1899. 8vo.
Electricity for Feb. 1899. 8vo.
Engineer for Feb. 1899. fol.
Engineering for Feb. 1899. fol.
Homœopathic Review for Feb. 1899. 8vo.
Horological Journal for Feb. 1899. 8vo.
Industries and Iron for Feb. 1899. fol.
Invention for Feb. 1899.
Journal of Physical Chemistry for Jan. 1899. 8vo.
Journal of State Medicine for Feb. 1899. 8vo.
Law Journal for Feb. 1899. 8vo.
Life-Boat Journal for Feb. 1899.
Lightning for Feb. 1899. 8vo.
Machinery Market for Feb. 1899. 8vo.
Nature for Feb. 1899. 4to.
New Church Magazine for Feb. 1899. 8vo.
Nuovo Cimento for Nov. and Dec. 1898. 8vo.
Photographic News for Feb. 1899. 8vo.
Physical Review for Feb. 1899. 8vo.
Public Health Engineer for Feb. 1899. 8vo.
Science Abstracts, Vol. II. Part 1. 8vo. 1899.
Science of Man for Dec. 1898. 8vo.
Science Siftings for Feb. 1899.
Travel for Feb. 1899. 8vo.
Tropical Agriculturist for Feb. 1899.
Zoophilist for Feb. 1899. 4to.

Emerson, P. H. Esq. (the Author)—Marsh Leaves from the Norfolk Broad Land. 8vo. 1898.

Florence, Biblioteca Nazionale Centrale—Bolletino, Nos. 314, 315. 8vo. 1899.

Forbes, J. Staats, Esq. M.R.I.—The National Telephone Company, Ltd. (Reprint of Articles in 'Times,' Jan. 1899). 8vo.

Franklin Institute—Journal for Feb. 1899. 8vo.

Geographical Society, Royal—Geographical Journal for Feb. 1899. 8vo.

Geological Society—Quarterly Journal, No. 217. 8vo. 1899.

Harlem, Société Hollandaise des Sciences—Archives Néerlandaises, Série II. Tome II. Liv. 2–4. 4to. 1899.

Horniman, F. J. Esq. M.P. M.R.I.—Eighth Annual Report of the Horniman Museum, 1898. 8vo.

Imperial Institute—Imperial Institute Journal for Feb. 1899.

Japan, College of Science of the Imperial University of Tokyo—Journal, Vol. IX. Part 3; Vol. X. Part 3; Vol. XI. Part 1; Vol. XII. Parts 1–3. 8vo. 1898.

Johns Hopkins University—American Chemical Journal for Feb. 1899. 8vo.

Manchester Museum, Owens College—Publications, No. 24. 8vo. 1899.

Massachusetts Institute of Technology—Technology Quarterly for Dec. 1898. 8vo.

Mechanical Engineers, Institution of—Proceedings, 1898, No. 3. 8vo.

Meux, Lady—The Lives of Mabâ' Sĕyôn and Gabra Krĕstôs. The Ethiopic texts edited, with an English translation, by E. A. W. Budge. 4to. 1898. (Printed for Private Circulation.)

Microscopical Society, Royal—Journal, 1899, Part 1. 8vo.
Mitchell & Co. Messrs.—Newspaper Press Directory, 1899. 8vo.
Munich, Royal Bavarian Academy of Sciences—Sitzungsberichte, 1898, Heft 4. 8vo.
Navy League—Navy League Journal for Feb. 1899. 8vo.
Paris, Société Française de Physique—Bulletin, No. 127. 8vo. 1899.
Pharmaceutical Society of Great Britain—Journal for Feb. 1899. 8vo.
Photographic Society, Royal—Photographic Journal for Jan. 1899. 8vo.
List of Members, 1899. 8vo.
Rochechouart, La Société les Amis des Sciences et Arts—Bulletin, Tome VIII. No. 3. 8vo. 1898.
Rome, Ministry of Public Works—Giornale del Genio Civile, 1898, Fasc. 10°, 11°. 8vo. And Designi. fol.
Royal Engineers, Corps of—Professional Papers. Vol. XXIV. 8vo. 1898.
Royal Society of London—Proceedings, Nos. 407, 408. 8vo. 1899.
St. Bartholomew's Hospital—Reports, Vol. XXXIV. 8vo. 1899.
Saxon Society of Sciences, Royal—
Mathematisch-Physische Classe—
Berichte, 1898, No. 1. 8vo.
Abhandlungen, Band XXIV. No. 6. 8vo. 1899.
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WEEKLY EVENING MEETING,

Friday, March 10, 1899.

The Hon. Sir James Stirling, M.A. LL.D., Vice-President, in the Chair.

Professor H. L. Callendar, M.A. F.R.S.

Measuring Extreme Temperatures.

The measurement of extreme temperatures is a subject of great theoretical interest, especially in connection with the determination of the laws of radiation and of chemical dissociation and combination. The temperature in each case is the factor of paramount importance, and without means of measuring the temperature there is no possibility of formulating any rational theories. The subject possesses, in addition, a powerful fascination for the experimentalist, on account of the difficulty of the observations involved, and of the extremely conflicting nature of the results obtained by different observers and different methods of research.

Attempts have frequently been made to estimate the temperatures of the electric arc and of the sun, which may be taken as examples of the most extreme temperatures known to science, and afford an illustration of the difficulties to be encountered, and of the methods available for attacking these problems. A brief consideration and comparison of the results will also serve to explain the causes of the remarkable discrepancies existing in the estimates of such temperatures by different observers and different methods.

In the case of the sun it is at once obvious that no terrestrial thermometer can possibly be directly applied. The only available method is (1) to measure the intensity of the solar radiation, and (2) to endeavour to deduce the temperature by determining the law of radiation at high temperatures. The measurement of the intensity of the solar radiation is in itself a sufficiently intricate problem, containing many elements of doubt and difficulty; but by far the greatest source of uncertainty lies in the solution of the second part of the investigation, the determination of the law of radiation. The origin of the discrepancies thus imported into the results may be summed up in the word "Extrapolation."

The method of investigation necessarily consists in taking a series of observations at temperatures within the laboratory range of thermometry, from which to calculate an empirical formula representing as closely as possible the results of experiment. It is then assumed that the formula may be "extrapolated," or used to estimate the temperature of a radiating source of known intensity *beyond the range* of the observations on which it was founded. This is a perfectly

justifiable method, and may lead to very good results if the empirical law happens to be correct; but if the formula happens to be unsuitable, it may lead to the most remarkable conclusions.

The curves shown in Fig. 1 illustrate some of the typical formulæ which have either been proposed for the law of radiation, or been

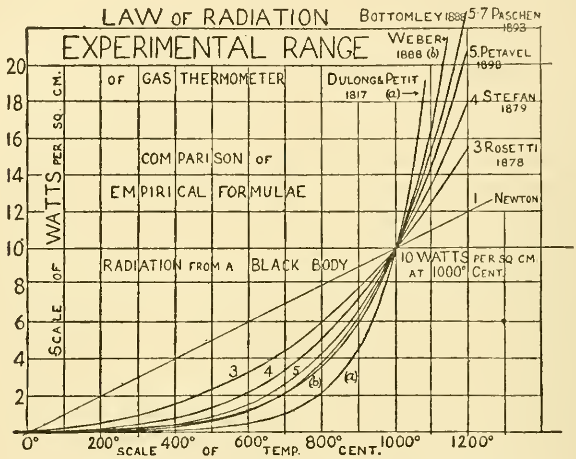

FIG. 1.—Formulæ of Radiation. Experimental range.

deduced from the results of modern experiments over the experimental range of the gas-thermometer, extending to 1200° C., to which trustworthy determinations of temperature on the theoretical scale are at present restricted. In order to obtain a comparison of the formulæ themselves, apart from other issues, the results of different observers are reduced to a common hypothetical value, 10 watts per square centimetre, for the radiation from a black body at 1000° C.

Excluding the law of Newton, which applies only to small differences of temperature, and also the law of Dulong and Petit, which was founded on observations over a very limited range with mercury thermometers, and is obviously inapplicable at high temperatures, there is a certain family resemblance between the remaining curves; but the differences between them are still so considerable that, if sufficiently accurate measurements of temperature were available, it should be possible to decide with certainty which of the formulæ was the most correct. A fairly close agreement is seen to obtain between the formula proposed by Weber and the curves representing the results of the recent experiments of Bottomley, Paschen and Petavel. But, on the other hand, there is strong evidence, both experimental and theoretical, in favour of the fourth power law proposed by Stefan, which differs materially from that of Weber; and many supporters may be found, especially among astronomers, for the very different formula of Rosetti.

The importance of choosing a correct formula is most easily realised by reference to Fig. 2, which represents the results of extrapolation as applied to deducing the probable temperature of the sun. On the scale of Fig. 2, the dimensions of the experimental range of Fig. 1 are reduced to the thickness of the line at the lower left-hand corner of the diagram. The line at the top represents the intensity of solar radiation, which is taken at 10,000 watts per square centimetre in round numbers. The points at which the various curves meet this line show the corresponding values of the solar temperature.

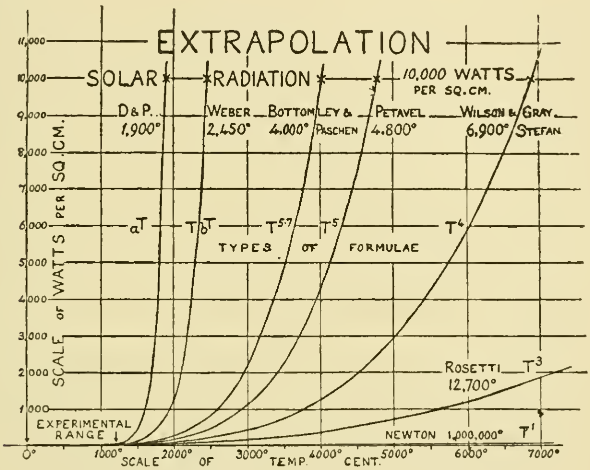

Fig. 2.—Temperature of the Sun by Extrapolation.

The estimates of one million degrees and upwards, which were current in many of the older books on astronomy, were deduced from the law of Newton, and are obviously out of the question. The celebrated formula of Dulong and Petit gives results between 1500° and 2000° C., according to the data assumed, and evidently errs too much in the other direction. At the same time, it must be observed that the recent formula of Weber gives a result which is very little higher. Paschen considered that his results lent support to Weber's formula, and disagreed entirely with Bottomley's. But, according to the writer's reductions, they agree very closely with Bottomley's, and are best represented by the formula $ET^{5.7}$. The experiments of Petavel agree most nearly with a fifth power law. On the other hand, the experiments of Wilson and Gray, in which the temperature was measured by the *expansion* of a platinum strip, instead of by the increase of its electrical resistance, appear to be in exact confirmation of the fourth power law of Stefan, and give a much higher result for the solar temperature. The formula of Rosetti is approximately a

third power law at high temperatures, and would not be admitted as probable, at least by physicists, at the present time.

The various formulæ above mentioned, together with the methods employed and the results deduced, are summarised in the following table:—

TABLE I.—LAW OF RADIATION.

Observers and date.	Temperature measured by	Radiation observed by	Formula proposed.	Solar temp. deduced.
				° C.
Dulong and Petit (1817)	Mercury thermometer	Rate of cooling in vac.	$E_1\ 1 \cdot 0077^T$	1900
Rosetti (1878) ..	Mercury thermometer	Thermopile Sb–Bi	E_3T^3 (nearly)	12,700
Stefan (1878) ..	No experiments made.		E_4T^4	6900
Schleiermacher (1885)	Platinum resistance	Heat loss C^2R in vac.	E_4T^4	6900
Weber (1888) ..	No experiments made.		$E_2T\ 1 \cdot 00043^T$	2450
Bottomley (1888) ..	Platinum resistance	Heat loss C^2R in vac.	$E_6T^{5 \cdot 7}$ *	4000
Paschen (1893) ..	Thermo-couple Pt–Pt Rh	Bolometer	$E_6T^{5 \cdot 7}$ *	4000
Wilson and Gray (1897)	Platinum expansion	Radio-micrometer	E_4T^4	6900
Petavel (1898) ..	Platinum resistance	Bolometer	E_5T^5 *	4800

* Formulæ deduced by the writer from the observations.

The foregoing table is not intended to be exhaustive, but merely as a comparison of typical formulæ, reduced to a common standard. It does not contain the results of photometric investigations.

The conclusion to be derived from the above illustrations appears to be, that in order to arrive at any certain knowledge with regard to the law of radiation, and the measurement of such extreme temperatures as those of the arc, and of the sun, the first step must be to secure a higher order of accuracy in the determination of the highest temperatures which can be observed and measured in the laboratory with material thermometers. There are other difficulties which are peculiar to the determination of the law of radiation, but we are at present concerned primarily with those relating to the measurement of temperature.

There are two comparatively independent lines along which research may proceed with advantage at the present time: (1) The direct comparison of different arbitrary methods; (2) the extension of the range of the gas-thermometer.

In order to secure consistency of statement and the reduction of the results of different observers to a common standard, it is in the first place desirable that the various methods available at the present time for the measurement of high temperatures in the laboratory should be *directly* compared *inter se*, through the greatest possible range. It is

the custom at present for different observers to reduce their results *indirectly* to the scale of the gas-thermometer by reference to certain assumed values of the boiling and freezing points of various substances. They generally assume different values for these fixed points, and adopt different methods of calibration, which are undoubtedly responsible for many of the discrepancies at present existing.

To take an illustration from the experiments already quoted, the remarkable discrepancy between the experiments of Bottomley, Paschen and Petavel, on the one hand, and those of Wilson and Gray and Schleiermacher, on the other, in the determination of the intensity of radiation from polished platinum, may be traced primarily to differences in the methods of measurement adopted. Bottomley and Petavel measured the electrical resistance of the radiating wire itself, and deduced the temperature by the usual formula for the platinum scale. Paschen calibrated his thermo-couple by reference to numerous fusing and boiling points. Wilson and Gray adopted the meldometer methods based on the expansion of platinum, which they found to be uniform. The vacuum in Schleiermacher's experiments could not be measured, and was probably vitiated by gas evolved from the heated platinum.

These and similar discrepancies might be in a great measure removed, so far as they depend on the measurement of temperature, by the direct comparison of the various methods of measurement. The "platinum" methods are among the most important and the most easily comparable by direct experiment. These methods are founded on the characteristic stability and infusibility of the metals of the platinum group, properties which are accompanied by an even more remarkable degree of constancy in their less obvious electrical attributes. The two older methods, based on (1) the expansion and (2) the specific heat of platinum, are of comparatively limited application, but have given very good results in the able hands of Joly and Violle. The more modern electrical methods have the advantage of much wider applicability and convenience. They are of two distinct kinds: (3) the thermo-electric method, represented by the Pt-Pd thermo-couple of Becquerel the Pt-Ir thermo-couple of Barus, and the Pt-Rh thermo-couple of Le Chatelier, and (4) the platinum resistance pyrometer of Siemens. The third method has been naturalised in this country, and brought to great perfection by the work of Sir William Roberts-Austen. The fourth method was that adopted by Bottomley, Schleiermacher, and Petavel in the experiments above mentioned, and has been applied with great success by Heycock and Neville at high temperatures, and by Dewar and Fleming at the other extremity of the scale.

The usual or indirect comparison of the foregoing methods by means of the fusing points of various metals is illustrated in the annexed table, which contains several of the most recent results. The numbers given in brackets are now published for the first time, and should be regarded as preliminary.

TABLE II.—FUSING POINTS BY "PLATINUM" METHODS.

Method.	Observers.	Silver.	Gold.	Copper.	Palladium.	Platinum.
(1) Expansion	C. & E. (1899) ..	(945)	(1061)	(1085)	(1640)	(1980)°
(2) Spec. heat	Violle (1879) ..	957°	1045°	1054°	1500°	1775°
(3) Thermo-couples	Becquerel (1863)	960°	1092°	1224°	..	..
	Barus (1892) ..	985°	1093°	1097°	1643°	1855°
	" (1894) ..	986°	1091°	1096°	1585°	1757°
	Holborn & Wien (1895)	968°	1072°	1082°	1587°	1780°
(4) Resistance ..	H. & N. (1895) ..	961°	1061°	1082°	..	..
	C. & E. (1899) ..	..	..	..	(1550)	(1820)

The results above given for the expansion method (1) were obtained by assuming the expansion to be uniform, and taking the F.P. of gold as 1061°. The results of Violle by the specific heat method (2) were deduced by assuming a linear formula for the specific heat of platinum. The discrepancies of the various results obtained by the thermo-electric method (3) are partly due to errors of observation, and partly to extrapolation, i.e. to differences in the formulæ of reduction. The high value found by Becquerel for the F.P. of copper as compared with gold and silver is probably to be explained by the use of a much thicker wire in the case of copper. The very accurate and consistent experiments of Heycock and Neville leave little doubt that the F.P. of pure copper is at least 20° above that of gold. The much smaller difference of 4° to 5°, given by Barus, may possibly be explained by contamination with oxygen or other impurity. In the case of silver and gold, Messrs. Holborn and Wien adopted the Becquerel method of observing the fusion of fine wires. In the case of copper, they adopted the much more accurate method of observing the freezing point of a large mass of metal in a crucible, which had been employed by the writer in 1892, and was used by Heycock and Neville throughout their researches. The Becquerel method is very liable to give results which are too high.

The determination of the higher fusing points of palladium and platinum is necessarily attended with greater uncertainty because it involves extrapolation, and is therefore more dependent on the particular formula of reduction assumed, in addition to the experimental difficulties of the higher temperatures. Considering all the obstacles to be encountered, it would be unreasonable to expect such different methods to give any closer agreement at these points.

Whatever the origin of these discrepancies, there can be no question that they greatly retard the progress of research and discovery at high temperatures. With the object of helping to remove these obstacles, the writer has recently been engaged, in conjunction

with Mr. Eumorfopoulos, in a direct comparison of methods (1), (3) and (4), which are simplest and most generally applicable. The advantages of the direct method of comparison are very great. (1) The comparison may be extended continuously throughout the scale, and is not confined to a few arbitrary selected points. (2) It is easy to apply the electric method of heating, which is of all methods the most easily regulated. (3) It is easy to arrange the experiments in such a way that there can be no question of difference of temperature between the thermometers under comparison, which is the most insidious source of error in high temperature measurement.

In the comparison of the scale of the expansion of platinum (1), with that of the platinum resistance thermometer (4), it is simply necessary to observe simultaneously the expansion and the electric resistance of a platinum strip, tube or wire maintained at a steady temperature by means of an electric current. The expansion may be measured, as in the meldometer of Joly, by means of a micrometer screw; but for lecture purposes it is preferable to adopt the method of the optical lever employed by Laplace in his experiments on expansion a century ago. By employing a direct-reading ohmmeter to indicate the changes of electrical resistance, it is thus possible to exhibit the difference between the two methods by the simultaneous advance of two spots of light on a single scale. If the two instruments are adjusted to read correctly at 0° and 1000° C., the resistance thermometer will be in advance at temperatures below 1000°, but will lag behind at higher temperatures, because the rate of expansion increases as the temperature rises, whereas the rate of change of resistance diminishes. As the result of these experiments, it appears that the two scales (1) and (4) differ from that of the gas-thermometer to a nearly equal extent, but in opposite directions.

The resistance of platinum at its melting point is more than six times as great as at 0° C., whereas the whole expansion amounts to only one-fiftieth part of the length. The electrical method is for this reason by far the most accurate and sensitive. It also possesses in a very striking degree the merit of pliability and adaptability to the needs of each particular problem. For this reason the scale of the platinum resistance thermometer has come to be regarded as the platinum scale *par excellence*, and has been adopted as the standard of reference in many recent researches.

As an illustration of the facility of applying this method, the determination of the fusing point of platinum on the platinum scale may be taken. This is a difficult experiment to perform by any other method. In performing the experiment by the measurement of the electrical resistance, it suffices to take a fine wire of which the electrical constants are accurately known, and to raise it gradually to its melting point by steadily increasing the current. The observation of the resistance of the central portions of the wire at the moment of fusion gives directly the temperature required on the platinum scale. In attempting to perform the same experiment by the expansion

method, we are met by the difficulty that the platinum begins to soften and stretch at a temperature considerably below its melting point. Owing to the smallness of the expansion, a very slight viscous extension produces a relatively large error. In the resistance method it is not necessary to subject the wire to tension, and a small strain would in any case produce an inappreciable error on account of the very large increase of resistance with temperature. To obtain an equal degree of accuracy by the calorimetric method (2), or the thermo-electric method (3), it is necessary to use a furnace in which relatively large quantities of platinum can be melted. This has been done by Violle for method (2), and by Barus and Holborn and Wien for method (3). The latter used a linear formula for extrapolation, although their gas-thermometer experiments appeared to indicate a cubic formula for temperatures below 1200° C.

The temperature of the melting point of platinum on the platinum scale by the resistance method (4) is approximately pt = 1350°, and varies but slightly for different specimens of platinum. The result, when reduced to the scale of the gas-thermometer by assuming that the rate of increase of resistance diminishes uniformly with rise of temperature (according to the usual formula of platinum thermometry, which has been verified with great care at moderate temperatures), gives a temperature of 1820° C. on the scale of the gas-thermometer. It is not improbable that platinum may deviate slightly from this formula at the extreme limit of the scale in the close neighbourhood of its melting point, but the evidence for this result is at least as good as that obtainable by any of the other methods. The observations are very easy and accurate as compared with the calorimetric method, and it is not necessary to make any arbitrary assumptions with regard to the formula of reduction, as in the case of the thermo-electric method.

As the accuracy of this formula has recently been called in question, on what appears to be insufficient grounds, by certain German and French observers, it is the more interesting at the present time to show that it leads to a result which cannot be regarded as improbable at the extreme limit of the scale. A different formula has recently been employed by Holborn and Wien, and supported by Dickson (Phil. Mag., December 1897). The writer has already given reasons (Phil. Mag., February 1899) for regarding this formula as inferior to the original, of which, however, it is a very close imitation. The above observations on the melting point of platinum, if reduced by Dickson's formula, would give a result $t = 1636°$ C., which appears to be undoubtedly too low as compared with the results of other methods, however great the margin of uncertainty we are prepared to admit in these difficult and debatable regions of temperature measurement.

It should be observed that the results of Violle by method (2) are consistently lower than those given by the resistance method in the case of silver, gold and copper. We should, therefore, expect a

difference in the same direction at the F.P. of Pt as found by method (4), and not a difference in the opposite direction as given by the thermo-electric method, on the arbitrary assumption of a different type of formula for extrapolation at high temperatures. It is a matter of some interest that the assumption of linear formulæ for both the specific heat and the rate of change of resistance, should lead to results so nearly consistent over so wide a range of temperature in the case of platinum.

The chief difficulty and uncertainty encountered by Paschen in his experiments on radiation, was that of arranging the thermo-couple so as to be at the same temperature as the radiating strip of platinum. It is better for this reason to measure the temperature of the strip itself by means of its electrical resistance, the method adopted by Schleiermacher, Bottomley and Petavel. The same difficulty occurs in the direct comparison of the scales of the thermo-couple and the platinum-resistance thermometer. The simplest method of avoiding this objection appears to be that recently adopted by the writer, of enclosing the thermo-couple completely in a thin tube of platinum, which itself forms the resistance thermometer. There can be no question of difference of temperature between the two, and the same tube may serve simultaneously for the expansion method and as a radiating source for bolometric investigation of the law of radiation. The uniformity of temperature throughout the length of the tube can be tested at any time by means of potential leads, or by shifting the thermo-couple to different positions along its length. The method of electric heating is employed, and the central portion only of the tube is utilised in the comparison.

The methods of measurement, so far as considered, are in a certain sense *arbitrary* in so far as they depend on extrapolation of empirical formulæ. If all these methods could be reduced by direct comparison to perfect agreement with each other, a definite scale of temperature would be attained to which all measurements could be referred, and which would leave nothing to be desired from a purely practical point of view. It is probable that this scale would not differ much from the theoretical or absolute scale of temperature. For theoretical investigations, however—without which no true scientific advance can be made—it is a matter of such fundamental importance to refer every measurement to the absolute scale, that no opportunity should be neglected of extending the possible range of accurate observation with the gas-thermometer, because this instrument affords at present the closest approximation to the absolute or theoretical scale. A consideration of the difficulties of the methods of gas-thermometry at present in use will lead naturally to the best methods of extending the range and accuracy of the instrument.

In the ordinary method of gas-thermometry a *bulb* containing the gas is exposed to the temperature to be measured, and the observation consists in determining either the expansion of volume or the increase of pressure of the gas. The principle is very similar to that of the

ordinary liquid in glass thermometers, but the apparatus is more cumbrous and difficult to use on account of the necessity of observing both the volume and the pressure of the gas. This method is very accurate at moderate temperatures, but the difficulties increase very rapidly above 1000° C. Above 1200° C., it is doubtful whether such measurements are of any greater value than those obtained by extrapolation. Apart from the difficulty (which is common to nearly all methods at high temperatures) of maintaining a uniform and steady temperature, the bulb-method of gas-thermometry is liable to the following special sources of error:—

(1) Changes in volume of the bulb.
(2) Leakage and porosity.
(3) Occlusion or dissociation.

In order to investigate these sources of error a special form of porcelain air-thermometer (Fig. 3) was designed by the writer, and was constructed in Paris, in December 1886, under the supervision of

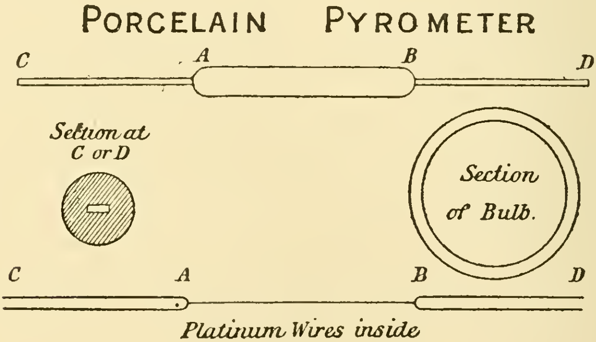

FIG. 3.

W. N. Shaw, F.R.S., of Emmanuel College, Cambridge. A figure and description of this instrument were published in the Phil. Trans. A., 1887. The same form has since been adopted by MM. Holborn and Wien in their experiments on the measurement of high temperatures at the Reichsanstalt. Thick tubes of 3 sq. mm. cross section, marked AC, BD in Fig. 3, were connected at each end of the cylindrical bulb BA. The length CD could be directly observed at any time with reading microscopes, and the linear expansion of the bulb could be deduced. The volume of the bulb could also be gauged at any time with air, and the mean temperatures of the separate portions AB, AC, BD, could be determined by means of platinum wires extending along the axis of the instrument. This was a more essential part of the apparatus, as the wires afforded a means of accurately reproducing any given set of conditions, and of testing the performance of the

gas-thermometer at high temperatures in respect of all the various sources of error above mentioned. (1) It was observed that the volume of the bulb underwent continuous changes, chiefly in the direction of contraction, and that the shrinkage was not symmetrical, being apparently greater in the circumference than in the length of the cylinder. (2) To prevent leakage, and to close the pores of the material, it is necessary to have the porcelain bulb glazed both inside and out. The glaze becomes sticky, and begins to run at a temperature below 1200° C., and the bulb begins to yield slightly and continuously to pressure above this point. (3) With some gases there appear to be slight traces of chemical action or occlusion of the gas by the walls of the bulb at high temperatures. It is for this reason preferable to use the inert gases nitrogen or argon as the thermometric material. In any case, the limit of high temperature measurement would be reached when either the gas, or the material of the bulb, began to dissociate or decompose. Deville and Troost, employing CO_2 for filling the porcelain bulb, found the temperature of the B.P. of zinc nearly 150° higher than with air or hydrogen. This they attributed to a partial dissociation of the CO_2 at the temperature as low as 930° C. Some experiments made by the writer appeared, however, to indicate that the effect was due to chemical action between the gas and the porcelain.

For these and other reasons it appears very doubtful whether any improvement or extension of range can be expected from the use of glazed porcelain. If an attempt is made to employ any of the more refractory kinds of fire-clay, there is the difficulty of finding a suitable glaze, and of eliminating leakage and porosity. The writer suggested the use of bulbs of fused silica some years ago (Proc. Iron and Steel Institute, 1892), and endeavoured to get such bulbs constructed, but without success. This material possesses many of the requisite qualities, but is for this very reason extremely difficult to work. Metallic bulbs of platinum or platinum-iridium are by far the most perfect in respect of constancy of volume, regularity of expansion, and facility of accurate construction; but unfortunately, as Deville and Troost showed, they have such an inveterate tendency for occluding or dissolving gases at high temperatures, that the use of metallic bulbs has been practically discontinued, in spite of their obvious advantages in other respects.

After making many vain experiments, the writer was forced to the conclusion that the ordinary bulb-methods did not promise any satisfactory solution of the problem of extending the range of the gas-thermometer, and that it was necessary to attempt a radically new departure. The optical method, depending on the measurement of the refractivity of a gas at high temperatures, and the acoustical method, depending on the observation of the wave-length of sound, although of great theoretical interest, did not appear to promise sufficient delicacy of measurement or facility of practical application. Experiments were therefore made on the methods of effusion and

transpiration, which had been occasionally suggested by previous writers, but have not as yet (so far as the author is aware) been practically investigated as a means of measuring temperature on the absolute scale. The method of effusion consists in observing the resistance to the efflux of gas through a small hole or orifice in a thin plate. In the method of transpiration the gas is made to pass through a fine tube instead of a small orifice, and the resistance to its passage is observed in a similar manner. These methods may be called "resistance-methods" to distinguish them from the ordinary or "bulb-methods" of pyrometry. They are closely analogous to the now familar resistance-method of electrical pyrometry, and possess many of the advantages of that method in point of delicacy and facility of application. One very obvious and material advantage, especially for high temperature work, is the smallness and sensitiveness of the instrument as compared with the bulb of an ordinary gas-thermometer. But the most important point of difference, which led the writer to the adoption of these methods, is that the measurements are practically unaffected by occlusion or evolution of gas by the material of the tubes. There is a *continuous* flow of gas through the apparatus. This flow is very large in proportion to any possible leakage, and it is therefore possible to employ platinum tubes with perfect safety.

The method of effusion may be very simply illustrated by means of a fine hole in the side of a large and thin platinum tube which is heated by an electric current. The current of air is heated in its passage through the tube before it effuses through the orifice. The heated air expands in volume, and the resistance to effusion is increased in proportion to the temperature to which the air is heated. The increase of resistance may be shown by means of a gas-current indicator or "rheoscope," which consists of a delicately suspended vane deflected by a current of gas. A mirror is attached to the vane, and the deflection is measured by the motion of a spot of light reflected on to a scale, exactly as in the case of the mirror galvanometer when used for indicating changes of electrical resistance. As a standard of comparison, to show the changes of temperature of the tube, the changes of electrical resistance of the same tube are simultaneously shown by means of a suitable ohmmeter.

The method of effusion is a beautifully simple method, and gives a nearly uniform scale; but it has two disadvantages, which it shares with the thermo-electric method of measurement. (1) It necessarily measures temperature at a point, namely at the point of effusion, and cannot be easily arranged to give the mean temperature throughout a space. (2) It is difficult to make the effusion resistance sufficiently large for purposes of accurate measurement. A large resistance means a very fine hole, and it is not easy to satisfy the theoretical conditions of the problem with sufficient accuracy and eliminate the effects of viscosity.

The method of transpiration is more complicated, and does not

give so uniform a scale, or so simple a formula. It has the great advantage, however, that the theoretical conditions of flow may be realised with unlimited accuracy, and that the transpiration resistance can be measured with a degree of precision very little, if at all, inferior to the corresponding electrical measurement.

The complication of the transpiration problem arises from the fact that the flow depends on the increase of the viscosity of the gas, as well as on its expansion. The viscosity of liquids in general decreases very considerably with rise of temperature. That of water, for instance, is six times less at the boiling point than at the freezing point. If the viscosity of gases diminished in a similar manner, it might happen that the transpiration resistance would decrease with rise of temperature. Maxwell was the first to give a theoretical explanation of the behaviour of gases in this respect. On certain simple kinetic assumptions, he showed that the viscosity should increase in direct proportion to the absolute temperature. Since the expansion follows the same law, the transpiration resistance on Maxwell's hypothesis should increase in proportion to the square of the temperature. This would give a fairly simple formula, and would make the transpiration thermometer a very sensitive instrument, but the scale would be very far from uniform. Maxwell made some experiments on the temperature variation of the viscosity between 0° and 100° C., which appeared to give support to his mathematical assumptions; but his apparatus did not happen to be of a very suitable type for temperature measurement, and it is clear that he did not regard this part of his experimental work with great confidence.

The question of the viscosity of gases was next attacked with great vigour in Germany by a number of different physicists. They ultimately succeeded in proving that the law was not quite so simple as Maxwell had supposed, and that the rate of increase of viscosity was less than that of volume. A summary of some of the principal results obtained, over the range 0° to 100° C., is given in the following

TABLE III.—VARIATION OF VISCOSITY v WITH TEMPERATURE T. FORMULA, $v/v_0 = (T/T_0)^n$.

Observers.	Dates	Values of Index n (0° to 100° C.)			
		Air.	O_2.	H_2.	CO_2.
Maxwell	1866	1·000	—	—	—
Meyer	1873	·61 – ·83	—	—	—
Puluj	1874	·47 – ·65	—	—	—
Obermeyer	1875	·76	·80	·70	·94
Wiedemann	1876	·73	—	—	·93
Warburg	1876	·74 – ·77	—	·63	—
„ and Kundt	1876	·72	—	·69	—
Holman	1876	·74 – ·80	—	—	—

table, in which the rate of increase is expressed by finding the power n of the absolute temperature T to which the viscosity is most nearly proportional. The most concordant results were obtained by the method of transpiration, and gave an average of ·76 for the index n in the case of air. The more condensible gases gave larger values for the rate of increase, but the value for hydrogen appeared to be smaller.

It will be observed that the results are not very concordant, but the experiments are much more difficult and liable to error than might be supposed. The most accurate method was that employed by Holman, but even in this case the margin of uncertainty is considerable. It would evidently be impossible to employ the method of transpiration to any advantage for the determination of temperature, unless a far higher order of accuracy could be easily attained. After repeating the majority of the more promising methods in detail, including the original method of Maxwell, the writer came to the conclusion that they were entirely unsuitable for the purposes of thermometry, and would have abandoned the attempt entirely if he had not fortunately succeeded in finding a more perfect way.

In studying the flow of electricity through conductors, which is in many respects analogous to that of a fluid through a fine tube, electricians have been compelled, from the intangible nature of the fluid with which they work, to elaborate the most delicate and powerful methods of investigation. One of the most useful of these methods is generally known as the Wheatstone-bridge method, and is used for measuring the resistance of a conductor to the passage of an electric current. The method is equally applicable and equally exact for determining the resistance of a fine tube to the passage of a gas. The writer was already very familiar with the application of this method in all its refinement of detail to electrical resistance thermometry. The suggestion for applying it to the closely analogous problem of transpiration was supplied by the researches of W. N. Shaw, F.R.S., who had already applied it, in connection with certain experiments on ventilation, to the effusion of air through large orifices at ordinary temperatures.

The apparatus used by Shaw (described in the Proc. Roy. Soc., vol. xlvii., 1890) consisted of boxes to represent rooms, with apertures about half a square inch in area to represent ventilators. Two of these apertures were made in the form of adjustable slits. The circulation of air through two rooms in parallel was maintained by a gas burner, and the slits were adjusted to make the pressure in the two rooms the same, as indicated by the absence of flow in a connecting tube, containing a pivoted needle and vane as a current detector. The balance was shown to be independent of the air-current when that was varied from one to four cubic feet per minute. The effusion resistance of an aperture was also verified to be approximately proportional to the square of the reciprocal of the area, with apertures of similar shape. This method of investigation was admirably adapted

to problems in ventilation, in which the phenomena depend mainly on effusion through relatively large apertures. It would, however, be difficult to adapt to the problem of temperature measurement. It would not be easy to make an aperture which could be continuously varied without changing its shape, and at the same time to measure the change of area with sufficient accuracy, if the area were small enough to prevent appreciable cooling of the thermometer by the current of air flowing through it. There is also the disadvantage that the pressure-difference varies as the square of the current; so that, if very small currents are used, the effects of viscosity become more important, and the balance ceases to be independent of the current, unless everything is symmetrical and at the same temperature in corresponding parts.

For these reasons it seems preferable, in applying the Wheatstone-bridge method to air-currents, to employ fine tubes as resistances, and to eliminate the effects of effusion as completely as possible, at least in the resistance-measuring part of the apparatus. With transpiration resistances the current is directly proportional to the pressure difference, the electrical analogy is much closer, and the theoretical conditions can be very accurately realised.

The Wheatstone-bridge method of measurement proved to be so exact, and so perfectly adapted to the problem of transpiration thermometry, that, after some preliminary experiments, the writer had a very elaborate apparatus constructed, in the year 1893, which was in every detail the exact analogue of an electrical resistance thermometer. The fine wire resistances of the electrical apparatus, in terms of which the change of resistance of the thermometer is measured, are replaced in the transpiration box by a graduated series of fine tubes, which can be short-circuited by means of taps of relatively large bore, corresponding to the plugs of negligible resistance in the electrical resistance box. The galvanometer is replaced by a rheoscope, constructed after a pattern devised by Joule for a different purpose, which can be made to rival in delicacy the best modern electrical instruments. The pyrometer itself consists of a fine tube of platinum instead of a wire, and is fitted with "compensating leads" to correspond with those of the electrical instrument. All the detail of the methods of observation and calibration are faithfully copied from the electrical apparatus, and the results, so far as the measurement of transpiration resistance is concerned, are equally satisfactory.

Fig. 4 is a diagram of a working model of the transpiration balance, which was exhibited at the lecture. This model has a vertical needle for index, and a pivoted mica vane, which is deflected when a current flows through the bridge piece. It is constructed to work on the ordinary lighting-gas pressure, and to give its maximum deflection for a 10 per cent. change of resistance with the gas about half off. With all the taps off, the resistances on either side are equal, and there is no deflection. In the diagram the balance is supposed to have been disturbed by opening one of the taps. The apparatus

actually used for temperature measurement has sixteen taps and a mirror rheoscope, and is a thousand times more sensitive.

In order to apply the method to the measurement of extreme temperatures, it is not sufficient to be able to measure resistance. It is also necessary to determine the law of the variation of viscosity with temperature. Here, again, recourse must be had to the method of extrapolation. Fortunately, in the present instance, the temperature can be measured through a very wide range, and the range of extrapolation, being limited by the melting point of platinum, is not

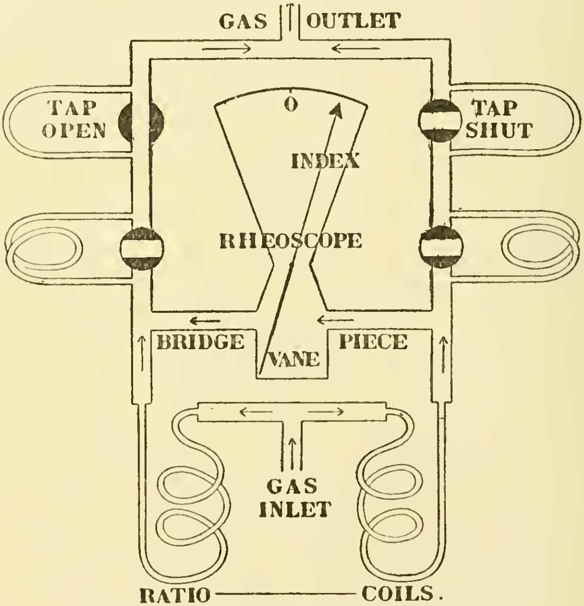

Fig. 4.—Diagram of Transpiration Balance.

very great in comparison. It should be possible, therefore, by sufficiently varying the conditions of the experiments, and by comparing the behaviour of different gases throughout the whole range of temperature, to arrive at a very fair degree of certainty with regard to the essential nature of the phenomenon. Owing to want of leisure for the work, the author's experiments have not as yet extended over a sufficient range of temperature, except in the case of air, to warrant the publication of any general conclusions with regard to the law of variation of viscosity, or of any results at high temperatures obtained by the method of extrapolation. It may be stated, however, that the formula above quoted, according to which the viscosity varies as some

power n of the temperature, though fairly exact over a moderate range of temperature, fails entirely when tested at higher points. The results of Obermayer appear to be the most accurate for the different gases between 0° and 100° C., but if the same formula is retained, the value of the index n diminishes as the temperature is raised. Taking the average value between 0° and 100° for air as being 0·76, the value falls to 0·70 between 100° and 450°. A result of this nature was found by Wiedemann, but the rate of diminution which he gives appears to be far too great. He gives, for instance, the value $n = 0 \cdot 67$ for air between 0° and 184°, which implies a rate of diminution of the index many times greater than that which actually occurs. It would be very difficult by the method which he employed to make sure of *any* deviation whatever from the formula over so small a range, and since the error of his determination is much greater than that of the formula, he can hardly be said to have disproved the index law.

The problem is seriously complicated by the failure of the simple formula; but since the measurements are capable of great exactitude, and since it is possible to obtain many independent checks by comparing the results of the two methods of effusion and transpiration, and also by examining the behaviour of different gases, the author is confident of ultimate success. The method of experiment here described has already led to many promising and interesting results, and it is probable that the complete solution of the problem when attained, besides leading to more accurate determinations of extreme temperatures, may also throw light on dissociation and on many other points which are at present obscure in the theory of gases.

[H. L. C.]

WEEKLY EVENING MEETING,

Friday, March 17, 1899.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Professor Francis Gotch, M.A. F.R.S.

The Electric Fish of the Nile.

The lecture dealt almost exclusively with the formidable fish found in the rivers of North and of West Africa, *Malapterurus electricus.*

Photographs were shown of the drawings upon the interior of the tomb of Ti, showing that the fish was recognised as remarkable by the Egyptians five thousand years before the Christian era. Living specimens of the fish were also displayed, these having been given to the lecturer, for the purpose of illustrating the lecture, by the authorities of the Liverpool Corporation Museum.

The structure of the electrical organ was then described. It is situated in the skin enclosing the whole body of the fish, and has a beautiful and characteristic appearance when seen in microscopic sections. Each organ consists of rows of compartments, and each compartment has slung athwart it a peculiar protoplasmic disc shaped like a peltate leaf, with a projecting stalk on its caudal side. Nerves enter each compartment, and end, according to the recent work of Ballowitz, in the stalk of each disc. By these nerves nervous impulses can reach the organ; the arrival of such impulses at the nerve terminations evokes a state of activity which is associated with the development of electromotive charges of considerable intensity constituting the organ shock. The shock is an intense current traversing the whole organ from head to tail and returning through the surroundings; it stuns small fish in the neighbourhood and can be felt by man when the hand is placed near the fish, as a smart shock reaching up the arms to the shoulders.

Recent investigations made by the lecturer at Oxford in conjunction with Mr. G. J. Burch were next described. These comprised a large series of photographic records of the displacement of the mercury of a capillary electrometer in consequence of the electrical disturbance in the organ which is "the organ shock." A number of these records were exhibited; they showed the time relations, mode of commencement and manner of subsidence of the shock, and demonstrated its similarity to the electrical changes known to exist in nervous tissue during the passage of a nervous impulse. A remarkable feature of the organ shock as distinct from the phenomena of

nerve was then brought forward. The shock even when evoked by a single stimulus was shown to be rarely if ever a single one. Each effect consists of a rhythmical series of electrical changes occurring one after another in a perfectly regular manner at intervals of $\frac{1}{100}''$ to $\frac{1}{300}''$, the rate depending upon the temperature. By special experiments it was shown that this rhythmical series is due to self excitation, each change producing an electrical current of sufficient intensity to excite the nerves of the tissue in which it was generated. It follows that only the initial member of the series need be evoked by nervous impulses descending the nerves, since the others must then ensue. The potency of the organ as a weapon to be wielded by the fish is thus enormously increased by its resemblance to a self-loading and self-discharging automatic gun. The total electromotive-force of the whole organ in a fish only eight inches long can reach the surprising maximum of 200 volts, at any rate in the case of the initial shock. The attainment of this maximum is due to the simultaneous development of perfectly similar electromotive changes in each of the two million discs of which the organ is composed. In a single disc the maximal electromotive-force only amounts to from ·04 to ·05 volt, and since in a small nerve an electrical change of ·03 to ·04 volt has been observed, the large total effect is not due to any extraordinarily intense electrical disturbance in each tissue element, but to the tissue elements being so arranged that the effect in one augments those simultaneously produced in its neighbours.

Finally, the remarkable characters of the nervous connections of the organ were described. Each lateral half of the organ, although it has a million plates receiving nerve branches, is innervated by one single nerve fibre and this is the offshoot of a single giant nerve-cell situated at the cephalic end of the spinal cord. The structure of this nerve-cell was displayed by means of microscopic sections and by wax models made by G. Mann, of Oxford. As regards the nervous impulses which the fish can discharge through this nerve-cell, experimental results were described which show that the fish is incapable of sending a second nervous impulse after a preceding one until a period of $\frac{1}{10}$ second has elapsed, and that this interval is rapidly lengthened by fatigue to as much as several seconds. The inability of the central nervous system to repeat the activity of the organ obviously presents disadvantages to the use of the shock as a weapon for attack or defence, but such disadvantage is more than counterbalanced by the property of the organ alluded to in the earlier part of the lecture, viz. that of self-excitation, since a whole series of shocks continue to occur automatically in rapid succession provided that an initial one has been started by the arrival at the organ of a nervous impulse sent out from the central nerve-cell.

[F. G.]

WEEKLY EVENING MEETING,

Friday, March 24, 1899.

Sir Frederick Bramwell, Bart., D.C.L. LL.D. F.R.S., Honorary Secretary and Vice-President, in the Chair.

The Right Hon. Lord Rayleigh, M.A. D.C.L. LL.D. F.R.S. *M.R.I.*, Professor of Natural Philosophy, *R.I.*

Transparency and Opacity.

One kind of opacity is due to absorption; but the lecture dealt rather with that deficiency of transparency which depends upon irregular reflections and refractions. One of the best examples is that met with in Christiansen's experiment. Powdered glass, all from one piece and free from dirt, is placed in a bottle with parallel flat sides. In this state it is quite opaque; but if the interstices between the fragments are filled up with a liquid mixture of bisulphide of carbon and benzole, carefully adjusted so as to be of equal refractivity with the glass, the mass becomes optically homogeneous, and therefore transparent. In consequence, however, of the different dispersive powers of the two substances, the adjustment is good for one part only of the spectrum, other parts being scattered in transmission much as if no liquid were employed, though, of course, in a less degree. The consequence is that a small source of light, backed preferably by a dark ground, is seen in its natural outlines but strongly coloured. The colour depends upon the precise composition of the liquid, and further varies with the temperature, a few degrees of warmth sufficing to cause a transition from red through yellow to green.

The lecturer had long been aware that the light regularly transmitted through a stratum from 15 to 20 mm. thick was of a high degree of purity, but it was only recently that he found to his astonishment, as the result of a more particular observation, that the range of refrangibility included was but two and a half times that embraced by the two D-lines. The poverty of general effect, when the darkness of the background is not attended to, was thus explained; for the highly monochromatic and accordingly attenuated light from the special source is then overlaid by diffused light of other colours.

More precise determinations of the range of light transmitted were subsequently effected with thinner strata of glass powder contained in cells formed of parallel glass. The cell may be placed between the prisms of the spectroscope and the object-glass of the collimator. With the above mentioned liquids a stratum 5 mm. thick transmitted, without appreciable disturbance, a range of the spectrum measured by 11·3 times the interval of the D's. In another cell of

the same thickness an effort was made to reduce the difference of dispersive powers. To this end the powder was of plate glass and the liquid oil of cedar-wood adjusted with a little bisulphide of carbon. The general transparency of this cell was the highest yet observed. When it was tested upon the spectrum, the range of refrangibility transmitted was estimated at 34 times the interval of the D's.

As regards the substitution of other transparent solid material for glass, the choice is restricted by the presumed necessity of avoiding appreciable double refraction. Common salt is singly refracting, but attempts to use it were not successful. Opaque patches always interfered. With the idea that these might be due to included mother liquor, the salt was heated to incipient redness, but with little advantage. Transparent rock-salt artificially broken may, however, be used with good effect, but there is some difficulty in preventing the approximately rectangular fragments from arranging themselves too closely.

The principle of evanescent refraction may also be applied to the spectroscope. Some twenty years ago, an instrument had been constructed upon this plan. Twelve 90° prisms of Chance's "dense flint" were cemented in a row upon a strip of glass (Fig. 1), and the whole was immersed in a liquid mixture of bisulphide of carbon with a little benzole. The dispersive power of the liquid exceeds that of the solid, and the difference amounts to about three-quarters of the

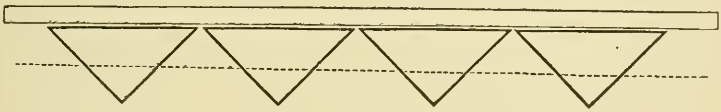

Fig. 1.

dispersive power of Chance's "extra dense flint." The resolving power of the latter glass is measured by the number of centimetres of available thickness, if we take the power required to resolve the D-lines as unity. The compound spectroscope had an available thickness of 12 inches or 30 cm., so that its theoretical resolving power (in the yellow region of the spectrum) would be about 22. With the aid of a reflector the prism could be used twice over, and then the resolving power is doubled.

One of the objections to a spectroscope depending upon bisulphide of carbon is the sensitiveness to temperature. In the ordinary arrangement of prisms the refracting edges are vertical. If, as often happens, the upper part of a fluid prism is warmer than the lower, the definition is ruined, one degree (Centigrade) of temperature making nine times as great a difference of refraction as a passage from D_1 to D_2. The objection is to a great extent obviated by so mounting the compound prism that the refracting edges are *horizontal*, which of course entails a horizontal slit. The disturbance due to a stratified temperature is then largely compensated by a change of focus.

In the instrument above described the dispersive power is great—

the D-lines are seen widely separated with the naked eye—but the aperture is inconveniently small ($\frac{1}{2}$-inch). In the new instrument exhibited the prisms (supplied by Messrs. Watson) are larger, so that a line of ten prisms occupies 20 inches. Thus, while the resolving power is much greater, the dispersion is less than before.

In the course of the lecture the instrument was applied to show the duplicity of the reversed soda lines. The interval on the screen between the centres of the dark lines was about half an inch.

It is instructive to compare the action of the glass powder with that of the spectroscope. In the latter the disposition of the prisms is regular, and in passing from one edge of the beam to the other there is complete substitution of liquid for glass over the whole length. For one kind of light there is no relative retardation ; and the resolving power depends upon the question of what change of wave length is required in order that its relative retardation may be altered from zero to the quarter wave length. All kinds of light for which the relative retardation is less than this remain mixed. In the case of the powder we have similar questions to consider. For one kind of light the medium is optically homogeneous, i.e. the retardation is the same along all rays. If we now suppose the quality of the light slightly varied, the retardation is no longer precisely the same along all rays ; but if the variation from the mean falls short of the quarter wave length it is without importance, and the medium still behaves practically as if it were homogeneous. The difference between the action of the powder and that of the regular prisms in the spectroscope depends upon this, that in the latter there is complete substitution of glass for liquid along the extreme rays, while in the former the paths of all the rays lie partly through glass and partly through liquid in nearly the same proportions. The difference of retardations along various rays is thus a question of a deviation from an average.

It is true that we may imagine a relative distribution of glass and liquid that would more nearly assimilate the two cases. If, for example, the glass consisted of equal spheres resting against one another in cubic order, some rays might pass entirely through glass and others entirely through liquid, and then the quarter wave length of relative retardation would enter at the same total thickness in both cases. But such an arrangement would be highly unstable ; and, if the spheres be packed in close order, the extreme relative retardation would be much less. The latter arrangement, for which exact results could readily be calculated, represents the glass powder more nearly than does the cubic order.

A simplified problem, in which the element of chance is retained, may be constructed by supposing the particles of glass replaced by thin parallel discs which are distributed entirely at random over a certain stratum. We may go further and imagine the discs limited to a particular plane. Each disc is supposed to exercise a minute retarding influence on the light which traverses it, and they are sup-

posed to be so numerous that it is improbable that a ray can pass the plane without encountering a large number. A certain number (m) of encounters is more probable than any other, but if every ray encountered the same number of discs, the retardation would be uniform and lead to no disturbance.

It is a question of Probabilities to determine the chance of a prescribed number of encounters, or of a prescribed deviation from the mean. In the notation of the integral calculus the chance of the deviation from m lying between $\pm r$ is *

$$\frac{2}{\sqrt{\pi}} \int_0^{\tau} e^{-\tau^2} d\tau,$$

where $\tau = r / \sqrt{(2m)}$. This is equal to $\cdot 84$ when $\tau = 1 \cdot 0$, or $r = \sqrt{(2m)}$; so that the chance is comparatively small of a deviation from m exceeding $\pm \sqrt{(2m)}$.

To represent the glass powder occupying a stratum of 2 cm. thick, we may perhaps suppose that $m = 72$. There would thus be a moderate chance of a difference of retardations equal to, say, one-fifth of the extreme difference corresponding to a substitution of glass for liquid throughout the whole thickness. The range of wave lengths in the light regularly transmitted by the powder would thus be about five times the range of wave lengths still unseparated in a spectroscope of equal (2 cm.) thickness. Of course, no calculation of this kind can give more than a rough idea of the action of the powder, whose disposition, though partly a matter of chance, is also influenced by mechanical considerations; but it appears, at any rate, that the character of the light regularly transmitted by the powder is such as may reasonably be explained.

As regards the size of the grains of glass, it will be seen that as great or a greater degree of purity may be obtained in a given thickness from coarse grains as from fine ones, but the light not regularly transmitted is dispersed through smaller angles. Here again the comparison with the regularly disposed prisms of an actual spectroscope is useful.

At the close of the lecture the failure of transparency which arises from the presence of particles small compared to the wave length of light was discussed. The tints of the setting sun were illustrated by passing the light from the electric lamp through a liquid in which a precipitate of sulphur was slowly forming.† The lecturer gave reasons for his opinion that the blue of the sky is not wholly, or even principally, due to particles of foreign matter. The molecules of air themselves are competent to disperse a light not greatly inferior in brightness to that which we receive from the sky.

[R.]

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* See Phil. Mag. 1899, vol. xlvii. p. 251. † Op. cit. 1881, vol. xii. p. 96.

GENERAL MONTHLY MEETING,

Monday, April 10, 1899.

His Grace the Duke of Northumberland, President, in the Chair.

Mrs. Aston,
Thomas Oswald Belshaw, Esq.
Wilfred Hall, Esq.
Mrs. F. McClean,

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned for the following Donations to the Fund for the Promotion of Experimental Research at Low Temperatures:—

Sir Benjamin Baker, K.C.M.G.	£50
Nobel's Explosives Company	£105

His Grace the Duke of Northumberland announced that the Hodgkins Medal, which was the first gold medal for scientific work ever given by the Smithsonian Institution, had been awarded to Professor Dewar in recognition of his discoveries in the liquefaction of air. In making the announcement His Grace congratulated Professor Dewar on the honour conferred on him, and also on the Royal Institution, which he regarded as specially gratifying as a sympathetic recognition of scientific work and discovery in this country, coming from the great nation of our own blood on the other side of the Atlantic.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

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The Secretary of State for India—Progress Report of the Archæological Survey of Western India for the year ending June 1898. fol.
The Lords of the Admiralty—The Nautical Almanac for 1902. 8vo. 1899.
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Catalogue of Welwitsch's African Plants, Parts 2, 3. By W. P. Hiern.
List of the Types and figured Specimens of Fossil Cephalopoda. 8vo. 1898.
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Accademia dei Lincei, Reale, Roma—Classe di Scienze Morali, Storiche e Filologiche: Rendiconti. Serie Quinta, Vol. VII. Fasc. 12. 8vo. 1899.
Atti, Serie Quinta: Rendiconti. Classe di Scienze Fisiche, &c. 1° Semestre, Vol. VIII. Fasc. 4, 5. 8vo. 1899.

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Athenæum for March, 1899. 4to.
Author for March, 1899.
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Brewers' Journal for March, 1899. 8vo.
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Engineer for March, 1899. fol.
Engineering for March, 1899. fol.
Homœopathic Review for March, 1899.
Horological Journal for March, 1899. 8vo.
Industries and Iron for March, 1899. fol.
Invention for March, 1899. 8vo.
Journal of Physical Chemistry for Feb. 1899. 8vo.
Journal of State Medicine for March, 1899. 8vo.
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Machinery Market for March, 1899. 8vo.
Nature for March, 1899. 4to.
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L'air atmosphérique exerce-t-il une influence sur la hauteur d'un mince jet d'eau?
Principes généraux d'une nouvelle théorie capillaire. 8vo. 1899.
Sur l'interprétation du principe d'Archimède fondée sur la parfaite élasticité des liquides. 8vo. 1898.
Le principe d'Archimède et l'égalité de l'action et de la réaction. 8vo. 1898.
Sur une résistance spéciale constaté à la surface des grands cours d'eau. 8vo. 1898.
Conservation des toiles peintes. 8vo.
Sur les propriétés fondamentales des liquides. 8vo. 1898.
Sur les nombreux effets de l'élasticité des liquides. 8vo. 1898.
Quelques remarques sur une expérience curieuse de J. Plateau. 8vo. 1898.
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WEEKLY EVENING MEETING,

Friday, April 14, 1899.

Sir William Crookes, F.R.S., Vice-President, in the Chair.

Professor A. W. Rücker, M.A. D.Sc. Sec.R.S. *M.R.I.*

Earth Currents and Electric Traction.

Discussing the cause of the earth's magnetism, the lecturer said it might be due to the existence of electric currents at a considerable distance below the surface or to the presence of magnetised material within the earth. He described the magnetarium constructed by Mr. Henry Wilde in accordance with the latter hypothesis and the means by which he was able to reproduce on a suitably arranged geographical globe the magnetic condition of the earth as observed in nature. He came to the conclusion, after an examination of the difficulties involved, that the hypothesis must be admitted to be at least one that was not physically impossible. Turning next to the artificial surface currents such as resulted from electric railways, he pointed out how dangerous they were to magnetic observatories. This danger was first experienced in America, where rough methods of construction had produced great disturbances, the observatories at Washington and Toronto having been ruined from this cause. Doubtless construction both in England and America had greatly improved, but that did not end the matter, for it was not certain that the most careful construction could entirely prevent the evil. The engineers concerned in the various industrial enterprises had received those connected with the observations very kindly and had agreed to practice certain precautions, such as not putting currents to earth and providing for insulated return wires. Still the next year or two must be an anxious time, since if these measures proved inefficient, the observatories would have to be moved, and a break be caused in the observations of which it was very important to preserve the continuity. In conclusion he discussed the possibility of the existence of currents between earth and air.

WEEKLY EVENING MEETING,

Friday, April 21, 1899.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Frederick Walker Mott, M.D. F.R.S.

Structure of the Brain in Relation to its Functions.

What we know, even what we believe we know, of the relation of the structure of the brain to its functions is comparatively small indeed to what we do not know. The history of the evolution of our knowledge of the structure of the nervous system is full of interest, but time will not permit me to mention more than the fact that Descartes, although he was never able to see the structure of the nervous system, yet considered the nervous fibres to consist of little tubes along which the "animal spirits" were conveyed from the brain to the muscles; if we substitute the phrase "nervous impulse" for animal spirits, his conception was not so very far behind our conception of this structure at the present time.

At the beginning of this century the microscope demonstrated that the nervous fibre was not a hollow tube, but that it contained a central solid axial core. A little later, microscopical research showed that the brain not only contained *nerve fibres* but myriads of little masses of protoplasm of a regular shape—*nerve cells.* These are the two essential elements of the nervous system of all animals with a nervous system. At first, microscopical research was unable to determine the relationship of the cells to the fibres in the brain. All that was known was that the fibres were found in the white matter, the cells in the grey matter; where the fibres came from and where they went to, was not known. Later on, I shall endeavour to show the enormous advance which has been made in our knowledge of the minute structure of the nervous system, by the invention of instruments with which extremely thin slices of the brain can be cut; so that these delicate sections can be examined with the high magnifying powers of the microscope. These sections are stained with various chemical reagents so that different structural features in the nerve cells and fibres are seen by the affinity they possess for colouring with the reagents used. The brilliant aniline dyes are among the most useful of these reagents. The investigation of the

minute structure of the brain is truly a most elaborate microchemical study, and we can test the condition of the tissues of the body by their colour reactions, on the same principle that the chemist tests fluids by colour reactions in his test tube.

Before proceeding further, however, I feel that my remarks will be more intelligible if I point out some of the more important features in the general anatomy of the brain.

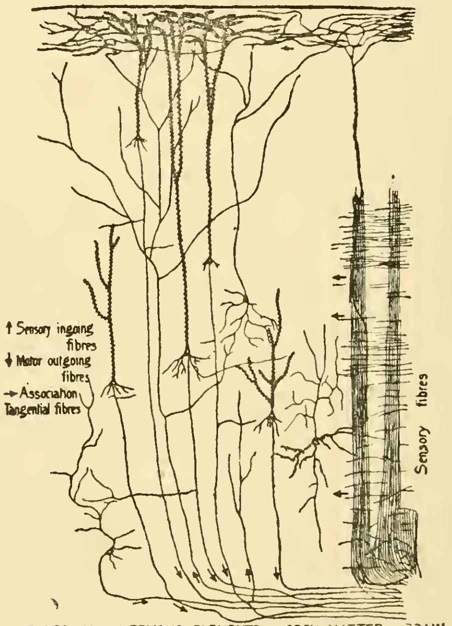


Fig. 1.

Diagrammatic representation of the Cerebral Cortex, showing the neurones with their processes, also sensory ingoing, motor outgoing, and association fibres; after Foster.

If you look at a series of brains of vertebrates you will observe that as you rise in the zoological scale the brain becomes more and more complex in structure, until in man it reaches its highest degree of complexity. Yet, architecturally speaking, these brains are all built on the same general plan, and at one time during its development the human brain was as simple in structure as that of a bird or fish. Moreover, the microscopical nervous units which make up the structure of the brain of all vertebrates are constructed on a

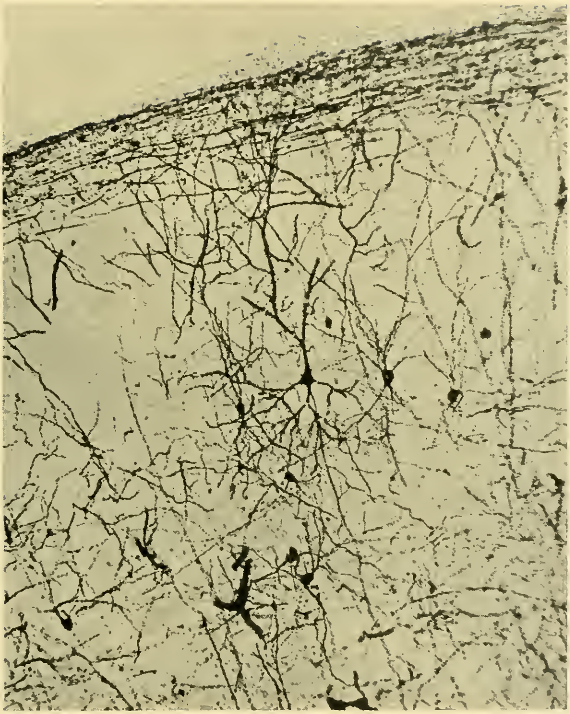


Fig. 2.

Photomicrograph of a Section of a Dog's Brain, stained by Cox's method, showing large psycho-motor cells sending their processes up to the surface layer of horizontal (tangential) fibres, which serve to bring them into relation with the sensory ingoing fibres. Magnified 100 diam.

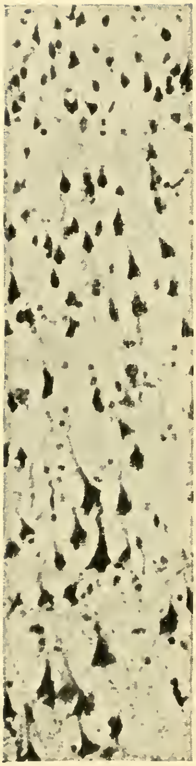


FIG. 3A. Section of the Brain of a Healthy Man, killed suddenly by a stab in the heart. Shows layers of neurones of the cerebral cortex (grey matter). Magnified 150 diam.

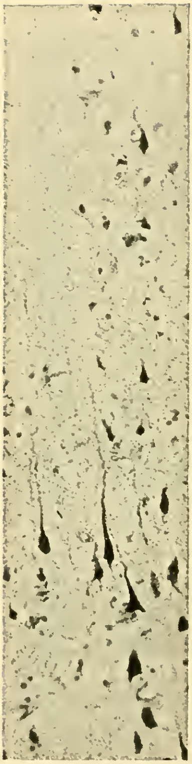


FIG. 3B. Section of the Brain of a case of Juvenile General Paralysis of the Insane, from the same region of the brain. Shows great diminution and atrophy of the nervous elements in the grey matter. Magnified 150 diam.

similar plan, only in man they are relatively more numerous, more varied and more complex.

If a brain be sliced into, it will be seen that the centre is white, and that it has a mantle or rind of a greyish colour. This grey mantle consists of countless millions of cells with their processes. The photograph I am showing is of a tiny speck of grey substance of the brain highly magnified. You observe the little pyramids of protoplasm with a nucleus in the middle sending out branches in all directions, these processes forming apparently a delicate network. The whole of the surface of the brain is covered by myriads of these little cells arranged in five layers (Figs. 3A and 3B). Streaming in among these cells of the grey matter, and derived from other cells in the deeper structures of the brain, are innumerable fibres, while a still larger number of fibres may be seen lower down, and these are in great part the fibres which stream out from the grey matter to form the white substance (Fig. 1).

The nervous units are all of similar general structure in the brains of all vertebrates.

I will describe now the structure of one of these simple nervous units. It consists of a little lump of protoplasm, the cell body; in the centre of which, as in the centre of other cells, is a nucleus and nucleolus; starting out from the cell in all directions are numbers of processes like the branches of a tree, these branching processes are termed protoplasmic processes; they were once believed only to possess, like the roots of a tree, the power of absorbing nutrition and carrying the same to the cell to be utilised; we now know, also, that they serve to conduct nervous currents. There is, however, only one process of the cell which becomes a nerve fibre and arises as a cone-like swelling at the centre of the base of the little pyramid. After a short distance this fibre becomes insulated by a sheath of fatty substance termed myelin. Nervous currents when transmitted along the nervous elements are collected by the protoplasmic processes and transmitted to the cell, and thence outwards by the process which will become the core of a nerve fibre, so that the direction of the molecular vibration is always the same. Although there are countless myriads of these nervous units which are termed *neurones*, forming an inextricable network, yet when stained by the chromate of silver method invented by Professor Golgi, the innumerable delicate processes of these nerve cells are found not to be in continuity. Like the trees of a forest, there is contiguity but not continuity; each one is independent, and has an independent life and structure (Fig. 2).

The nervous system consists then of systems, groups, communities and clusters of these nervous elements called neurones, mingled with supporting tissue and blood vessels for their nutrition. The amount of blood in the grey matter is very much more abundant than in the white matter, indicating that the chemical changes incidental to vital activity take place there, rather than in the white matter. The processes of mind are inseparably connected with the functional

activities occurring in the cell bodies of the nervous units, but we know very little or nothing of the biochemical processes occurring in the neurones when we think, and feel, and move, and have our being. Some authorities presume to know the biophysical processes which take place, and I shall speak of these later on. One fact we do know, is, that if blood for a few seconds fails to reach the mantle of grey matter which covers the surface of the brain, there is loss of consciousness, as in fainting. Consciousness then depends upon the vital functions of these nervous elements in the grey rind or cortex of the brain. The physiologist Flourens taught that all parts of this grey substance were of equal value as regards function. This doctrine, however, was even worse than that of phrenology which it was directed against. The grey cortex or rind in the human brain is, as you observe, thrown into a number of folds by fissures (Fig. 4). These folds on the brain's surface become more numerous and complex the higher we rise in the zoological scale, until in a cultured European it finds its greatest development. Anatomically speaking, this increasing number of folds means an increase in number of the nervous units of the grey matter without an inconvenient increase in the size of the head. I hold up to you the brains of an idiot and a chimpanzee, you will observe there is not much difference. It has been shown that the intellectual faculties are more developed in persons with complex convolutions, and this means increased numbers of nervous units in the grey matter, especially in certain regions which I shall indicate to you later. With the death and disappearance of these nervous units paralysis of mind and body ensues.

Intellectual processes depend not so much upon relative increase of brain weight as increase of the superficial area of the mantle of nerve cells of certain regions. How has it been ascertained that certain portions of the brain have definite functions? The first step in this direction was the observation that persons who suffered with injury or disease of the left half of the brain were not only liable to paralysis of the right half of the body, but also to loss of speech. Subsequently Broca determined the exact portion of the brain which is connected with the function of articulate speech. Later it was shown that destruction of a certain region produced word deafness; a person would not understand the meaning of words although he could hear sounds. Further back still, there is a portion of brain which, if destroyed, is followed by word blindness, a person so affected would be able to converse, but would not be able to read aloud, although he is not blind. It is only written language which has no meaning to him. It is probable, therefore, that all right-handed people use the left half of the brain much more than the right. The whole of the central portion of the brain has been found by experiments on animals, and by applying that knowledge to diseases in man, to be tactile-motor in function—that is to say, it is the part of the brain by which voluntary movements are directed and controlled; it is also the part of the brain where sensations coming

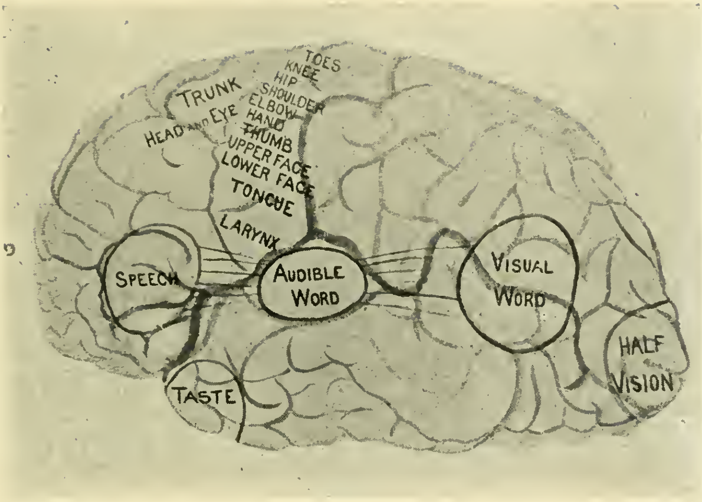


FIG. 4.

Diagram showing the various known Regions of the Brain having special functions. It will be observed that this is the left hemisphere, and that the various speech centres are represented, connected together by lines, indicating the paths of the association impulses. The portion marked *speech* is called Broca's convolution, and is connected with *articulate* speech.

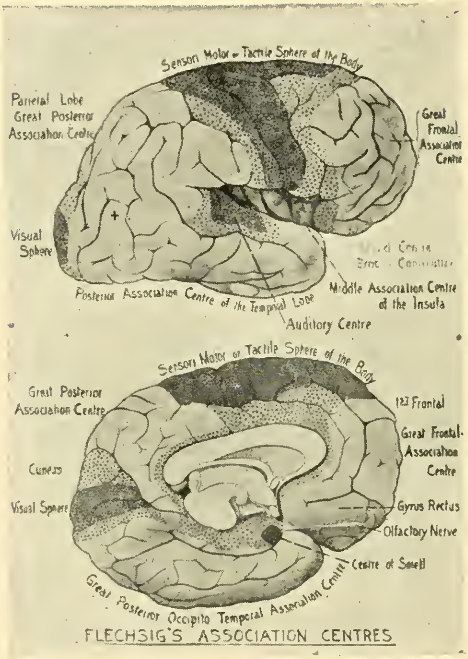


Fig. 5.

Diagrams of the Outer and Inner Surface of the Right Hemisphere of the Brain, showing the parts corresponding to Flechsig's association centres. All the portions dotted and dark indicate the situation of the projection system of neurones, afferent and efferent. Other portions, according to Flechsig, are concerned with the higher functions of mind and the simple psychical reflexes.

from the muscles and the surface of the body are received and rise into consciousness. It was found by Fritsch and Hitzig that this portion of the brain is excitable by electrical stimulation, and they were able to map out in the dog the whole excitable surface of the brain into definite areas, each area when electrically excited causing a definite representative movement. Subsequently, Ferrier showed the same in the monkey, which being nearer to man made the experiments of greater value. This discovery was of the greatest importance to the physiologist, physician and surgeon. It explained what Hughlings Jackson had previously observed and described, namely, the march of a fit due to local disease of this region of the brain. He had observed that from a definite focus of irritation the spread of the excitation as shown by the succession of definite movements that occur in this form of epilepsy always began in the same way, and gave rise to a definite series of movements of the same order and character. If you look at the diagram you will understand how a fit starting from irritation in the arm area will spread to the face area below, and to the leg area above, causing convulsive movements in the muscles. You will observe that other areas of the brain have also been investigated, and we now know that the posterior part of the brain, especially the inner surface, is connected with sight, another with hearing, and another with taste and smell (Figs. 4 and 5). In the dog the sense of smell is highly developed, and is the main avenue of intellectual experiences, and the incitement to volitional action; therefore the area of this sense in its brain is highly developed. In man, on the contrary, the senses of smell and taste, which stand like sentinels to guard the respiratory and alimentary systems, are little connected with intellectual faculties, and therefore not much developed. There is, however, yet a large portion of the brain, quite two-thirds in fact, to which I have allocated no special function.

Flechsig, by studying the development of the structures of the brain of the new born child and of infants of various ages, has shown that certain parts of the brain are prepared for functional activity before others. I will call your attention to the diagram Fig. 6, which represents a slice through the brain of a child at birth; only certain definite systems of fibres are insulated by a myelin sheath, and therefore ready to conduct nervous currents. For only those fibres which have a sheath of myelin are stained purple by his method, and thin slices of the brain can thus be made to reveal the parts which are prepared for conduction. These are all tracts of fibres which convey sensory impressions from the eye, skin, muscles, ears, nose and mouth to the very portions of the brain which I have already indicated to you that experiment and pathological observations have shown to be connected with the special sense functions. These sensory tracts developed first then, are the primary avenues of consciousness and of all the higher functions of the mind. All the

rest of the brain is asleep, waiting to be awakened by the sensory impressions from without. The base of the brain and its stalk, which is stained deeply purple, subserves the vegetative functions of life—breathing, circulation of the blood, swallowing, digesting, etc. The portions of the brain indicated by dots in the diagram (Fig. 5) form receiving stations for all the nervous currents subserving special and general sensibility; but by the side of these receiving stations for ingoing currents are developed transmitting stations for outgoing currents to the muscles, consequently we are not surprised to find in the diagram (Fig. 7) of a vertical slice of the brain of a child aged three months, evidence of the formation of this outgoing tract to the muscles. It is by this tract of fibres the infant commences to exercise volitional movements. You will see, moreover, that there are now developed fibres in the other regions of the brain; these are for the purpose of linking together and coordinating all the different sensory areas—association fibres as they are called. You will, moreover, see that the greater part of the surface of the brain is made up of these association systems, and in these regions there are, according to Flechsig, no sensory and motor nervous elements. Flechsig terms these portions of the brain association centres. They are situated in the frontal region, the temporal, occipital, parietal, and a small lobe which lies at the back of the large sylvian fissure called the island. Flechsig states that it is in these association centres that every sensation perceived leaves an ineffaceable imprint which constitutes memory. It is there in these higher centres that visual, auditory, tactile-motor and other sensations meet and fuse together. It is there they are compared one with another and to previous sensations. It is there the mind finds the indispensable elements for all the acts of intellectual or physical life. They form, in fact, the anatomical substratum of human experience, knowledge, language, sentiments and emotions. From the association centres nerve currents pass into the sensory sphere, controlling the lower centres of sensation and movement. It is generally believed that great development of the frontal region of the brain indicates high intellectual power. Observations have, however, been made which would seem to point to the great posterior lobe being connected with the higher intellectual functions of the mind. The brains of eighteen very distinguished men deceased were examined, and the essential features which were noted were extraordinary convolutional complexity and development of the posterior lobe. Recently the skulls of Beethoven and Bach have been examined, and measurements indicate that this portion of their brains must have had a great development.

In the nervous system we have three systems of nervous units or neurones. I will call your attention to the accompanying diagram (Fig. 8) which shows these systems, namely—1. Ingoing sensory; 2. Outgoing motor; and 3. Association. The latter form the great bulk of the nervous units of the brain. You will observe that in the

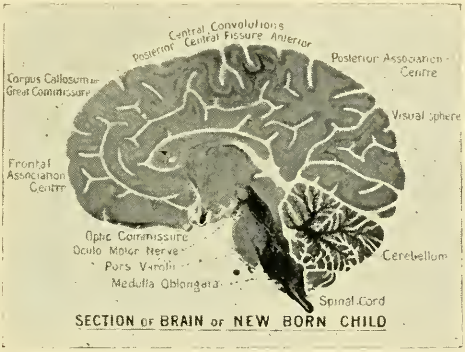


FIG. 6.

Diagram of Vertical Section through the Brain of a New-born Child, stained by a special method to show myelination of the fibres. All the parts which are dark contain myelinated fibres. Attention is particularly called to the rather faint deep staining about the central fissure, which corresponds to the tactile motor area. It will be observed that the association centres are not myelinated as in Fig. 7.

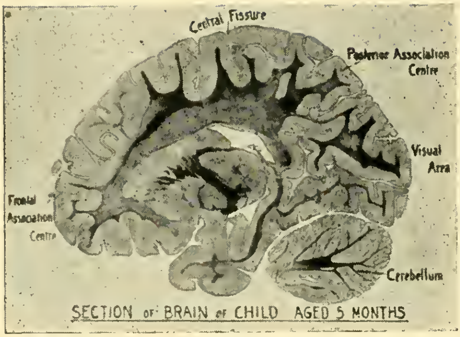


FIG. 7.

Diagram of Vertical Section of the Brain of a Child aged Three Months. The greater part of the brain now shows by the staining myelination of the white matter, showing development of the association centres.

projection systems the neurones are arranged in series, and that a sensation coming from the skin before it reaches the surface of the brain has to pass through a series of three neurones which are in physiological connection with one another, and the consciousness of the skin perception depends upon the intensity of the stimulus and the conducting capacity of the chain of nervous elements. You will

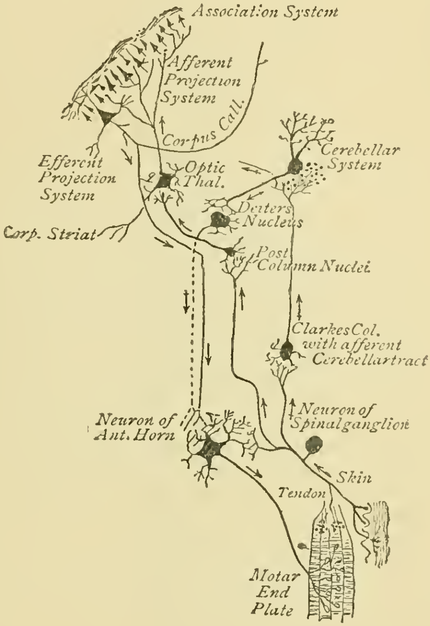


FIG. 8.

Diagram to show the Three Systems of Neurones, illustrating the path of sensory impulses to the Cerebrum and Cerebellum, the path of outgoing impulses from the Brain in a voluntary movement and the association of the afferent and efferent projection systems in the grey matter of the Brain. This diagram also illustrates the path of a simple spinal reflex and of a psychical reflex.

observe at right angles to the ingoing and outgoing fibres there are other fibres which join up these systems serving to coordinate the nervous currents arriving at and leaving this area of the brain: these are association fibres, so that if a skin sensation excites a conscious volitional movement, this will be the path of the nervous current.

If, however, we move our hand in response to some visual sensation, it is obvious that long association systems of neurones must

come into play to join up the visual area with the centre of movement of the hand, that is to say, that impulses must pass from the back to the middle of the brain. These conscious motor responses to sensory impressions are truly psychical reflexes, because the nervous current is bent back from the sensory ingoing system down along the motor system to the muscles.

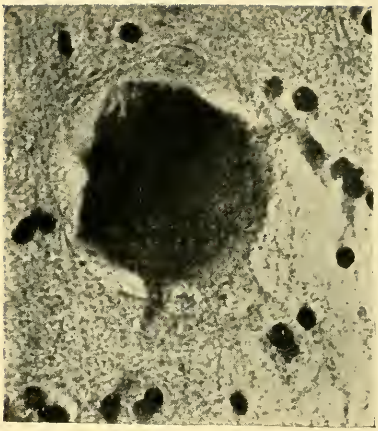
I have told you there is reason to believe that every neurone is an independent unit, and although its processes appear to enter into an inextricable network, yet there is really no continuity of structure. By inference we should believe this view to be correct, because if the whole of the nervous elements were connected together, diffusion of the nervous current through the whole substance of the brain would occur, and instead of orderly sequence, only confusion could arise in response to an external stimulus. If the neurones then are separate living units, can they by biochemical or biophysical processes promote or retard the transmission of currents along systems or between clusters, groups and communities? The method of contact between two neurones is always by the terminal arborisation of the nerve fibre process of one with the branching protoplasmic processes of another. Movements in the nerve cell of a minute aquatic animal having been observed under the microscope, it was conceived that the terminal twigs of the nerve fibre process might elongate, and come in better contact therefore with the protoplasmic processes of the next neurone of the series; and it has also been thought that sleep and unconsciousness from anæsthetics and narcotics, also trance and the hypnotic state, might be due to retraction of the terminal twigs of the sensory neurones on the surface of the brain so that contact is broken, and the transmission of nervous currents consequently interrupted.

It has been attempted to found a theory of retraction of the terminal buds or points of contact of the branching processes of the dendrons, by fixing in various fluids small pieces of the brain of animals which have been anæsthetised with chloroform and other anæsthetics and narcotics, and comparing the appearances presented by the dendrons with those presented by the brain of an animal killed suddenly. One set of observers finds retraction during narcosis, with the appearance of little moniliform swellings on the processes and disappearance of the gemmules. Another set finds no retraction of the gemmules, but retraction they say occurs when the brain is in activity; thus the facts are entirely opposed to one another. My observations, also those of my assistant Dr. Wright, who has given special attention to this subject, are opposed to the facts stated by the first set of observers (Figs. 2 and 9).

The whole difficulty lies in the fact that we are looking not at living matter but dead matter; still the theory of association and dissociation of groups or systems of neurones subserving special functions is an attractive theory for explaining the problems of

Fig. 9.

Photomicrograph, showing the Branching Processes of the Dendrons of the Psycho-motor Cells of the Cerebral Cortex of a Dog killed with chloroform; studded all over with little buds, which come into contact, but not connection, with fibres at right angles. Unfortunately the reproduction of the photograph is very indistinct. Magnified 200 diam.



Figs. 10 and 11.

Photomicrographs of two Psycho-motor Cells from the Human Cortex.

Fig. 10, healthy, shows the cell with processes and a mastic pattern, due to differentiation of the protoplasm of the cell into stainable and unstainable substances, seen in the picture as dark and light parts respectively.

Fig. 11 is a similar cell, from a patient who had died in status epilepticus. The cell is swollen up, the nucleus is nearly extruded, and there is no longer any differentiation in the protoplasm of the cell, showing a profound bio-chemical change due probably to the asphyxia.

repose and activity of the brain. I am inclined to believe that Lugaro's view is the better one: that cerebral activity is associated with a cutting off of the great majority of interneuronic connections, and the strengthening of the current traversing a few; that during repose or under narcotics there is a general expansion of the gemmules due to exhaustion of their contractility, and thus all the neurones being in contact, nervous currents are so diffused that they are not of sufficient intensity to rise into consciousness.

A new method of staining has revealed a fact concerning the internal constitution of the neurone. There are two biochemical substances entering into the formation of the neurone—a substance stainable by aniline dyes which exists in the body of the cell and upon the protoplasmic processes, and another unstainable substance which forms the framework of the cell, and consists of extremely delicate threads which pass into and form the processes. This latter is the essential conducting agent, and upon its integrity the life of a neurone depends. The stainable substance is probably a store of food or energy which is continually used up during functional activity, and replaced and stored up during rest from the surrounding lymph (Figs. 10 and 11).

The arrangement of this colouring matter in the cell serves as a means of distinguishing cells with different functions; for example, the large motor cells of the brain, with a characteristic arrangement of the stainable substance, are found only in the central regions of the cortex, and in the occipital lobe; they do not exist in the association centres. This is possibly a proof of relation of structure of the neurone to its function.

Very little is known of the chemical composition of the nervous elements, only that they are among the most complex bodies on our planet. When they die they split up into simple bodies, and this fact has served a most useful purpose in following the course taken by nerve fibres; e. g. if a motor nerve of the brain is separated from its cell by injury or disease, the fibre below the injury undergoes degeneration which can be recognised by the chemical changes occurring in the fibre during its destruction. We can follow this dead fibre to its ultimate destination by the microchemical reactions of the products of degeneration even though they extend for more than a yard in length. Besides this there is a change in the life of the cell body itself when the fibre is cut; for the stainable substance disappears, and only reappears if a new fibre grows out from the cell.

In this address I have attempted to give you a sketch of what we know and what we believe we know of the relation of the structure of the brain to its functions. The vast problems of mind still remain unsolved; but the conception of the neurone as an independent unit, a microcosm within a macrocosm, has shed a flood of light upon the problems of disease of the nervous system, and if further evidence is

forthcoming to support the theory of association and dissociation of the neurones either by biophysical or biochemical processes occurring at the terminal twigs of their tree-like processes, then we may be on the eve of great discoveries relating to the functions of the brain as an organ of mind. We must, however, not scoff at this theory because we cannot see this process taking place in the brain, but remember the phenomenal advances Chemistry made after the adoption of the atomic theory; and yet the chemist never can see or even conceive what an atom is.

[F. W. M.]

WEEKLY EVENING MEETING,

Friday, April 28, 1899.

Sir Edward Frankland, K.C.B. D.C.L. LL.D. F.R.S., Vice-President, in the Chair.

Professor C. A. Carus Wilson, M.A. M.Inst.E.E.

Some Features of the Electric Induction Motor.

The action of a magnetic field upon a conductor carrying an alternating current might be illustrated in a simple manner by placing an incandescent lamp in the neighbourhood of an electro-magnet and connecting the lamp to an alternating supply circuit. If the magnetic field were uniform, the filament would vibrate in front of the magnet, and if the vibrations could be minutely studied they would be found to increase first of all to a maximum in one direction, then fall to nothing, and then reach a maximum in the other direction, following an harmonic law. The variations of the force on the filament were similar to those which took place in a steam engine, where the turning moment on the crank went through a complete cycle from a positive to a negative maximum, the law of variation being harmonic if the steam pressure were constant and the connecting rod of infinite length.

The conditions under which the conductor vibrated could be shown more perfectly if it were possible to attach a mirror to the conductor and reflect a beam of light therefrom. But in order that the motion of the conductor should give a true indication of the force acting upon it, the moving part must have a very small periodic time and be perfectly dead-beat. These conditions were fulfilled in a remarkable way in the oscillograph recently designed by Mr. W. Duddell, who had kindly lent one of his instruments for the purpose of illustration. In this instrument the conductors were metal strips stretched in a magnetic field; a minute mirror fixed to the strips reflected a spot of light on to the screen. The delicacy of the arrangement was such that the period of vibration of the mirror was only the two-thousandth part of a second.

When an alternating current was passed through the strip, the magnetic field being constant, the spot of light on the screen assumed an up and down motion in a straight line. It was possible, however, to indicate the true nature of the law of motion by making the beam of light on its way to the screen strike on a mirror which moved to and fro synchronously with the variations of current in the strip. The spot then followed a wave-like course, tracing out an S-shaped

figure. If the spot moved quickly enough, the successive impressions would remain on the eye for a sufficient length of time to give the effect of a continuous line of light.

When examined in this way, the force due to a constant magnetic field and a variable current in the strip was seen to vary from nothing to a certain maximum, then to nothing again, then to change sign and increase to an equal negative maximum, and finally to fall to nothing. The strip was thus subjected to a series of equal impulses varying in sign so that the resultant impulse was zero.

To prevent the impulses from changing sign it was necessary to make the magnetism change sign at the same time as the current. This could be done by exciting the magnet by an alternating current in step with that in the strip. When this change was made, the spot of light was seen to fluctuate up and down from a fixed line, instead of alternating as before, showing that the strip was now subject to a series of fluctuating impulses. There were, however, dead-points at which the strip was not subject to any force. It appeared then possible to combine an alternating field with a conductor or conductors carrying an alternating current in such a way as to obtain a series of unidirectional impulses, and motors had been constructed on this principle. Such motors had the disadvantage of dead-points, and uniform motion could be obtained only by making the moving parts of considerable weight, so that part of each impulse was stored up as kinetic energy, and the dead-points thus successfully passed over.

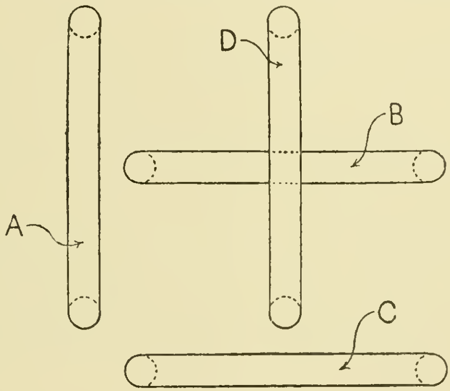
The dead-points could be avoided in a manner often used in steam engines, where two separate cylinders were set so that when the action of one on the crank shaft was a minimum, the action of the other was a maximum. This principle could be carried out far more completely in an electric motor than in any other form of motor for the reason that while in a steam engine, for instance, the force of each one of the two cylinders varied according to an harmonic law, with uniform steam pressure, and two series of harmonic impulses at right angles did not give a uniform turning moment, with the electric motor, on the other hand, the law of force variation with synchronously varying field and current was not an harmonic law—as might be seen by the shape of the curves on the screen—but a law having the remarkable peculiarity that two such series of impulses at right angles gave a uniform impulse when acting together. It was thus possible to construct an alternating electric motor in which the sum of the turning moments on the shaft was a constant quantity. The Induction Motor was such a motor.

Taking the fixed coil A, shown in plan in the figure, to represent the magnetising coil used in former experiments, and the moving coil B to represent the conductor or strip, the condition of a unidirectional fluctuating impulse from A to B was that the magnetism of A should be in step with the current in B. To produce the latter recourse might be had to the principle of induction, by which currents could

be induced in coils through which an alternating magnetic field was allowed to pass.

By way of illustration, coils A and B were placed opposite and parallel to one another, each being connected to a separate strip on the oscillograph, in which the magnetic field was kept constant. When an alternating current was passed through A, a current was induced in B, and the two currents were shown on the screen in the form of two curves overlapping each other. The explanation of the overlapping was that the induced current in B was not in step with the magnetism produced by A, but lagged behind it by an amount of time equal to a quarter of a period.

This lagging of the induced current behind the inducing magnetism had an important bearing on the motor problem, because it showed that it was not possible to make the magnetising coil A



induce in coil B a current suitable for producing a unidirectional series of fluctuating impulses, since the necessary condition was that the current in B should be in step with the magnetism produced by A.

If a second magnetising coil C could be used, in which the current differed from that in A by a quarter of a period, it could be made to induce currents in B which were in step with the magnetism produced by A. The intermediate coil B would then be subject to the influence of two coils A and C, of which the former produced an alternating magnetic field, and the latter an induced alternating current in step with that field, the two together giving a series of unidirectional fluctuating impulses.

The magnetising coil A exerted the greatest force on the coil B when the planes of the two coils were at right angles to each other, and the coil C induced the greatest current in B when their planes were parallel. Hence it followed that the best effect was obtained by

placing the planes of the coils A and C at right angles to one another, and B at right angles to A, as in the figure.

If a second intermediate coil D were placed across coil B, there would be currents induced in it by A, and a force exerted on it by the field due to C, resulting in a series of impulses similar to those acting on B, but differing from them by a quarter of a period. The sum of the two sets of impulses on the intermediate conducting system was a uniform twisting moment.

If the two coils B and D were replaced by a continuous conductor—such, for instance as a copper drum—this action could go on while the conducting system rotated. In the experiment shown, a copper drum was suspended in front of two coils placed at right angles, and excited by currents differing by one quarter of a period. When both coils were excited the drum rotated at a uniform speed.

A striking feature in the Induction Motor was the rotating magnetic field caused by the variations of magnetism in the different coils. This was illustrated by an experiment in which a small permanent magnet carrying a mirror was centred upon a vibrating rod placed between two pairs of electromagnets. When two opposite magnets were excited the mirror vibrated between them and reflected a spot of light on to the screen, tracing out a vertical line. When the second pair of opposite magnets was excited the spot traced out a horizontal line of equal amplitude. Both sets were then excited together by currents in step with each other, with the result that the spot vibrated in a line making an angle 45° with the former lines of vibration. When the exciting currents were made to differ by one quarter of a period, the spot of light reflected from the mirror described a circle, showing that the magnetic field produced by two magnets set at right angles and excited by two currents differing by a quarter of a period is of uniform intensity and rotates at a uniform rate.

The action of the Induction Motor was based on principles which governed the action of the better known transformer, and for purposes of calculation it was convenient to regard the Induction Motor as a transformer. One form only of the Induction Motor had been alluded to; of other possible forms that in which three magnetising coils were used, with currents differing from one another by one-third of a period was the one most commonly employed in practice.

ANNUAL MEETING,

Monday, May 1, 1899.

The Duke of Northumberland, F.S.A., President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1898, testifying to the continued prosperity and efficient management of the Institution, was read and adopted, and the Report on the Davy Faraday Research Laboratory of the Royal Institution, which accompanied it, was also read.

Fifty-eight new Members were elected in 1898.

Sixty-three Lectures and Nineteen Evening Discourses were delivered in 1898.

The Books and Pamphlets presented in 1898 amounted to about 271 volumes, making, with 657 volumes (including Periodicals bound) purchased by the Managers, a total of 928 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Honorary Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

President—The Duke of Northumberland, F.S.A.
Treasurer—Sir James Crichton-Browne, M.D. LL.D. F.R.S.
Secretary—Sir Frederick Bramwell, Bart. D.C.L. LL.D. F.R.S. M. Inst. C.E.

Managers.

Sir Frederick Abel, Bart. K.C.B. D.C.L. LL.D. F.R.S.
Sir William Crookes, F.R.S.
The Duke of Devonshire, K.G. M.A. D.C.L. LL.D. F.R.S.
The Right Hon. the Earl of Halsbury, M.A. D.C.L. F.R.S.
Donald William Charles Hood, M.D. F.R.C.P.
David Edward Hughes, Esq. F.R.S.
The Right Hon. Lord Kelvin, G.C.V.O. D.C.L. LL.D. F.R.S.
Alfred B. Kempe, Esq. M.A. Treas. R.S.
Hugh Leonard, Esq. M. Inst. C.E.
Sir Andrew Noble, K.C.B. F.R.S.
The Right Hon. The Marquis of Salisbury, K.G. M.A. D.C.L. LL.D. F.R.S.
Alexander Siemens, Esq. M. Inst. C.E.
Basil Woodd Smith, Esq, F.R.A.S. F.S.A.
William Hugh Spottiswoode, Esq. F.C.S.
Sir Henry Thompson, Bart. F.R.C.S. F.R.A.S.

Visitors.

William Henry Bennett, Esq. F.R.C.S.
Henry Arthur Blyth, Esq. J.P.
Maures Horner, Esq. J.P. F.R.A.S.
Edward Kraftmeier, Esq.
Lieut.-Colonel Llewellyn Wood Longstaff, F.R.G.S.
Frank McClean, Esq. M.A. LL.D. F.R.S. F.R.A.S.
Henry Francis Makins, Esq. F.R.G.S.
T. Lambert Mears, Esq. M.A. LL.D.
Rudolph Messel, Esq. Ph.D. F.C.S.
Lachlan Mackintosh Rate, Esq. M.A.
John Callander Ross, Esq.
William James Russell, Esq. Ph.D. F.R.S.
Alfred Gordon Salamon, Esq. F.C.S. F.I.C.
Sir James Vaughan, B.A. J.P.
John Jewell Vezey, Esq. F.R.M.S.

WEEKLY EVENING MEETING,

Friday, May 5, 1899.

Sir Frederick Bramwell, Bart., D.C.L. LL.D. F.R.S., Honorary Secretary and Vice-President, in the Chair.

William James Russell, Esq., Ph.D. V.P.R.S. *M.R.I.*

Pictures Produced on Photographic Plates in the Dark.

I think I may fairly assume that every one in this theatre has had their photograph taken, and consequently must have some idea of the nature of the process employed. I have, therefore, only to add, with regard to what is not visible in the process of taking the picture, that the photographic plate is a piece of glass or such like body, coated on one side by an adhesive paste which is acted on by light, and acted on in a very remarkable manner. No visible change is produced, and the picture might remain latent for years, but place this acted-on plate in a solution, of, say pyrogallol, and the picture appears. The subsequent treatment of the plate with sodium hyposulphite is for another purpose, simply to prevent the continuance of the action when the plate is brought into the light. Now, what I purpose demonstrating to you to-night is that there are other ways of producing pictures on photographic plates than by acting on them by light, and that by these other means a latent picture is formed, which is rendered visible in precisely the same way as the light pictures are.

The substances which produce on a photographic plate these results so strongly resembling those produced by light, are, some of them, metallic, while others are of vegetable origin. At first it seemed very remarkable that bodies so different in character should act in the same way on the photographic plate. The following metals—magnesium, cadmium, zinc, nickel, aluminium, lead, bismuth, tin, cobalt, antimony—are all capable of acting on a photographic plate. Magnesium most strongly, antimony but feebly, and other metals can also act in the same way, but only to a very slight extent. The action in general is much slower than that of light, but under favourable conditions a picture may be produced in two or three seconds.

Zinc is nearly as active as magnesium or cadmium, and is the most convenient metal to experiment with. In its ordinary dull state it is without the power of acting on a photographic plate, but scratch it or scrape it, and it is easy to prove that the bright metal is active. I would say that all the pictures which I have to show you, by means of the lantern, are produced by the direct action of the metal,

or whatever the active body may be, on the photographic plate, and that they have not been intensified or touched up in any way. This first slide is the picture given by a piece of ordinary zinc which has been rubbed with some coarse sand-paper, and you see the picture of every scratch. Here is a piece of dull zinc on which some circles have been turned. It was exposed to the photographic plate for four hours at a temperature of 55° C. In the other cases, which are on a larger scale, a zinc stencil was polished and laid upon a photographic plate, and you see where the zinc was in contact with the plate much action has occurred. In another case a bright zinc plate was used, and a Japanese stencil interposed between it and the photographic plate, and a very strong and sharp picture is the result. The time required to produce these zinc pictures varies very much with the temperature. At ordinary temperature the exposure would have to be for about two days, but if the temperature was, say, 55° C., then half to three-quarters of an hour might be sufficient. Temperatures higher than this cannot be used except for very short times, as the photographic plate would be damaged. Contact between the zinc and photographic plate is not necessary, as the action readily takes place through considerable distances. Obviously, however, as you increase the distance between object and plate, so you decrease the sharpness of the picture, as is shown by the following pictures, which were taken respectively at a distance of 1 mm. and 3 mm. from the scratched zinc surface. The appearance of the surfaces of different metals varies, and the following slides show the surface of a plate of bismuth, a plate of lead, and one of aluminium. On the next slide are the pictures produced by similar pieces of pure nickel and cobalt, and it clearly shows how much more active in this way nickel is than cobalt. Many alloys, such as pewter, fusible metal, brass, etc., are active bodies, and in the case of brass the amount of action which occurs is determined by the amount of zinc present. Thus you will see that a brass with 30 per cent. of zinc produces hardly any action on the photographic plate, but when 50 per cent. of zinc is present there is a fairly dark picture, and when as much as 70 per cent. is present a still darker picture is produced.

The second class of bodies which act in the same way on a photographic plate are organic substances, and belong essentially to the groups of bodies known as terpenes. In trying to stop the action of metallic zinc, which I thought at the time might arise from vapour given off by the metal, copal varnish was used, but in place of stopping the action it was found to increase it, and this increase of activity was traced to the turpentine contained in the varnish. In experimenting with liquids it is convenient to use small shallow circular glass vessels such as are made for bacteriological experiments, the plate resting on the top of the vessel, and the amount of liquid in the vessel determining the distance through which the action shall take place. The following slide, produced in this way, shows how dark a picture ordinary turpentine produces. All the terpenes are active

bodies. Dipentine is remarkably so; in a very short time it gives a black picture, and if the action be continued, the dark picture passes away, and you then have a phenomenon corresponding to what photographers call reversal. The strong smelling bodies known as essential oils, such as oil of bergamot, oil of lavender, oil of peppermint, oil of lemons, etc., are all active bodies, and all are known to contain in varying quantities different terpenes; therefore ordinary scents are active bodies, and this is shown by the following pictures produced by eau de Cologne, by cinnamon, by coffee, and by tea. Certain wines also act in the same way: Sauterne gives a tolerably dark picture, but brandy only a faint one. Other oils than these essential ones are also active bodies: linseed oil is especially so; olive oil is active, but not nearly as much so as linseed oil; and mineral oils, such as paraffin oil, are without action on the photographic plate.

Interesting results are obtained with bodies which contain some of these active substances; for instance, wood will give its own picture, as is shown by the following slides: the first is a section of a young spruce tree, the next a piece of ordinary deal, and the third of an old piece of mahogany. Again, the next slide you will recognise as the picture of a peacock's feather. There is much interest in these pictures of feathers, as they distinguish the brilliant interference colours from those produced by certain pigments; the beautiful blue in the eye of the peacock's feather is without action on the photographic plate. Butterflies' wings, at least some of them, will draw, as you see, their own pictures. Linseed oil, which is a constituent of all printing ink, makes it an active body, and it can, like the zinc and other active bodies, act through considerable distances. In the picture before you the ink was at a distance of one inch from the plate, and the next slide shows what a remarkably clear and dark picture ordinary printing can produce. As the composition of printing ink varies so does its activity, and here are pieces of three different newspapers which have acted under the same conditions on the same plate, and you see how different the pictures are in intensity. Printed pictures, of course, act in the same way—here is a likeness of Sir H. Tate taken from "The Year's Art." The pictures and printing in *Punch* always print well; so does the yellow ticket for the Friday evening lectures at the Royal Institution; also the rude trade-mark on Wills's tobacco, and it is of interest because the red pigment produces a very clear picture, but the blue printing is without action on the plate.

An interesting and important peculiarity of all these actions is that it is able to pass through certain media; for instance, through a thin sheet of gelatin. Here are two plates of zinc; both have been scratched by sand-paper; one is laid directly on the photographic plate, and the other one has a sheet of gelatin, its colour is of no note, laid between it and the sensitive plate; the picture in this case is, of course, not so sharp as when no gelatin is present, but it is a good and clear likeness of the scratches.

Celluloid is also a body which allows the action to pass through

it, as is seen in this picture of a piece of perforated zinc, a picture which was produced at ordinary temperatures. Gold-beaters' skin, albumen, collodion, gutta-percha, are also bodies which are transparent to the action of the zinc and the other active bodies. On the other hand, many bodies do not allow the transmission of the action through them; for instance, paraffin does not, and among common substances writing-ink does not, as is easily shown by placing ordinary paper with writing on it between the active body and the photographic plate. The active body may conveniently be either a plate of zinc or a card painted with copal varnish and allowed to dry, or a dish of drying oil. The picture of an ordinarily directed envelope shows this opacity of ink well. It is a property long retained by the ink, as this picture of the direction of a letter, written in 1801, shows; also this letter of Dr. Priestley's, dated 1795; and here is also some very faded writing of 1810, which still gives a very good and clear picture. Even if the writing be on parchment, the action passes through the parchment, but not through the ink, and hence a picture is formed.

With bodies which are porous, such as most papers, for instance, the action passes gradually through the interstices, and impresses the plate with a picture of the general structure of the intervening substance. For instance, the following pictures show the structure and the water-mark of certain old and modern writing-papers. Some modern writing-papers are, however, quite opaque; but usually paper allows the action to take place through it, and combining this fact with the fact of strong activity of the printing-ink, the apparently confused appearance produced on obtaining a picture from paper with printing on both sides is accounted for, as the printing on the side away from the photographic plate, as well as that next to it, prints through the paper, and is, of course, reversed.

I hope I have now given you a clear idea how a picture can be produced on a photographic plate in the dark, and the general character and appearance of such pictures. I now pass on to the important question of how they are produced. Moser suggested fifty years ago that there was "dark light," which gave rise to pictures on polished metallic plates, and lately it was suggested that pictures were produced by vapour given off by the metals themselves; the explanation, however, which I have to offer you is, I think, simpler than either of these views, for I believe that the action on the photographic plate is due to the formation of a well-known chemical compound, hydrogen peroxide, which undergoing decomposition acts upon the plate and is the immediate cause of the pictures formed. The complicated changes which take place on the sensitive plate I have nothing to say about on the present occasion, but I desire to convince you, that this body, hydrogen peroxide, is the direct cause of these pictures produced in the dark. Indirect proof has to be resorted to. Water cannot be entirely excluded, for an absolutely dry photographic plate would probably be perfectly inactive, and as long as water is present

peroxide of hydrogen may be there also. But what are the conditions under which these pictures are formed? Only certain metals are capable of producing them. This list of active metals which I have mentioned to you was determined solely by experiment, and when completed it was not evident what common property bound them together. Now, however, the explanation has come, for these are the very metals which most readily cause, when exposed to air and moisture, the formation of this body peroxide of hydrogen. Schönbein showed as long ago as 1860 that when zinc turnings were shaken up in a bottle with a little water hydrogen peroxide was formed, and the delicate tests which we now know for this body show that all the metals I named to you not only can in the presence of moisture produce it, but that their power of doing so follows the same order as their power of acting on a photographic plate. Again, what happened with regard to the organic bodies which act on the photographic plates? I have already mentioned that in experimenting with the metals it was accidentally observed that copal varnish was an active substance producing a picture like that produced by zinc, and that the action was traced to the turpentine present; again a process very much like groping in the dark had to be carried on in order to determine which were active and which inactive organic bodies, and the result obtained was that the active substances essentially belonged to the class of bodies known to chemists as terpenes. Now a most characteristic property of this class of bodies is that in presence of moisture and air they cause the formation of hydrogen peroxide, so that whether a metal or an organic body be used to produce a picture, it is in both cases a body capable, under the circumstances, of causing the formation of hydrogen peroxide. Passing now to experimental facts, which confirm this view of the action on sensitive plates, I may at once say that every result obtained by a metal or by an organic body can be exactly imitated by using the peroxide itself. It is a body now made in considerable quantity, and sold in solution in water. Even when in a very dilute condition it is extremely active. One part of the peroxide diluted with a million parts of water is capable of giving a picture. It can, of course, be used in the glass dishes like any other liquid, but it is often convenient not to have so much water present; and then it is best to take white blotting paper, wet it in the solution of the peroxide, and let it dry in the air. The paper remains active for about twenty-four hours; or, what is still better, take ordinary plaster of Paris, wet it with the peroxide solution, and let it set "in a mould" so as to get a slab of it. This slab increases in activity for the first day or two after making, and retains its activity for a fortnight or more. Such a slab will give a good and dark picture in three or four seconds.

To show how similar the pictures produced by the peroxide and those by zinc are, pictures of a Japanese paper stencil, which had been paraffined to make it quite opaque, have been made by both processes, and are shown with other instances in which turpentine

was used in the following slides. It is also very easy to obtain good pictures with the peroxide alone of the structure of paper, etc.; see, for instance, this one of a five-pound note and these of lace. Again, the strict similarity between the action of the peroxide and that of the metals and organic bodies is further shown by the fact that its action passes through the same media as their action does; and here are good pictures formed by the action of the peroxide after passing through a sheet of these substances. How this singular transmission can be explained, I have treated of elsewhere, and time does not allow of my discussing the matter to-night.

There are many ways in which the bright, active zinc surface can be modified. Draw your finger across it, press your thumb upon it, and you stop its activity, as is shown by the picture it will give. Lay a printed paper on the zinc, and let the contact continue for three-quarters of an hour, at a temperature of 55°, then bring the zinc in contact with a sensitive plate, a picture of the printing is formed, but allow the contact between the zinc and printing to continue for eighteen hours at the same temperature, and the picture then given by the zinc is the reverse of the former one. Where the ink has been is now less active than the rest of the plate. Here are slides which show these positive and negative pictures. Another way of modifying the zinc surface is interesting. You have seen that the ordinary zinc surface which has been exposed to air and moisture is quite inactive, but if a bright piece of zinc be immersed in water for about twelve hours, the surface is acted on; oxide of zinc is formed, showing generally a curious pattern. Now, if the plate be dried, it will be found that this oxide is strongly active, and gives a good picture of the markings on the zinc. The oxide evidently holds, feebly combined or entangled in it, a considerable quantity of the hydrogen peroxide, and it requires long drying or heating to a higher temperature to get rid of it. Also, if a zinc plate be attacked by the hydrogen peroxide, the attacked parts become more active than the bright metal. Thus place a stencil on a piece of bright zinc, and expose the plate to the action of an active plaster of Paris slab, or to active blotting-paper for a short time, then, on removing the stencil, the zinc plate will give a very good picture of the stencil. Any inactive body—for instance, a piece of Bristol board or any ordinary soft paper—can be made active by exposing it above a solution of peroxide, or, more slowly, by exposing it to a bright zinc surface. If, for instance, a copper stencil be laid on a piece of Bristol board, and a slab of active plaster of Paris be placed on the stencil for a short time, the Bristol board will even, after it has been removed from the stencil for some time, give a good picture of the stencil. Drying oil and other organic bodies may be used in the same way to change the paper. A curious case of this occurred in printing a coloured advertisement cut out of a magazine, for there appeared printing in the picture which was not in the original. This printing was ultimately traced to an advertisement on the oppo-

site page, which had been in contact with the one which was used; thus this ghostly effect was produced.

I believe, then, that it is this active body, hydrogen peroxide, which enables us to produce pictures on a photographic plate in the dark. There are many other curious and interesting effects which it can produce, and which I should like to have shown you, had time permitted.

I would only add that this investigation has been carried on in the Davy Faraday Laboratory of this Institution.

[W. J. R.]

GENERAL MONTHLY MEETING,

Monday, May 8, 1899.

His Grace The Duke of Northumberland, President, in the Chair.

Alfred Cooper, Esq. F.R.C.S.
Alfred William Porter, Esq. B.Sc.
S. Stephenson, Esq.
Theodore Uzielli, Esq.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Mr. Hugh Dewar for his donation of £200, to Mr. Thomas H. Sowerby for his donation of £5 5*s*., and to Mr. John H. Usmar for his donation of £50, to the Fund for the Promotion of Experimental Research at Low Temperatures.

The President referred to the death of Mr. Benjamin Vincent, for many years Assistant-Secretary to the Royal Institution, and said that the Managers desired to express the high appreciation in which they held his services to the Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Lords of the Admiralty—Greenwich Observations for 1896. 4to. 1898.
Greenwich Spectroscopic and Photographic Results for 1896 and 1897. 4to. 1898.
Annals of Cape Observatory, Vol. I. 4to. 1898.
Academy of Natural Sciences, Philadelphia—Proceedings, 1898, Part 3. 8vo. 1899.
Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. 1° Semestre, Vol. VIII. Fasc. 6, 7. 8vo. 1899.
American Academy of Arts and Sciences—Proceedings, Vol. XXXIV. Nos. 6–14. 8vo. 1898–99.
American Philosophical Society—Proceedings, No. 158. 8vo. 1898.
Asiatic Society, Royal—Journal for April, 1899. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. LIX. No. 6. 8vo. 1899.
Bankers, Institute of—Journal, Vol. XX. Part 4. 8vo. 1899.
Batavia, Magnetical and Meteorological Observatory—Observations, Vol. XX. 1897. 4to. 1898.
Rainfall in the East Indian Archipelago, Nineteenth Year, 1897. 8vo. 1898.

Birt, Sir William—Poppy Land. By C. Scott. 8th edition. 8vo. 1899.
Summer in Broadland. By H. M. Doughty. 6th edition. 8vo. 1899.
Handbook to the Rivers and Broads of Norfolk and Suffolk. By G. C. Davies. 31st edition. 8vo. 1899.
East Coast Scenery. By W. J. Tate. 8vo. 1899.
Vera in Poppy Land. By Mr. A. Berlyn. 8vo.
The Land of the Broads. By E. R. Suffling. New edition. 8vo.
Rambles in East Anglia. By H. Brittain. 3rd edition. 8vo. 1899.
How to Organise a Cruise on the Broads. By E. R. Suffling. 3rd edition. 8vo. 1899.
Boston Public Library—Monthly Bulletin, Vol. IV. No. 4. 8vo. 1899.
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Proceedings, Nos. 207, 208. 8vo. 1899.
Collective Index to Transactions, &c. 1873–1882. 8vo. 1899.
Cracovie, l'Académie des Sciences—Bulletin International, 1899, No. 3. 8vo.
Civil Engineers, Institution of—Minutes of Proceedings, Vol. CXXXV. 8vo. 1899.
Cutter, Dr. E.—Phonation. 8vo 1899.
Cutter, Dr. E. and Cutter, J. A. Esq.—Fatty Ills and their Masquerades. 8vo. 1898.
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WEEKLY EVENING MEETING,

Friday, May 12, 1899.

His Grace The Duke of Northumberland, K.G. F.S.A., President, in the Chair.

Professor Thomas Preston, M.A. D.Sc. F.R.S.

Magnetic Perturbations of the Spectral Lines.

The subject which we are about to consider this evening forms a connecting link between two of the most interesting branches of human knowledge—namely, that which treats of magnetism and that which treats of light. Almost as soon as the properties of magnets became known, mere curiosity alone must have prompted philosophers to ascertain if any relation existed between magnetism and "the other forces of nature," as they were generally termed. We are consequently led to expect, amongst the records of early experimental investigations, some accounts which treat of the action of magnetism on light.

When we seek for such accounts, however, we find that they are almost wholly absent from the literature of science; and this arises, I believe, from the great difficulty of the investigation and from the circumstance that only negative results were obtained, rather than that no such inquiry suggested itself or was undertaken. Even in quite recent times this inquiry has been prosecuted, but without success, by physicists who have published no account of their experiments. We may take it, therefore, that the inquiry is in itself an old one, although it is only now that it has been carried to a successful issue.

The earliest recorded attempt to solve this problem with which we are acquainted, is that of a celebrated British physicist whose name must for ever shed lustre on the annals of the Royal Institution —I speak of Michael Faraday. In order to understand the nature of the investigation which Faraday took in hand, and which has led up to the discourse of this evening, it is best to consider briefly some elementary facts concerning magnetism and light.

In the first place I shall assume that we know in a general way what the peculiarities of a body are which lead us to say that it is magnetised, or a magnet. These are that, when freely suspended, it sets itself in a definite direction over the earth's surface, as illustrated by the compass needle, and that in the space around it there is "magnetic" force exerted on pieces of iron, and in a smaller degree on other substances. For this reason we say that a magnet is surrounded by a magnetic field of force. The field of force is simply the space surrounding the magnet, and it extends to infinity in all directions from the magnet. Near the magnet the force is strong,

and far away from it the force is almost insensible; and so we say that the field is strong at certain places near the magnet, and that it is weak at places far away from the magnet. The direction of the force at any point is the direction in which the north pole of another magnet would be urged if placed at that point, and the push which this pole experiences may be taken to represent the intensity or strength of the magnetic field at the point in question. This is represented diagrammatically by these drawings [referring to figures suspended before the audience], which show roughly the nature of the field of force surrounding an ordinary bar magnet, a horse-shoe magnet, and the much more powerful form—the electro-magnet. It will be seen that the space outside the iron is filled with a system of curved lines running from the north pole to the south pole of the iron core. Where the lines are closest together there the magnetic force is strongest, and the direction of a line at any point is the direction of the resultant magnetic force at that point—that is, the direction in which a north pole would be urged if placed at that point.

Faraday always pictured the magnetic field as filled with lines of force in this way, and the importance of the conception can scarcely be over-rated, for it leads us to view the magnetic action as being transmitted continuously through the intervention of some medium filling all space, rather than by the unintelligible process of direct action at a distance. This medium is called the ether; but as to what it is that is actually going on in the ether around a magnet, we cannot definitely say. It may be that there is a flow of ether along the lines of magnetic force, so that there is an out-flow at one end of the magnet and an in-flow at the other, or it may be that the ether is spinning round the lines of force in the magnetic field. For our present purpose it is not a matter of very much importance what the exact condition of the ether may be in a magnetic field, for if the ether in a magnetic field is either in some peculiar condition of strain or of motion, and if light consists of an undulatory motion propagated through this same ether, then it may be naturally expected that some action should take place when light is propagated through, or radiated in, a magnetic field of force. This is what Faraday suspected; and in order that we may appreciate the problem with which he had to deal, let us place ourselves in his position and ask ourselves the question: "In what manner can we test experimentally if there is any magnetic action on light?"

In answer to this question, the first thing that occurs to us is to pass a beam of ordinary light through the magnetic field, in some chosen direction, and examine by all the means at our disposal if any action has taken place. When this is done we find that no observable effect is produced. But the scientific investigator does not rest satisfied with one negative result. He varies the conditions of the experiment, and returns to the attack with renewed vigour and hopes. In our first trial we passed a beam of light through the air-filled space around the magnet, and we may vary this experiment

either by removing the air altogether, and so causing the beam to traverse a vacuum, or we may replace the air by some dense transparent substance such as glass or water. Under these new conditions we still fail to detect any influence of the magnetic field on a beam of ordinary light. This negative result might arise from the field of force being too weak to produce an observable effect, or it might be that the effect (if any effect really does exist) may be of such a character that it is impossible to detect it with ordinary light. In common light the vibrations take place indifferently in all directions around the ray, and follow no law or order as to their type. They possess no permanent relation to any direction around the ray, so that if the magnetic action should happen to be a twisting of the vibrations round the ray, it will be impossible to detect this twist in the case of ordinary light.

As a matter of fact it is a twist of this kind that actually happens, and this is probably what Faraday anticipated. In order to detect it, therefore, it is necessary to employ a beam of light in which the vibrations are restricted to a single plane passing through the ray. Such light is said to be plane-polarised, and may be obtained by transmitting common light through a doubly refracting crystal. Faraday found that when a beam of this plane-polarised light is passed through the magnetic field, in the direction of the lines of force, a distinct effect takes place, and that the effect is a twisting of the plane of polarisation of the light vibrations as they pass through the magnetic field—or, to be more precise, as the light passes through the matter occupying the field.

This is the Faraday effect. Its magnitude depends on the strength of the field and upon the nature of the matter, through which the light passes in that field. This latter is an important fact that should not be lost sight of in reasoning upon the nature of this effect. The presence of matter in the field appears to be necessary. The effect is not observed in a vacuum, but becomes greater as the field becomes filled with matter of greater density. It is, therefore, not a direct action of the magnetic field on the light vibrations, but rather an indirect action exerted through the intervention of the matter which occupies the magnetic field.

This action, as we have said, is a rotation of the plane of polarisation of the beam of light, and it arises from the circumstance that, in passing through the magnetic field, vibrations which take place from right to left do not travel forward with the same velocity as those which take place from left to right. There is no change in the periods of the vibrations: it is essentially a change of velocity of propagation that occurs. If we examine the transmitted light with a spectroscope, we find that the wave-lengths are unaltered, but that the amount of rotation of the plane of polarisation is different for waves of different lengths. The law which governs the effect is that the rotation of the plane of polarisation varies inversely as the square of the wave-length of the light employed.

You will have noticed that in the foregoing experiment the source of light was placed quite outside the field of magnetic force, while the beam of light was transmitted through the field for examination. Now we might place the source of light itself in the magnetic field, and then examine if the light emitted by it is in any way affected by the magnetic force. This variation of the experiment suggests itself at once, and was indeed also tried by Faraday—in fact it formed his last experimental research of 1862, but without success. The same experiment has been tried, no doubt, by many other physicists, with the same negative result.

The first recorded success, or at least partial success, was by M. Fievez in 1885. He placed the source of light—a gas flame impregnated with sodium vapour—between the pole-pieces of a powerful electro-magnet. This being done, the light radiated by the flame was passed through the slit of a highly dispersive spectroscope and examined. What M. Fievez observed was that the bright spectral lines became broadened by the action of the magnetic field on the radiating source. His account is, perhaps, somewhat confused, owing to his imperfect apprehension of the true nature of the phenomenon which he observed; but, without doubt, he observed a true magnetic effect on the radiated light—namely, this broadening of the spectral lines. But he did not convince the scientific world that he had made any new discovery, and so the matter fell into neglect until it was revived again in 1897 by the now celebrated work of Dr. P. Zeeman.

The credit which attaches to Dr. Zeeman's work is that he not only, after prolonged effort, succeeded in obtaining this new magnetic effect, but he also convinced the world that the effect was a true one, arising from the action of the magnetic field on the source of light. That Dr. Zeeman was able to do this was due, perhaps, as much to the present advanced state of our theoretical knowledge of this subject as to his own skill and perseverance as an observer; and this is a striking example of the great assistance which well-founded theory affords to experimental investigation. The theory connects the facts already known in reasonable and harmonious sequence, predicts new results, and points out the channels through which they must be sought. Without such scientific theory this general systematic advance would be impossible, and new results would be stumbled on only by accident.

To see how this applies to our case, we revert to the fact determined by Dr. Zeeman—namely, that when the source of light is placed in a strong magnetic field the spectral lines become broadened. [Slide shown here.] As soon as this was announced Professor Lorentz, and subsequently Dr. Larmor, examined the question from the theoretical point of view. They analysed the subject mathematically, and came to the conclusion that each spectral line should be not merely broadened, but should be actually split up into three —that is, each line should become three lines, or, as we shall say in future, a triplet. They also arrived at the further most important

and interesting conclusion, viz. that the constituent lines of this triplet must be each plane-polarised—the central line of the triplet being polarised in one plane, while the side lines are polarised in a perpendicular plane. In fact the vibrations of the light forming the central line are parallel to the lines of magnetic force, while the vibrations in the side lines are perpendicular to the lines of force. This prediction of tripling and polarisation from theoretical considerations may be regarded as the key to the subsequent advance that has been made in the investigation of this region of physics. In order to understand it, let us place ourselves in Dr. Zeeman's position when he found that the spectral lines became broadened by the magnetic field, and let us be informed that this broadening is in all probability a tripling of the lines accompanied by plane-polarisation. The question now is, "How are we to determine if this is the case?"

It is clear that if the broadened line is really a triplet, then the components of this triplet must be so close together that they overlap each other, and so appear to the eye merely as one broad line, as illustrated by the model which is here before you. [Model illustrating the overlapping shown here.] We know that the spectral lines are not infinitely narrow lines, but are really narrow bands of light of finite width, and consequently we are quite prepared to regard the magnetically broadened line as an overlapping triplet; but we cannot remain satisfied until we have proved beyond all doubt that it really is a triplet, and not merely a single broad line. To do this, Dr. Zeeman made use of the second prediction of the theory—namely, that the constituents of the triplet must be plane-polarised. If this is so, then the outer edges of the broadened line must be plane-polarised, and therefore by introducing a Nicol's prism into the path of the light it must be possible to turn the Nicol so that the plane-polarised edges shall be cut off, and the breadth of the line shall be reduced to its normal amount. In fact, in this position of the Nicol the outside lines of the triplet are extinguished, and the central component alone remains. This component is, of course, the same in width as the original line, and consequently when the outer members of the triplet are extinguished all the magnetic broadening of the line is removed. When the Nicol is turned through a right angle the central component of the triplet is extinguished, while the side lines remain; and, if these side lines are sufficiently separated so that they do not overlap, then, when the central line is removed, a narrow dark space will exist between the side components, which represents the space intervening between the outer members of the triplet, as illustrated by this photograph. [Slide shown.]

But even though we may be able to so increase the strength of the magnetic field that when the central component of the triplet is removed by a Nicol the side lines stand apart with a clearly defined interval between them, yet this in itself does not absolutely satisfy us that the broadened line is a triplet. It might be contended that

the broadened line is not really a triplet, but is merely a band of light polarised in one plane along its edges and in the perpendicular plane along its centre, and that increase of the magnetic field might never separate it into distinct constituents, but merely continue to broaden it. This contention, however, might be disposed of by a careful study of the facts, even though we might not be able to produce a magnetic field strong enough to completely separate the constituent lines of the triplet.

But clearly the thing to be arrived at is to so arrange matters—in fact, to so design our electro-magnet and to plan the conditions of our experiment—that the magnetic field acting on the source of light shall be strong enough to completely separate the members of the triplet, if such exist. You will understand that this is no easy thing to do when you remember that it was only after repeated efforts and many failures that even a slight broadening of the spectral lines was obtained. Nevertheless, in spite of the great difficulty which besets this investigation, and which arises from our inability to obtain a magnetic field of unlimited strength, yet, with a properly designed magnet and other properly arranged conditions, it is possible to obtain a magnetic field strong enough to completely separate the constituents of the magnetic triplet, and thus to prove that the prediction of theory is verified by the actual facts. [Slide shown.]

But with a magnetic field of great strength the facts as shown by these slides [photographs shown here] turn out to be more complicated and more interesting than the simple theory led us to expect. For while some of the spectral lines are split up into triplets as indicated by theory, some on the other hand become resolved into sextets, or octets, or other complex types. [Slides shown here.] Thus, when the magnetic field becomes sufficiently intense, we realise to the full all the theoretical predictions and more. The reason of this surplus of realisation over expectation lies in the fact that the theory in its simplest form deals only with the simplest types of motion under the simplest conditions, and the conclusions arrived at are of course of corresponding simplicity. When more complicated types of motion are contemplated, the theory furnishes us with the dynamical explanation of the more complicated types of effect produced by the magnetic field. That tripling pure and simple should occur in the case of every spectral line (as predicted by the simplest form of theory) is not a result which we should expect from a broader consideration of the problem. In fact, if we reflect on the subject, we are forced to the conclusion that deviations from the pure triplet type should be expected, and, as we have seen, such deviations actually do occur. In this respect, therefore, the experimental investigation which yields more than the simple theory expected is not to be taken as in any way discordant with that theory, but, on the contrary, to be in harmony with it.

In order that you may form some idea as to what it is that the theory supposes to be in operation in the production of these pheno-

mena, I have had this elliptic frame constructed [model shown], which I ask you for the present to consider as the orbit described by one of those elements of matter which by their motions set up waves in the ether, and thereby emit what we call light. This white ball, which slides on the elliptic frame, is supposed to represent the element of matter. It is sometimes called an ion, which name is used to imply that the element of matter carries an electric charge inherently associated with it.

Now, under ordinary circumstances this ion revolving in its orbit with very great rapidity will continue to do so peacefully, unless external forces come into play to disturb it. When external forces come into action the orbit ceases in general to be the same as before. The orbit becomes perturbed, and the external forces are termed perturbing forces. But you now ask, What is the character of the forces introduced by the magnetic field when the ion is moving through it? In answering this, we are to remember that the ion is supposed to be an element of matter charged with an electric charge—or, if you like, an electric charge possessing inertia. Now, if a charged body moves through a magnetic field, it is an experimental fact that it experiences a force arising from the action of the magnetic field on the moving electric charge. The direction of this force is at right angles both to the direction of motion of the charged body and to the direction of the magnetic force in the field. The effect of this force in our case is to cause the elliptic orbits of the ions to rotate round the lines of magnetic force; or to cause them to have a precessional motion [illustrated by model] instead of staying fixed in space, just as the perturbing forces of the planets in the solar system cause the earth's orbit to have a precessional motion. The angular velocity of this precessional motion is proportional to the strength of the magnetic field, and depends also, as you would expect, on the electric charge and the inertia associated with the ion.

This precessional motion of the orbit, combined with the motion of the ion around the orbit, gives the whole motion of the ion in space, and the result of this combined movement, of these two superposed frequencies—viz. the frequency of revolution of the ion in its orbit, and the frequency of rotation of the orbit around the lines of force—is that, in the case of the light radiated across the lines of force, each period becomes associated with two new periods, or, in other words, each spectral line becomes a triplet. A partial analogue to this, which may to some extent help you to understand the introduction of the two new periods, occurs in the case of sound, although the two phenomena at basis are quite different. The analogue (or quasi-analogue) is this. When two notes of given pitch, that is of given frequency of vibration, are sounded together, their superposition produces two other notes of frequencies which are respectively the sum and the difference of the frequencies of the two given notes. These are known as the summation and the difference tones of the two given notes. Corresponding to these are the two side lines of the

magnetic triplet. The frequency of the vibration in one of these lines is the sum, and the frequency of the other is the difference, of the two frequencies mentioned before—namely, the frequency of the revolution of the ion around its orbit, and the frequency of the precessional revolution of the orbit round the lines of force. The centre line of the triplet has the frequency of the original vibration, and this frequency disappears completely when the light is viewed along the lines of force—that is, through axial holes pierced in the pole-pieces. In this direction, too, a further peculiarity arises, for not only does the triplet drop its central member and become a doublet, but each member of this doublet is not plane-polarised, as the members of the triplet are. They are each, on the contrary, circularly polarised—that is, the vibration is circular instead of being rectilinear.

This all follows as the expectation of the simple theory which supposes that the ions are free to describe their elliptic orbits undisturbed by any forces other than the magnetic field. But it is only to be expected that other perturbing forces must come into play in the assemblage of ions which build up incandescent matter of the source of light. We know, for example, that the other members of the solar system perturb the earth's motion, so that it deviates from the simple elliptic motion predicted by the simple theory which did not take these perturbing forces into account. Hence, if any such perturbing forces exist, and we should be surprised if they did not exist, the tripling pure and simple of the spectral lines will be departed from, and other types will arise. From the character of these new types we may infer the nature of the perturbations which give rise to them, and hence by the study of these types we obtain a view of what is going on in matter when it is emitting light, which we should not possess if such perturbations did not occur. These deviations from pure tripling are consequently of more importance almost, in regard to our future progress, than the discovery of the tripling itself. To give you some idea of the influence of such perturbations in modifying the triplet form, I may mention that it follows from simple theoretical considerations, that if the perturbing forces cause the orbit to revolve in its own plane, or cause it to change its ellipticity periodically, then each line of the triplet produced by the magnetic field will be doubled, and a sextet will result, and other oscillations of the orbit will give rise to other modifications of the normal triplet type. It is not quite easy to see at once, however, what the perturbing forces are exactly, for we do not know the way in which the ions are associated in matter; but if we regard an ion as a charged element of matter describing an orbit, it will be analogous to a closed circuit, or to a magnetic shell, and will be urged to set in some definite way in the magnetic field. In coming into this position it may oscillate about the position of equilibrium, and thus introduce an oscillation into the precessional motion of the orbit, which may have the effect of doubling or tripling the constituents of the pure precessional triplet.

Now, experimental investigation shows us that all the spectral lines do not become triplets when viewed across the lines of force in a magnetic field, for some lines show as quartets, or sextets, or octets, or in general as complex triplets derived from the normal triplet by replacing each component by a doublet or a triplet. We conclude, therefore, that the ions which give rise to these complex forms are not perfectly free in their motions through the magnetic field, but are constrained in some way by association with each other in groups, or otherwise, while they move in the magnetic field.

And now we come to a very important point in this inquiry. According to the simple theory every spectral line, when viewed across the lines of force, should become a triplet in the magnetic field, and the difference of the vibration frequency between the side lines of the triplet should be the same for all the spectral lines of a given substance. In other words, the precessional frequency should be the same for all the ionic orbits, or the difference of wave-length $\delta \lambda$ between the lateral components of the magnetic triplet should vary inversely as the square of the wave-length of the spectral line under consideration. Now, when we examine this point by experiment, we find that this simple law is very far from being fulfilled. In fact, a very casual survey of the spectrum of any substance shows that the law does not hold even as a rough approximation; for, while some spectral lines show a considerable resolution in the magnetic field, other lines of nearly the same wave-length, in the same substance, are scarcely affected at all. This deviation is most interesting to those who concern themselves with the ultimate structure of matter, for it shows that the mechanism which produces the spectral lines of any given substance is not of the simplicity postulated in the elementary theory of this magnetic effect.

Our previous knowledge of the line spectra of different substances might indeed have led us to suspect some such deviation as this from the results predicted by the simple theory. For if we view the line spectrum of a given substance we find that some of the lines are sharp while others are nebulous or diffuse, and that some are long while others are short—in fact, the lines exhibit characteristic differences which lead us to suspect that they are not all produced by the motion of a single unconstrained ion. On closer scrutiny they are seen to throw themselves into natural groups. For example, in the case of the monad metals (sodium, potassium, etc.), the spectral lines of each metal form three series of natural pairs, and again, in the case of the diad group (cadmium, zinc, etc.), the spectrum of each shows two series of natural triplets, and so on.

Thus, speaking generally, the lines which form the spectrum of a given substance may be arranged in groups which possess similar characteristics as groups. Calling the lines of these groups A_1, B_1, C_1 . . ., A_2, B_2, C_2 . . ., A_3, B_3, C_3 . . . we may regard the successive groups as repetitions of the first, so that the A's—that is A_1, A_2, A_3, &c.—are corresponding lines produced probably by the same ion; while the B's—namely, B_1, B_2, B_3, &c.—correspond to one

another and are produced by another ion, and so on. This grouping of the spectral lines has been noticed in the case of several substances, and it has been a subject of earnest inquiry amongst spectroscopists for some time past. All such grouping, however, up to the present, has had to depend on the judgment of the observer as to certain similarities in the general character and arrangement of the lines, and similarities which indeed may or may not have any specific relation to the mechanism by which the lines are produced. In fact, such grouping has been effected by guess-work, or by empirical formulæ, and we need not be surprised if it is found that the groups so far obtained are more or less imperfect.

I introduce this grouping of the spectral lines to your notice in order that we may attack the problem of reducing to order the so far apparently lawless magnetic effect. As I have already mentioned, the lines in the spectrum of any given substance are not all resolved into triplets by the magnetic field, but some are resolved into triplets while others become sextets, etc.; and further, the magnitude of this resolution, that is the interval $\delta\lambda$ between the lateral components does not appear at first sight to obey any simple law.

According to the prediction of the simple theory the separation $\delta\lambda$ should be proportional to λ^2, and although this law is not at all obeyed, if we take all the lines of the spectrum as a single group, yet we find that it is obeyed for the different groups if we divide the lines into a series of groups. In other words, the corresponding lines A_1, A_2, A_3, etc. have the same value for the quantity e/m,* or, as we may say, they are produced by the motion of the same ion. The other corresponding lines, B_1, B_2, B_3, etc. have another common value for e/m, and are produced therefore by a different ion, and so on. We are thus led by this magnetic effect to arrange the lines of a given spectrum into natural groups, and from the nature of the effect we are led to suspect that the corresponding lines of these groups are produced by the same ion, and therefore that the atom of any given substance is really a complex consisting of several different ions, each of which gives rise to certain spectral lines, and these ions are associated to form an atom in some peculiar way which stamps the substance with its own peculiar properties.

In order to illustrate the meaning of this, let us consider the spectrum of some such metal as zinc. The bright lines forming the spectrum of this metal arrange themselves to a large extent in sets of three—that is, they group themselves naturally in triplets. Denoting these triplets in ascending order of refrangibility by A_1 B_1, C_1, A_2, B_2, C_2, etc. we find that the lines A_1, A_2, etc. show the same magnetic effect in character, and have the same value of e/m, so that they form a series obeying the theoretical law deduced by Lorentz and

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* The quantity e is the electric charge of the ion, and m is its inertia, and the ratio e/m determines the precessional frequency, or spin, of the ionic orbit round the lines of magnetic force in a given field.

Larmor. In the same way the lines B_1, B_2, B_3, etc., form another series which also obeys the theoretical law, and possess a common value for the quantity e/m, similarly for the lines C_1, C_2, C_3, etc. The value of e/m for the A series differs from that possessed by the B series or the C series, and this leads us to infer that the atom of zinc is built up of ions which differ from each other in the value of the quantity e/m, and that each of these different ions is effective in producing a certain series of lines in the spectrum of the metal. When we examine the spectrum of cadmium, or of magnesium—that is, when we examine the spectra of other metals of the same chemical group—we find that not only are the spectra homologous, not only do the lines group themselves in similar groups, but we find in addition that the corresponding lines of the different spectra are *similarly* affected by the magnetic field. And further, not only is the character of the magnetic effect the same for the corresponding lines of the different metals of the same chemical group, but the actual magnitude of the resolution as measured by the quantity e/m is the same for the corresponding series of lines in the different spectra. This is illustrated in the following table, and leads us to believe, or at least to

Magnetic effect.	Nonets or complex triplets.	Sextets.	Triplets.
Cadmium .. .. $\lambda =$	5086	4800	4678
Zinc .. .. $\lambda =$	4811	4722	4680
Magnesium .. .. $\lambda =$	5184	5173	5167
Precessional spin (approx.) ..	$e/m = 55$	$e/m = 87$	$e/m = 100$

[This table shows the effect for the three lines which form the first natural triplet in the spectrum of cadmium compared with the corresponding lines in the spectra of zinc and magnesium. It will be seen that the corresponding lines in the different spectra suffer the same magnetic effect both in character and magnitude. Thus the corresponding lines 4800, 4722, 5173 are each resolved into sextets, and the rate at which the ionic orbit is caused to precess is the same for each (denoted by $e/m = 87$ in the table). Similarly for the other corresponding lines.]

suspect, that the ion which produces the lines A_1, A_2, A_3, etc., in the spectrum of zinc is the same as that which produces the corresponding series A_1, A_2, A_3, etc., in cadmium, and the same for the corresponding sets in the other metals of this chemical group. In other words, we are led to suspect that not only is the atom a complex composed of an association of different ions, but that the atoms of those substances which lie in the same chemical group are perhaps built up from the same kind of ions, or at least from ions which possess the same e/m,

and that the differences which exist in the materials thus constituted arises more from the manner of association of the ions in the atom than from differences in the fundamental character of the ions which build up the atoms; or it may be, indeed, that all ions are fundamentally the same, and that differences in the value of e/m, or in the character of the vibrations emitted by them, or in the spectral lines produced by them, may really arise from the manner in which they are associated together in building up the atom.

This may be an unjustified speculation, but there can be no doubt as to the fascination which enquiry of this kind has always exerted, and must continue to exert, over the human mind. It is the speculation of the ignorant as well as of the philosophic and trained scientific mind, and even though it should never be proved to rest on any substantial basis of fact, it will continue to cast its charm over every investigator of nature.

It is ever the desire of the human mind to see all the phenomena of nature bound by one connecting chain, and the forging of this chain can be realised only gradually and after great labour in the laboratories of science. From time to time the hope has been entertained that metals may be transmuted, and that one form may be converted into another; and although this hope has been more generally nurtured by avarice and by ignorance rather than by knowledge, yet it is true that we never have had any sufficient reason for totally abandoning that hope, and even though it may never be realised that in practice we shall be able to convert one substance into another, even though the philosopher's stone be for ever beyond our grasp, yet when the recent developments of science, especially in the region of spectrum analysis, are carefully considered, we have, I think, reasonable hope that the time is fast approaching when intimate relations, if not identities, will be seen to exist between forms of matter which have heretofore been considered as quite distinct. Important spectroscopic information pointing in this same direction has been gleaned through a long series of observations by Sir Norman Lockyer on the spectra of the fixed stars, and on the different spectra yielded by the same substance at different temperatures. These observations lend some support to the idea, so long entertained merely as a speculation, that all the various kinds of matter, all the various so-called chemical elements, may be built up in some way of the same fundamental substance; and it is probable that this protyle theory will, in one form or another, continue to haunt the domains of scientific thought, and remain a useful and important factor in our progress, for all time to come.

Even though it may be that a knowledge of the ultimate constitution of matter must for ever remain a sealed book to our enquiries, yet, framed as we are, we must for ever prosecute the extension of our knowledge in every direction; and in pursuing knowledge it frequently happens that vast acquisitions are made through channels which at first seem most unlikely to lead us any further. It has

frequently happened that small and obscure effects, obtained after much labour and difficulty, have led to results of the highest importance, while very pronounced and striking effects which have forced themselves on the attention of the observer have proved comparatively barren. It was by a determined effort of this kind, founded on a correct appreciation of the importance of small outstanding differences—so small as to be despised or passed over by all other observers—that Lord Rayleigh discovered a new gas in our atmosphere, added argon to our list of elements, and initiated the attack which led to the brilliant capture by Prof. Ramsay of several new terrestrial substances.

Viewed from this standpoint I hope I am to some extent justified in occupying your attention this evening with the consideration of the action of magnetism on light, for although the effect produced is small and not easy to observe, yet it is likely to prove an important instrument of research in the study of matter, and it is not inappropriate that a public account of what has been already achieved should be given in this Institution, in which the enquiry was first begun by Faraday, and in which his spirit still lives.

[T. P.]

WEEKLY EVENING MEETING,

Friday, May 19, 1899.

The Duke of Northumberland, K.G. F.S.A., President, in the Chair.

The Right Rev. The Lord Bishop of Bristol.

Runic and Ogam Characters and Inscriptions in the British Isles.

I am frequently surprised by the ignorance which I find of the mere existence of such a thing as an Ogam inscription, and of the meaning of the phrase "a Runic inscription." My business this evening is to give some elementary information on both of these subjects.

The Runic alphabet was the character in which our earliest Anglian ancestors wrote their own language. The use of this character practically died out early, under the influence of the Latin and the Scottish missionaries, who were ignorant of runes, and probably suspected them of paganism and sorcery. Bede refers to runes once, and in that kind of connection. He tells us that a prisoner who frequently contrived to get loose from his chains was accused of having charms, *literæ solutoriæ*, which Ælfric translates "rune-staves." We have therefore but a limited number of runic inscriptions of the Anglian character remaining in our island. I say "Anglian" rather than "Saxon," because the inscriptions are almost entirely confined to the parts of the island occupied by the Northumbrian Angles, in whom I am accustomed to find the ancestors of our art and literature. The earliest piece of English literature in existence is found cut in deep large runic characters on the shaft of a cross in old Northumbria; and the earliest and most beautiful specimens of English art in sculpture and in draftsmanship are found on the remains of Northumbrian cross-shafts and in the gospel-book of the Northumbrians.

While the Ogam symbols are found only in these islands, Runes are found in connection with Gothic remains in Europe of an early date, and immense quantities of Runic inscriptions are found in Scandinavia. The Gothic runes are practically the same as the Anglian; the Scandinavian runes show considerable disintegration from the earlier type.

Several of the characters of the Runic alphabet are at once distinguishable, from their likeness to our ordinary capital letters. Such are T, I, B, R. Inasmuch as our ordinary capital letters are Latin, this means that some of the Runic characters have much resemblance to the Latin. But the Latin alphabet is only one stage younger than the Greek alphabet, with which in several letters its capitals are

identical. On which of these two alphabets was the Runic alphabet based? I fail to see sufficient length of time, or any geographical connection, to account for their being based on the Latin. On the other hand, there are a sufficient number of centuries and sufficient geographical links to account for the runes being based on a Greek alphabet; not the ordinary Attic, but one more archaic in some leading characters. It is technically an error to speak of a "Latin alphabet" or a "Runic alphabet." It is only the Greek that is properly called an alphabet, from its first two letters. The Latin is properly called the abecedarium, from its *a b c, d*, and the Runic the futhork, *f u th o r k*. The Ogam alphabet is called in a similar way the bethluisnion, from the first letters *b l* (*beth* and *luis*), pronounced baylushneen. The English "alphabet" is wrongly so-called; it is properly "the abecee."

I accept as conclusive Canon Isaac Taylor's theory as set forth in his *Greeks and Goths*, a preliminary portion of his great book on *The Alphabet*. He goes boldly to the time when the Ionian colonies in Thrace and about the Black Sea were cut off for ever from their mother country by the Persian invasions of the sixth century before Christ. They were shut off in that distant and dark land for centuries, with the alphabet of the mother country as it was at the time of their separation. What that alphabet was we know. In the course of centuries a junction was effected between the barbarians our ancestors, living about the Baltic, and the Greek traders from the Black Sea with their remains of an ancient civilisation. Our Gothic ancestors learned from the traders from the south to use their characters as a means of recording transactions. That is the simple theory. When you examine the table which I have prepared (Fig. 1), of the Ionian alphabet and the runes, you will need only two other hints to see the connection. Our Baltic ancestors kept their tallies by incisions on wood. Anyone who has cut his initials on a school bench or desk knows that boys with initials formed of straight strokes are lucky as compared with boys whose initials are rounded. He knows also that a straight stroke itself can be a nuisance if it goes along the grain; care is needed to prevent its splintering at the ends when he endeavours to extract the piece he is cutting out. The difference between the Ionian alphabet and the runes consists, roughly speaking, in all rounded curves being made into straight lines, forming angles instead of curves, and in the removal by one device or another of every horizontal line; there is not one horizontal line in the whole futhork.

The Anglian runes identical with the Ionian letters are B, I, L, R, S. The runes for W and long O are Ionian letters cut in straight lines instead of curves; E and T are only altered by horizontal lines being replaced by two lines at an angle, cut against the grain; G and U are the Ionian gutturals Ch and G; Ng is a double Ionian G; Th is the Ionian D; H has its horizontal line made slanting, as also F; M has its two middle lines cut through to the side lines, to

distinguish it from the E rune. D is the Ionian Th cut square, with the horizontal lines removed as unnecessary and troublesome. The vowel sounds, *A*, *Ae*, *O*, appear to be simple modifications of the Ionian short E, a very useful letter for cutting on wood. K and N have one of their three strokes knocked off. I have now mentioned 22 of the 26 Anglian runes; and of the remaining four, one is compounded of E and A, and the others are so exceedingly rare as scarcely ever to meet the eye of the investigator of Runic inscriptions.

Fig. 1.

Many questions of great interest are raised by some of these changes and appropriations. I will only mention two. One is the use of the Greek koppa, corresponding to the Hebrew koof, for the rune W. We are familiar with the mediæval use of *qu* for *w* in such words as *quhat* for *what*. The other is the use of the Greek G for the rune U. Here again we are familiar with the forms *guard* and *ward*, *guerre* and *war*.

The earliest pieces of English literature in existence are the inscriptions on the shaft of a cross in the churchyard of Bewcastle, nine miles from Gilsland. It is 14½ feet high, and when the head was on it stood 17 feet high. It was erected in the year 670, as the inscriptions show, and besides the inscriptions now shown it bears the names of Wulfhere, king of the Mercians, his wife Kynesuitha, and her sister Kynburug.

The sculptures on this cross in clear relief are so striking as works of art that I have had a slide specially prepared to show the figure of our Lord in the attitude of benediction; a figure about 4 feet high, of marvellous dignity in its simplicity. Very difficult questions are raised on the art side of this striking panel. For our runic purposes we note the inscription at the head, *Kristtus Gessus*, which shows that our Anglian ancestors pronounced their consonants with special emphasis; whereas

the Britons probably followed the modern Welsh rule, "pronounce the vowels."

I show on slides and in full-size facsimile diagrams portions of the following Bewcastle inscriptions:—

(1) "Fruman gear küninges ricæs thæes Ecgfrithu."

(2) "This sigbeku thun setton Hwætred Wothgar Olwfwolthu aft Alcfrithu ean küning eac Oswiung. Gebid heo sinna sowhula."

(1) "The first year of Ecgfrith king of this realm."

(2) "This slender token of victory Hwætred Wothgar and Olwlfwolthu set up in memory of Alchfrith formerly king and son of Oswy. Pray for the high sin of his soul."

There is a similar shaft, rather longer, at Ruthwell, in Dumfriesshire, which was under Anglian domination during Ecgfrith's reign down to his death, in 685, when it passed away. No later period can be mentioned at which a great religious poem in early Anglo-Saxon could have been incised in early Anglian runes on a cross in that district. Parts of the shaft, especially the upper part which is socketed into the lower and may be a little earlier in character, are defaced, but most part of the lengthy inscription can be read with ease. It is the original poem which was afterwards developed into the Dream of the Holy Rood, a poem of more than 300 lines found in a manuscript of the ninth century at Vercelli. The Cross of Christ itself is made to speak and describe its agony in the part it had to play.

The portions of this great shaft which I show on slides and in facsimile diagrams contain the runes of the four following passages of the Dream of the Rood:—

(1) "(On)geredæ hinæ God almeottig tha he walde on galgu gestige . . . ."

(2) "Krist was on rodi hwethræ ther fusæ fearran kwomu æththilæ ti lanum ic thæt al bi(hea)ld s(are) ic wæs mith sorgum gidræ(fe)d . . . . . ."

(3) "(Ahof) ic riicnæ cüninge heafunes hlafard hælda ic (n)i darstæ bismærædu ungcet men ba ætgadre . . . . ."

(4) "Mith strelum giwundad alegdun hiæ hinæ limwœrigne gistoddun him æt h(is) (l) icæs heaf(du)m bihealdun hiæ ther. . . ."

(1) "Girded him God Almighty then he would step on the gallows . . ."

(2) "Christ was on the Cross, but there in haste from far came they to their noble prince. All this I saw, sorely was I with sorrows harrowed . . . ."

(3) "I upraised the rich king the Lord of heaven. I dared not stoop, they scorned us both together . . . ."

(4) "With missiles wounded they laid him down limb-weary, stood by his head, there they looked upon . . . ."

Fig. 2 shows the inscriptions (2) and (4); (2) begins at the top and reads across the top and down the right side; (4) begins at the top of the left side and reads down that side. The figure is taken from my book on 'Theodore and Wilfrith' (S.P.C.K.), p. 247.

The runes are on two sides of the shaft. On the other sides are panels representing scenes from the New Testament and one from early Church History. They are surrounded by inscriptions chiefly

Fig. 2.

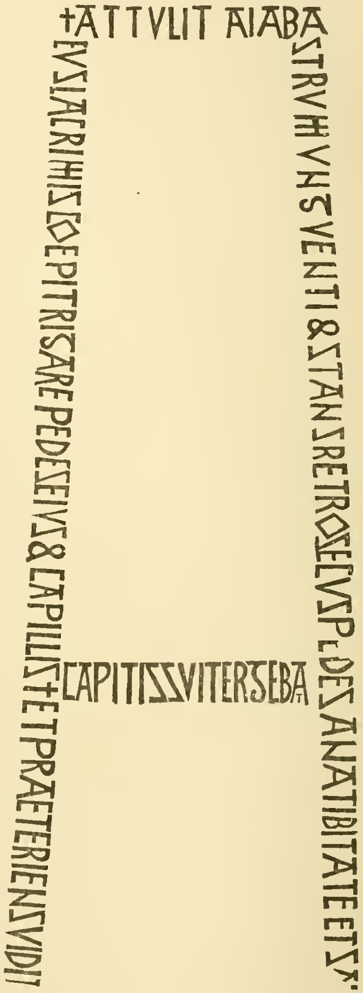


Fig. 3.

from the Vulgate, in beautifully cut Latin capitals of great palæographical value. As an illustration, I show the complete inscription which surrounds the scene of the woman wiping the Lord's feet with

the hair of her head. *Attulit alabastrum unguenti . . . . . capillis capitis sui tergebat.* Fig. 3 shows this inscription and parts of the one below; the L in *alabastrum* has lost or never had the horizontal stroke. The figure is taken from 'Theodore and Wilfrith,' p. 240.

The runic stone shown next was found a few years ago in the Wirral. It must be regarded as having an error in the inscription. Errors did occur, even in those careful times. Even on the Ruthwell cross a letter was cut as E instead of U, and a bold stroke of the chisel corrected it, but left the correction evident. On one of the pillow-stones found under the heads of the early Anglo-Saxon nuns in the cemetery at Hartlepool, the rune-cutter omitted a letter, and cut Hilddiyth instead of Hilddigyth; he remedied this by drilling a hole between the *i* and the *y*, and above the line he cut a *g*. This is shown on a slide. The error in the Wirral stone seems to be due to a confusion of the two words for "in memory of," *fore* and *æfter*. It is said, also, that there is a very irregular plural for *folk*. The inscription was in two lines, thus:

> "Folkæ arærdon bek [un . . .
> [geb]iddath fote Æthelmun[d . . "
>
> "The people erected a memorial . . .
> Pray for Æthelmund."

There are at Thornhill, near Dewsbury, three pretty little runic tombstones, with interlacing patterns in the upper part and the runes below. The longest of the three inscriptions is thus:—"Gilsuith arærde aft Berhtsuithe bekun at bergi gebiddath thær saule," "Gilsuith erected in memory of Berhtsuith a memorial at the grave-mound. Pray for the soul." Thus in three cases we have the word *bekn* or *bekun* for a memorial: it means "that which beckons to us, gives us a sign, reminds us." Another slide shows the whole of the Runic inscriptions on the Bewcastle and Ruthwell shafts, about 530 runes in all, 180 at Bewcastle and 350 at Ruthwell: but there were many more on the Ruthwell cross, now no longer legible. The remaining inscriptions in Anglian runes now known in England give about 400 runes. The only runes in the southern part of England are the 8 letters of a name at Dover, and 8 at Sandwich. There are about a dozen letters of Anglian runes in Wigtonshire, at Whithorn and in St. Ninian's cave.

Of runes Scandinavian in character, we have in England two examples in Cumberland (now practically ascertained to be forgeries of about the middle of the present century), and one in London. The runic head-stone now in the Guildhall Museum in London, shown on a slide, was found some twenty feet below the surface in St. Paul's Churchyard during the excavations for Messrs. Cook's warehouses. There is an excellent cast of it in Messrs. Cook's counting-house, and another in the library at St. Paul's. The front of the stone has a bold and intensely Scandinavian representation of the ancient Persian ornament, the antelope looking over his shoulder at the sun rising

among the trees. On the edge the inscription is found, in Scandinavian runes beautifully cut, *Kona let lekia stin thensi auk Tuki*, "Kona and Tuki caused lay this stone." Tuki was the minister of King Canute. The body stone bore on its edges the name of the deceased; portions of this stone are in the British Museum.

These runes have not digressed so widely from the Anglian type as is the case with the runes in the Isle of Mann (Fig. 4), where are about as many runic inscriptions as in England. At Kirk Braddan there are no less than five in the churchyard. I select (Fig. 5) the inscription which occupies one edge of the shaft whose other sides are covered with skilfully designed dragons. The runes run thus: *Thurlabr Neaki risti krus thono aft Fiak sun sin bruthur sun Eabrs*, "Thurlabr Neaki erected this cross in memory of Fiak his son, Eabr's nephew," probably but not certainly meaning that Thurlabr was nephew to Eabr or Eab.

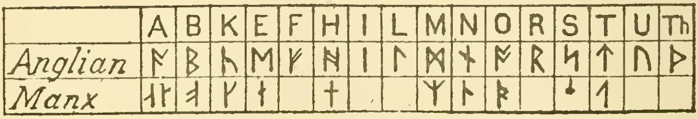


Fig. 4.

Kirkmichael is still more rich in runes; there are six in and near the churchyard. I select (Fig. 6) the great cross which stands on a pedestal of several courses outside the gate. The ornamentation is curious, having something in common with the Pictish stones of the east of Scotland. The inscription is *Iualfir sunr Thurulfs hins rautha risti krus thono aft Frithu muthur sino*, "Iualfir son of Thorolf the Red erected this cross in memory of Fritha his mother." Another of the Kirkmichael Runic inscriptions is the most interesting on the island (Fig. 7). The cross is cut in relief on an erect flat tombstone, as is usual in Mann and in Scotland, and has the specially Manx pattern, which is so near akin to patterns on tesselated pavements, and is so very seldom seen on sculptured stones out of Mann. The inscription runs up the front of the slab, on one side of the cross:— *Mail Brikti sunr Athakans Smith raisti krus thano fur salu sina sin brukuin kaut kirthi thano auk ala i Maun*, "Mael Brikti, son of Athakan, Smith, erected this cross for his soul. His — Gaut carved this and all in Mann." The meaning of *brukuin* is uncertain; surety, tenant, friend, kinsman, and other interpretations, have been assigned to the word. Gaut was clearly a great sculptor of crosses or incisor of runes. He had wrought all that up to that time had been wrought in Mann. Where he learned his art is a difficult question, if the sculpture of the whole cross is meant, less difficult if the runes only are meant. One ingenious writer remarks acutely that the statement cannot be true that Gaut carved all the crosses in Mann, for some of them are much later than his time.

The origin of the Ogam symbols (Fig. 8)—for letters they cannot be called—is lost in an obscure past. In this respect they are in a position very different from that of Runes, where the only question is from which of two closely related classes of alphabet the actual Runic letters are derived; that is, the early Italian form or the early

Fig. 5.

Greek form of the Phœnician alphabet. It is usual to say of the Ogams that they have evidently been invented for the sake of ease in cutting upon wood or stone. That view can scarcely be maintained in face of the facts that the letter *i*, which in the alphabets connected with the Phœnician is as simple as a letter can be, is in the Ogam script one of the four most laborious symbols, the three which share

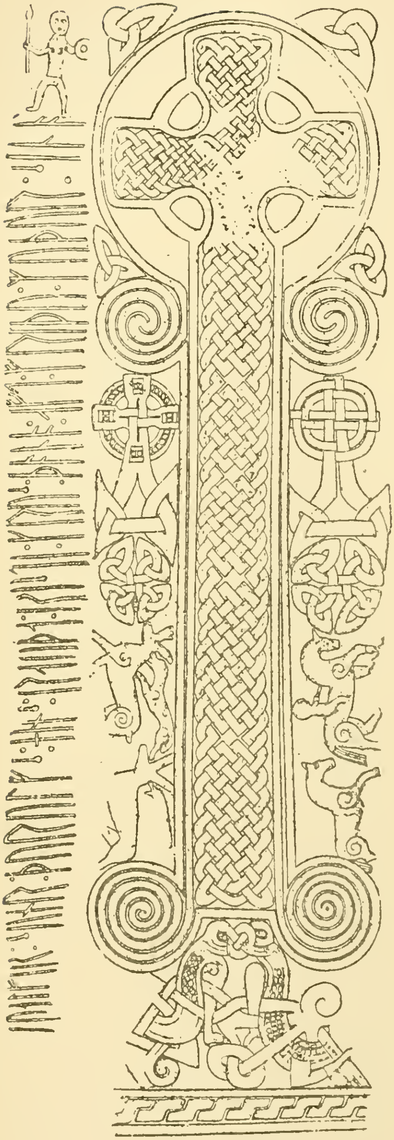


Fig. 6.

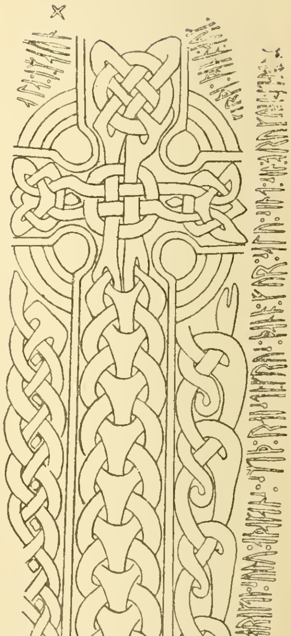
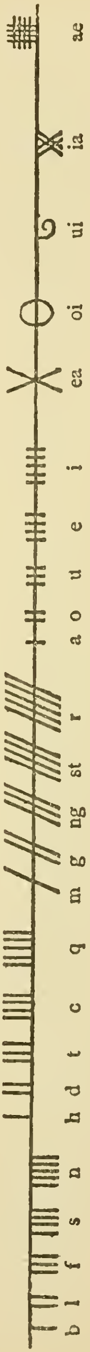


Fig. 7.

with it this distinction being *n*, *q*, and *r*, these four letters being as often used in Ogam inscriptions as any other four letters which can be named; and that the letter *h*, which is not incontestably present in any one of the large number of Ogam inscriptions known to the public as in existence at the present time, and is at least excessively rare, is one of the four least laborious of the Ogam symbols, the three which share with it this distinction being *a*, *b*, and *m*, *b* being of rare occurrence as compared with any one of the four heaviest symbols. Without making any assumption as to the language for which the Ogam symbols were originally used, it is fairly safe to say that in no known language is the relative frequency of occurrence of the several letters such that it should be made five times as easy to cut the four letters *a*, *b*, *h*, *m*, as to cut the four *i*, *n*, *q*, *r*. In the Gaelic languages, while still in an inflectional stage, for which we find the ogams actually used, the relative frequency points rather the other way, if anything.

That the Runes reached the state of development in which we find them in the earlier periods, by means of alterations in the rounded and curved and horizontal lines of letters, with a view to making them easy to cut on wood with a marked grain, may be taken as certain. The result accounts for and justifies the change. But convenience of cutting has not been the original cause of the assignment of Ogam symbols.

I shall not enter upon the question, what is the reason for the order of the Ogams. It is in fact, so I am assured, the actual order of the Irish alphabet. If this be so, the connection is of course certain; but which gave to the other this order, and where the one which gave it to the other got it from, are pertinent questions. On the latter question, where the order originally came from, I have—as I have said—no intention of entering. The performances of the champions in that field are not an encouragement to others. This much is certain; it is not an order which grew up unawares, nor is it an order which came from the Semitic alphabet, or from any other known primitive alphabet in any part of the world. No early alphabet



Ogam bethluisnion

Fig. 8.

would put all the vowels together. That has certainly been the work of men who had studied language and the means of expressing articulate sounds. Why, we may ask, should men who certainly must have been—I mean no play on the words—men of letters, so far as in those early times any man not of the two great nations of civilisation were, have devised an exceedingly cumbrous manner of writing the language and representing the letters of which they had at least some scientific knowledge? To say that it was for convenience of cutting on stone or wood is—as I have pointed out—to disregard the facts; that is to say, a very much more convenient arrangement of the system of notches could have been made. I do not at all mean to imply that those who speak of convenience of cutting suggest that the idea of rapid work was present to the minds of those who devised the Ogam. The world was young then, and people were not striking for so much an hour. But I think the principle of least effort may be taken as having guided, on the whole, the general conduct of men at all times, to their knowledge or not to their knowledge, and the principle of least effort was not present as the fairy godmother at the birth of the Ogam script.

In connection with the runes and the relative labour of cutting runes and ogams, I once took the trouble to count how many scores you must cut to make the Anglian runes which correspond to the 20 ogam symbols. The result is curious. In each set of 5 letters you must cut in ogam the sum of 1, 2, 3, 4, 5 scores or notches. In the first of these groups, 15 notches in ogam, *b, l, f, s, n*, in name, you must cut 15 scores in runes, exactly the same number. For the second group of 5 ogams, that is of 15 notches, you must cut 13 scores in runes. For the third 15, you have no *st* in runes; without it you cut 14 notches in runes, and with it you cut 15 in ogam. For the last group, 16 in rune and 15 in ogam. That is, it costs you 60 notches to cut the ogam bethluisnion straight through, and 58 to cut the corresponding runes less *st*, or 56 in ogam and 58 in rune omitting *st* in ogam. Of course this is the merest coincidence, but it has its bearing on the question of least effort expended on the whole alphabet, as contrasted with the question of least effort in individual letters. If we make a distinction between long notches and short ones, the runes take much less effort to cut, for 31 of the 58 notches are short. In the ogam only the vowels are short; and as the *m* group are all of them more than twice the ordinary length, the shortness of the vowels is more than compensated for. Indeed, if you take an ogam score for *b* of 3 inches in length as your normal length for ogams and runes alike, you will have to cut 216 inches of notch and 15 dots to make your ogams, and about 97 inches of notch to make the corresponding runes. The fact that he had carefully to fit together the various notches which form a rune would probably be more trying to an early stone-cutter than a much greater length of straight cutting in ogam would have been.

I am driven to believe, either that the ogam was invented of set

purpose as a cryptic alphabet, a set of symbols to be used on wood or on stone instead of letters, on a system known only to a few, or that the ogam was copied directly from some method of notation in which it was just as easy to mark five as to mark one. The two, as you will see, are not inconsistent, and the ogam may have come from some cryptic system of notation in which it was about equally easy to mark one, two, and up to five.

Now the tradition is—though no ogams have been found which belong to the earlier stages of which the tradition tells—that there were originally only 10 ogams; that they were then increased to 12; then to 16; and finally to 20. That is to say, beginning with 2 sets of 5 ogams, people went on to 3 sets of 4, then to 4 sets of 4, and at last to 4 sets of 5. And it is said that at one change of this kind the man who guided the change ordered that the ogam should no longer be a secret. What can be the explanation of the ringing of the changes on 4 and 5, with apparently no extra difficulty of treatment? And what hint can the story of the ogam ceasing to be secret have for us?

My theory is this, that the ogams are mere copies of signs made with the fingers of one hand or the other, and that when the ogams were in groups of 4, with 1, 2, 3, 4 notches for signs, the fingers only of each hand were used; when they were in groups of 5, with 1, 2, 3, 4, 5 notches for signs, the thumb of each hand was used as well as the fingers.

The ogams which we are accustomed to see, or which I hope my sheets of illustrations are accustoming you to see, run along on a long line without discontinuity. But of course each could be made separately for practical purposes. We may suppose that the original operator held up his left hand and applied the point of one finger at right angles for one letter, two fingers for two letters, and so on up to five. Now it really makes no difference, so far as trouble goes, whether you hold out five fingers or one. Then the operator held up his right hand, and applied one, two, . . fingers of his left hand. This accounts for ten letters. Then one finger, two, and so on, laid diagonally across the palm of the other hand, will give you five more. Finally, to apply the point of one finger, two, and so on, to the palm of the left hand, will give you the dots for the vowels; or laid from the middle to the side of the palm, that is, short notches. Conceivably, the knuckles of the clenched fist were touched for vowels. The diphthongs are easy to make with the fingers. There is a curious hint of fingers in the cross-line diphthongs, especially in the fact that there are crosses of one finger each, two fingers each, and four fingers each, none of three. The well-known difficulty of bringing up the third finger without the help of the second or fourth, seems an almost conclusive explanation of this phenomenon.

My guess is that these finger-signs were used for incantation, or for cryptic purposes, and that they were for long unknown except to a few of the initiated. They may well have come down from ex-

ceedingly early times, the times of Cæsar's Druids, for instance, and earlier. I cannot at all think that they are a mere literary invention of Christian times. Passing through many stages they arrived at length at the development in which we know them. Christianity rendered their use for cryptic purposes no longer applicable. The time for medicine-men had gone by. The abolition of the Druids abolished the impiousness of writing down any Druid secret. The ogams were then, for the first time, used for sepulchral purposes, just at their fullest development, and just at the time of transition in religious beliefs, among the people who occupied the limited districts where the survivors of those who had cryptically used them dwelt; and in a very short time their use passed away for ever. The knowledge of the key did not die out, and we have a few examples in Scotland probably quite as late as some of even the later runes.

You will of course have noticed that while our present finger alphabet for the deaf and dumb, which was only invented about 150 years ago, reproduces as far as fingers can the shapes of the letters, so that anyone looking on can see what several of the letters are, the ogam entirely avoids that, and is quite inscrutable if you do not know the key.

In cutting the ogams on stone, one edge of the stone, or a prominent ridge on the stone, was taken as the dividing line. In the following illustrations, which are taken by photography from my facsimile rubbings of the stones, the edge is not shown; it is usually irregular, the inscriptions being cut on a rude pillar-stone.

Fig. 9 shows the inscription on a stone now in the Queen's College at Cork. The ogams are read from the bottom upwards, and they pass round the top and down the other side. The inscription seems to be of comparatively late date, judging by its grammatical form. It has after the first four letters a symbol in form of X, and to this Mr. Brash assigns the function of dividing two parts of the inscription. But such division is unknown elsewhere, and has in this case no meaning, indeed it destroys meaning. The symbol X is given in the Book of Ballymote as representing the consonant *ea*, and there seems no doubt that on this stone it stands for that or some other letter or combination of letters.

The stone has been damaged at one end of the inscription since it was first found at Tinnahally farm, in the parish of Kilorglin. The journey of 70 miles on rough roads from Kilorglin to Cork may well have obscured the earlier letters, which were quite clear when it was found. They are fairly clear still. I read it *anm teagann mac deglenn.*

The usual form of the inscriptions in ogam characters is "A son of B." The words are all in the genitive, the word "the stone," or "the memorial," or "the grave," or possibly "the body," being understood :—"the monument of A son of B."

This inscription has Mac, instead of the old genitive Maqi. This looks as if *anm* were a verb, with some such sense as *requiescat*,

or *hic jacet.* The *anm* is at present a puzzle, but a puzzle that no doubt will soon be conclusively solved. On the companion stone to this, which I describe next, it is a worse puzzle, appearing in the form *ancm.* I show in illustration of this word, slides from my rubbings of two stones at Lismore, with minuscule inscriptions (Fig. 10), *bendacht for añm Martan,* "a blessing for the repose (or for the soul) of Martin," and (Fig. 11) *bendacht for anmain Colgen,* "a prayer for the repose (or soul) of Colgan." In both of these cases the word might well be understood as an adaptation of the Latin *anima,* the soul, as *bendacht* for benedictio; but that use would give us no help

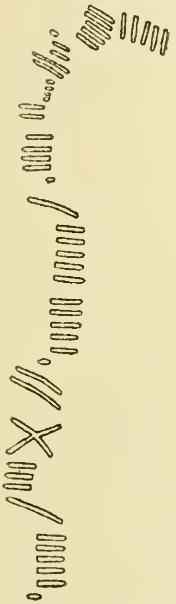


FIG. 9.

FIG. 10.

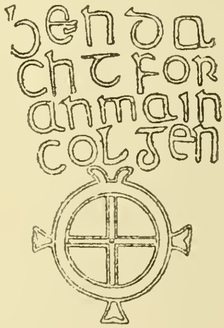


FIG. 11.

in the *mac deglenn* case, where "the soul" must have been followed by the genitive maqi. It has been suggested that *anm* may be taken either as a verb or as a substantive, so that we might read "may Teagann mac Deglann rest," and in the next case ["a prayer for] the repose of Farran."

But it seems to me that these are relatively late ideas. The Lismore inscriptions which I have shown in illustration of the word *anm* or *anmain* are later than the dates assignable to Ogam inscriptions. The Colgan (irreg. genitive Colgen) named in the earlier of the two was an eminent ecclesiastic who died at Lismore in the

year 850. Martin (genitive Martan) is probably Martin ua Roichlich, Abbat of Lismore, who according to the Four Masters died in 878.

The other Tinnahally stone (Fig. 12) is also in the cloister at the Queen's College, Cork. It, like its companion, is 7 feet 6 inches high, but it is less bulky, and the inscription is less clear. It has on it, in my judgment, traces of another ogam inscription, which tend to confuse the inscription we are now considering. I am not quite sure that the puzzling and feeble *c* which thrusts itself in among the digits forming the *anm* may not be part of such other inscription, just

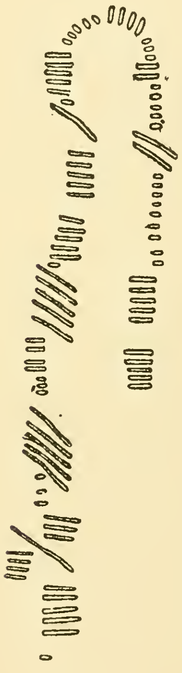


Fig. 12.

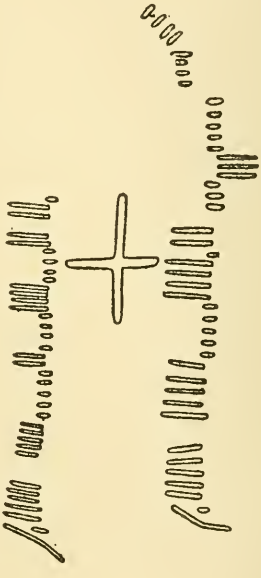


Fig. 13.

so far showing itself that the author of the inscription was obliged to leave it as a gap between his *n* and *m*.

I read the scores thus:—*an*(*c*)*m furuddran maqi culigeinn* "[a prayer for the] repose (or the soul) of Farran, son of Colgen." The name Furuddran is found in an Ogam inscription at Gortamaccaree in the same county of Kerry, in the form Furadran and Furadhran. The destructive effect of the letter *h* upon consonants which precede it in Irish words reduces this to Furaran and Farran. There are plenty of names similar to Culigenn; for instance, Cooligan.

Sir S. Ferguson reads *anmc* instead of *ancm*, but his account makes me think that he had not really examined the stone and that he accidentally transposed the *c* and the *m*; certainly my own personal inspection puts the *c* before the *m*. Further, he reads *doligeinn* where I read *culigeinn*, and he translates *maqi do ligeinn* as 'son of reading,' i.e. scholar, or learned man. This would be a stretch of imagination far beyond the reach of the ogam cutters. Mr. Brash reads culig....enn; I, like Sir S. Ferguson, felt fairly clear that there are nine vowel dots, divided into four and five, that is, *e i*.

In the Museum of the Science and Art Department in Dublin, among the collections of the Royal Irish Academy, is a stone (Fig. 13) from a rude ancient clochan at Gortnagullanagh, co. Kerry. It has two inscriptions on the two edges of the same face, both to be read the same way; probably both visible to persons entering the clochan. The stone is about 4 feet 6 inches long, and 11 inches broad. It has a Latin cross 8 inches by 6 inches, inscribed on the face.

One of the inscriptions is very clear. It reads, as all agree, *maqqi decedda*. The other is in most of its letters clear; all agree that it was *maqqi catuf*, except that Sir S. Ferguson prints *cattuf*, no doubt by accidental confusion with a stone at Corkaboy. The remaining scores I think read *ici* or perhaps *icc*; Mr. Brash reads them *ucuc*; Sir S. Ferguson, *ic*. My reading gives either 14 or 13 scores; Mr. Brash's gives 14; Sir S. Ferguson's 9. I incline to *Catuficc*, for at Corkaboy in this same county there is an Ogam stone which I have not seen, where Mr. Brash and Sir S. Ferguson agree in reading *Cattuffiq maqi Ritte*. Mr. Brash, reading *maqqi Catuf uc uc*, translates 'son of Catuf, alas, alas.' But that translation is out of the question.

In each of these cases the inscription would seem to be incomplete, the name of the person not being given, only the name of his father or some descriptive word standing in the place of a father's name or the name of a race. But there are curious evidences which go to show that the Ogam inscriptions are in Ireland usually—it is said almost or quite always—found in proximity to cilleens, said to be the remains of pagan cemeteries, used afterwards for the burial of unbaptised children. It is quite conceivable that an Ogam inscription should name the race or family whose general burial place it was, and not name any particular member of the race. We speak of "the Percy vault" in Westminster Abbey, and we are familiar with the appearance of a family name on a stone in the floor of a church indicating the entrance to a burial place. There is nothing distinctively Christian in this practice.

The inscription Maqqi Decedda has very wide relations. The Clan Degadi or Degaid was a famous clan in very early times; and though other explanations are given, it may probably be taken that the numerous inscriptions with this name in one form or another have to do with members of this clan. In that case the maqqi is

N 2

used in its general sense for "of the race of," not in its limited sense for "son of." I do not remember any Ogam inscription which uses the better known formula for "of the race of," Ua or O', i.e. "grandson of." As far as I have been able to see, the difference in principle between the formation of a family name from *Mac* and from the more distant *O'* would provide a very interesting subject of investigation. It is probably not known to all English readers that O'Neill, for instance, is properly only used as the name of a man; an Irish woman in an English hospital is addressed by an Irish physician as Mary Nin Neill, in the feminine, not Mary O'Neill in the masculine. Other examples of the patronymic Decedda are as follows: In another Kerry inscription, maqi deccedda; at Dunbel, Kilkenny, maqi decquedda; at Ballycrovane, co. Cork, maqi deccedda; at Ballintaggart, co. Kerry, maqi deccedda; at Killeen Cormac, maqi ddecceda; these are all in Ogam script: at Penrhos Lygwy, in Latin characters, hic iacit maccu deceti; and at Buckland Monachorum, near Tavistock, Sarini fili macco decheti.

The cross on this stone is, no doubt, a great deal later than the ogams. The ogams on the two arrises are both read the same way, instead of up one side and down the other. This is evidence that they are two separate inscriptions, not one continuous phrase; and it suggests that the stone was originally in a horizontal position, perhaps as the lintel of a rude entrance to the pagan cilleen.

Fortwilliam, co. Kerry. Fig. 14 is a stone in the vestibule of the Library of Trinity College, Dublin. It is in excellent preservation, and the ogams are beautifully bold and regular and clear; and yet it presents considerable difficulties. The Irish antiquarians appear to be agreed about the readings, and my rubbing and examination falls in with their view, though I have felt it necessary to mark two of the scores with a query. The chief difficulties are (1) that it is not easy to distinguish vowels from consonants, whether by size or by relative position; and (2) that there are rows of vowel scores without spaces to separate them into their proper numbers. My first score is quite as large as a consonant, and yet, with the Tinnahally examples before us, we seem compelled to take it as the vowel *a*. This gives *anm fedlliostoi macui eddoini*. The patronymic *eddoini* need not give us any trouble, it is probably the same as Aedan. The *fedll*, again, need not trouble us; there are other names of this character, as Feidhlimidh, or Fedhlimidh, our Phelim. The *oistoi*, or *iostoi*, appears to be too much for the science of interpretation. The only point on which I feel it possible in my ignorance to fasten is the group of four scores which is rendered *st*. There is no doubt that the Book of Ballymote assigns to it this value of *st*, though, so far as I know, it has not been found on any ogam stone. Considering the recognised fact that care was not taken by the author of this inscription to separate his lines of dots into their proper groups, I venture to suggest that we should read these four scores as two groups of two scores; that is, as *gg*, not with the usual pronun-

ciation *ng*, which in the Book of Ballymote is represented by three scores, but as a hard double *g*. We shall then have, as the name of the person buried, Fedlliogg, a name akin to Velioc, which in one form and another we find in very early times.

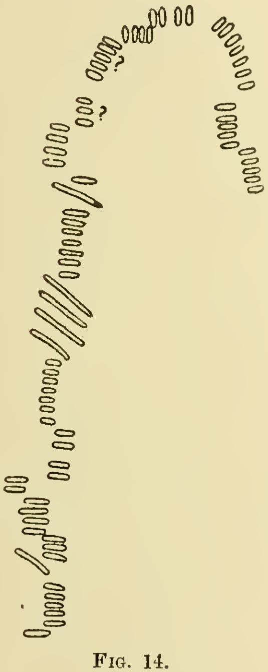


Fig. 14.

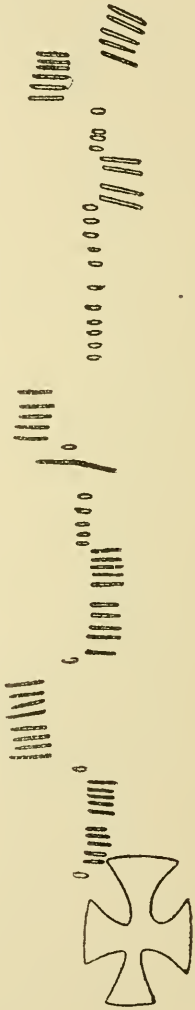


Fig. 15.

Knockourane, co. Cork (Cnoc-oran, the Hill of Song). This stone (Fig. 15) was bought by the late Mr. Windele, who intended to have

it used as his own grave-stone. It was not used for that purpose, and it is now in the cloister at Queen's College, Cork. It is remarkable for a large Maltese cross cut on its face, all but interfering with the ogam scores. Either the cross was cut first, and the ogam-cutter turned the stone upside down and cut his ogam upwards; or the cross-cutter found an ogam stone and turned it upside down to cut his cross at the top. In the case of the Llywell stone, as we shall see, it is certain that either the cutter of the ogams or the cutter of the human figures, whichever came last, turned his predecessor's work upside down. I do not hesitate to make the ogam-cutter the predecessor of the other. The cutters of the Knockourane cross and the Llywell human figures probably had not the faintest idea of the meaning of the ogams or of the direction in which they must be read.

The reading as given by my rubbing is *annaccanni maqi aill-uatt(a)n*. I am inclined to think that there are signs of an Ogam score below the first *a*. The second *nn* is rather doubtful, and may be *ss*. The *q* is unusual, running past the arris and appearing on the face of the stone; but the scores do not slant, and I do not see how we can read it as *r*, still less how Sir S. Ferguson can make it stand for both *q* and *r*. I have omitted the *a* in the last syllable of the third word, and Sir S. Ferguson omitted it; there is, however, certainly a notch in the stone where the *a* ought to be if it ever was there. Finally, the concluding scores slant decidedly, and the arris is not well-defined. I should not resist the reading *r*, though linguistic probabilities point to *n*. Sir S. Ferguson would seem not to have seen the stone itself; but the rubbing supplied to him was clear enough to warn him of difficulties not present to the eye of Mr. Brash. Both names are well known. Hannagan is an Irish name still, and the form Aenagan appears in the Four Masters as late as the years 878, 893, 898. The other goes very far back; the name of no less a person than the father of Ogma the sun-worshipper was Ealladan. Sir S. Ferguson read *Aillittr*, by adding a notch between the *u* and *a*; but there is certainly no notch there now. He made *Aillittr* mean "the pilgrim." It should be noted that a rubbing of this stone seems to show ogam scores where in fact no scores exist.

The stone at St. Dogmael's Abbey (Fig. 16) near Cardigan, is of great interest. It was the first ogam stone found with an inscription in Latin letters: it is also bilingual. Before the discovery of this stone, it had been a matter of dispute whether the Book of Ballymote gave the correct key to the Ogam script, the letters of the Irish inscriptions giving such curious words according to that key. It had, however, been acutely argued, that inasmuch as a certain group of four ogam scores occurred in so large a number of the inscriptions as to be almost universal, this group of four probably represented the word "son," coming between the name of the dead man and the name of his father; further, that as the name was probably in the genitive, the words "the monument," or "the memorial stone," or "the body," or "grave," being understood, the word "son" would also be in the genitive, and in the early times the genitive would be inflexioned

and *mac* would become *maqi* or *maqui*, the *qu* being regarded as one letter. This reasoning made the inscriptions fall in so far with the Ballymote key. The St. Dogmael's discovery clenched the matter. Taking the four letters *m*, *a*, *q*, *i*, as ascertained before the discovery, no less than eleven out of the twenty ogam scores found at St. Dogmael's were already known. The test was at once applied; the

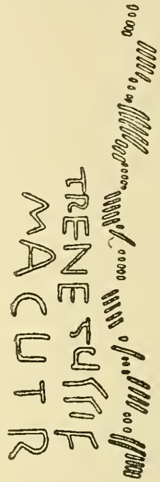


Fig. 17.

Fig. 16.

Fig. 18.

Latin letters agreed with the ogam scores; and the theoretical reasoning was triumphantly proved to be correct. The only differences are that Sagrani in the Latin is Sagramni in the Ogam, and Cunotami is Cunatami. The interchange of a very broad *o* and a very broad *a* is exactly what we might have expected; and inasmuch as the name Sagramn is almost of necessity pronounced Sagran, the other difference is quite natural. The Latin reads, *Sagrani fili Cunotami;* the Ogams read, beyond all question, *Sagramni Maqi Cunatami;* "the memorial of Sagramn, son of Cunatam." I think there can be at most very

little question that the date is not far off that of the departure of the Romans, and the style of the Latin letters is free from Welsh and Irish influence.

The Cilgerran stone (Fig. 17) had sunk in the soft ground of the churchyard when I visited it. Some years before it had been raised, and both of the inscriptions had been read in full. My slide shows each of the two Latin lines cut off:—

Trenegussi f
Macutr

and the Ogam appears to begin with *sgus* or *egus*. The full Latin inscription is *Trenegussi fili macutreni hic iacit.* It is usually said that this form of inscription, of which there are several in this island, is ungrammatical. I am not inclined to defend in all cases the grammar of the inhabitants of Wales, who could inscribe on a monument *Carausius hic iacit in hoc congeries lapidum*, or *Veracius hic iacit cum multitudinem fratrum*, and could very seldom indeed rise to the use of *iacet*. But my impression is that we may render the Cilgerran and other like inscriptions as "the memorial of Trenegus son of Macutren; he lies here." The full Ogam reads *Trenegusu maqi Maqitreni*, "the memorial of Trenegus son of Macitren." The Latin "son of Macitren" seems to make it clear that the Ogam Maqitren is meant to be the patronymic, and that we are not to read it as *maqi maqi Treni*, grandson of Tren. This opens up some very wide questions.

Fig. 18, from the left-hand gate post of the lane leading to the farm of Cwm Glöyn on the high road from Cardigan to Nevern, need not keep us long. The Latin reads *Vitaliani emerito*, "the memorial of Vitalianus, emeritus," that is, we are told, a Roman soldier with an honourable discharge. The Ogam is only one word, *Fitaliani*, "the memorial of Vitalianus." This seems to show that the ogam cutter knew the second Latin word to be no part of the man's name. The correspondence of the Ogam *f* with the Latin *v* may remind us that there is supposed to be no *v* in modern Welsh, *f* being properly always used where *v* is meant, and *ff* being used when the sound of our *f* is wanted.

We now come to two stones in the British Museum, from Fardell in Devonshire (Fig. 19), and Llywell in Brecon (Fig. 20). The Fardell stone and some very fine ogam stones from Ireland are in the room immediately on the left hand as you first enter the Museum; the Llywell stone is on the first floor, near the head of the stairs.

The Fardell stone has its ogams nobly cut. They are only much too clear, for they do not satisfy any ordinary experience or theory, except indeed, a theory of my own, which I will propound shortly. The Latin reads clearly enough *Fanoni Maquirini.* This ought to mean "the memorial of Fanon, son of Maquirin"; it seems impossible to import a Gaelic *maqui* into the middle of a Latin inscription, and translate "the memorial of Fanon, son of Rin." But it is not quite inconceivable that both the Latin letters and

the ogam scores represent a Gaelic inscription, and in that case the *maqui rini* is properly taken as "son of Rin," or Run, the latter a very well-known name.

The ogams read only too clearly *Sfaqquci Maqi Qici*, and "the memorial of Sfaqquc, son of Qic" does not take us into very intelligible regions.

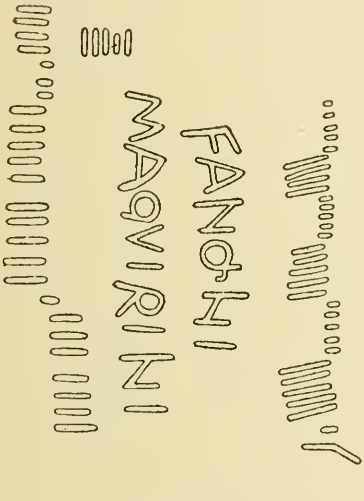


Fig. 19.

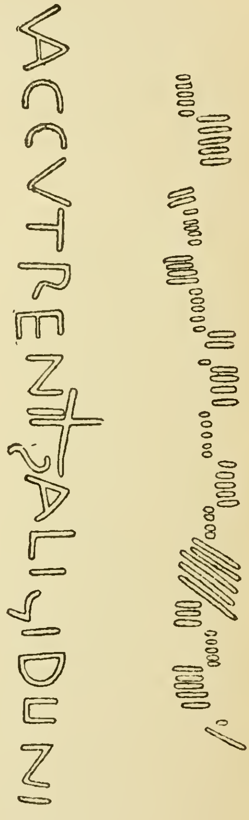


Fig. 20.

My theory is that we have here an ogam cutter who was only half familiar with the ogam scores, and if he did his best, his best was bad. The labour he expended on making the scores nobly clear had better have been spent on making them correct. The key to my suggestion is found in a zigzag scratch which comes before the F in *Fanoni*, and must represent an S. It is practically the same letter as the S of *Sagrani* which is found on the back of this same stone. This

gives a curiously close agreement between the forms of the two inscriptions, if we suppose the first *n* in Fanoni duplicated,

Sfannoni maqui Rini
Sfaqquci maqi Qici,

and when we examine the differences, we find that they are chiefly questions of one side or other of the arris. Thus if the ogam cutter had put his two groups of five scores each on the other side of the arris, we should have had *Sfann* in both scripts. In two other cases he puts four scores on one side of the arris where the Latin script requires five on the other side. If we can suppose that he made these two mistakes of transplanting from one side to the other of the dividing edge, and in one case stopped five scores at the edge instead of running them through, each repeated, we have

Latin, Sfanoni maqui Rini,
Ogam, Sfannuni maqi Rini.

It is clear that, phonetically, the difference between *o* and *u* is almost nothing; and if the *o* in the Latin script on this stone is examined, it will be seen to have a good deal more of a *u* than an *o* in it. There is, perhaps, in the abstract a greater probability that the cutter of the Latin letters mis-read some of the ogams, but the names seem to point the other way. If my suggestion has anything in it, this stone is of very great interest, as marking a parting of the ways between the two scripts.

The stone from Llywell, in Brecon, now in the British Museum, has on one side a mass of curious sculpture, about which a good deal might be said if this were the place to say it. On the back (Fig. 20) there is a Latin cross, and it is here that the two inscriptions, in Latin and Ogam script, are found. The Latin reads *Vaccutreni Saligiduni*, "the memorial of Vaccutren, son of Saligidun," or, inasmuch as the first letter is very near the end of the stone, and the A is joined on to the V, it may well have been M, and then we should read Maccutren. The Ogam settles the point in favour of M; it reads *Maqitreni Salicidni*, "the memorial of Maqitren (son of) Salicidn." It will be remembered that on the stone at Cilgerran, about 45 miles from Llywell as the crows fly, there is a memorial of "the son of Macutren."

I should give a warning to any one who attempts to follow the readings on this stone with Sir Samuel Ferguson's volume of Rhind Lectures in his hand. The late Sir A. W. Franks told me that Sir S. Ferguson, a devoted observer and recorder of ogams, spent hours upon this stone; but his notes were so far from clear that his account is most baffling. He remarks that the Latin inscription is *Vaccutrenii Maqi Saligiduni*. But the second *i* in the first word is only the limb of the incised Latin cross, and not a letter of the Maqi is there at all. Sir Samuel clenches the error by remarking that this is the only example of a Latin Maqi; and having read the limb of a cross as a second *i* at the end of Maccutreni, he reads the Ogam as Maqitrenii, thus inserting a whole row of five scores which are certainly not on the stone.

The easternmost of the English ogams, and from the place of its discovery the most puzzling of all the ogams in these islands, was found in a well at Silchester, incised on a Roman stone. It bears the word which has up to this time baffled every one, *mucoi*. The inscription is *Ebicati maqi mucoi.*

In order not to omit any of our combined islands and parts of islands from ogamic illustration, I show diagrams produced from rubbings of stones in Mann and in Scotland.

At Arbory, in Mann, we have *Cunamagli maq* . . ., "the memorial of Cunamgl, the son of . . ." The name is the same as the Irish Conmal and Conmhal.

On an irregular rounded stone at Arbory there is an Ogam inscription which is evidently honest, but otherwise might have been supposed to be a trick not very skilfully played. It reads *maqleogu*, the *u* being shown, as I believe, by the tips of the three short vowel strokes; others had not noticed this when I was there. The curious thing about it is, that Macleog, and its modern forms (after the Manx fashion of dropping all but the last letter of Mac) Cleague and Clague, are and have long been local names in Arbory parish; "Arbory" is "Kirk Cairbre." The minuscule inscription at Beckermet, in Cumberland, which has so long defied solution, has this year been read as Manx-Irish: it is the epitaph of Juan son of Cairbre.

At Ballaqueeny, also in Mann, are two Ogam inscriptions of much interest, on account of an unusual genitive found on each of them. They are *Dofaidona maqi droata* and *Bifaidonas maqi mucoi Qunafa.* The proper names are *Dofaidn*, *Bifaidn*. The former of the two is believed to mean "the memorial of Dofaidn, son of a Druid." If that is so, it is the only existing mention of the Druids in the epigraphy of these islands. It should be noted that Manx tradition and folklore attributes everything ancient to the Druids, so that Mann is the most likely of all places to have some mention of that elusive class of people.

In Scotland I show the ogams and minuscules upon the famous Newton stone (Insch, Aberdeenshire), on which volumes are written. It is rather a disgrace to us all that the minuscules are not as yet conclusively read. I do not propose to give any reading of either inscription here.

The ogams on a sculptured stone found at Scoonie, in Fife, of which I show a complete facsimile, read *Edarrnonn*, a name which would take us into very interesting questions if we were dealing with ecclesiastical history.

The sculptured stone at Aboyne, of which also I show a complete facsimile, has a longer inscription, a good deal disputed. On my outlined rubbing we may read on the left-hand line *maqqoi talluorrh.* Talorc and Taluorc, in various forms, is a well-known Pictish name in the lists of Pictish kings.

ADJOURNED GENERAL MEETING,

Monday, May 22, 1899.

His Grace The Duke of Northumberland, K.G. F.S.A., President, in the Chair.

The following persons were unanimously elected Honorary Members of the Royal Institution in Commemoration of the Centenary of the Foundation of the Royal Institution:—

Dr. Emile Ador (of Geneva).
Professor Joseph S. Ames (of Baltimore).
Professor Svante Arrhenius, F.C.S. (of Stockholm).
Professor George F. Barker (of Philadelphia).
Professor Carl Barus, Ph.D. (of Providence, U.S.A.).
Professor Henri Becquerel, Ph.D. (of Paris).
Dr. L. Bleekrode (of The Hague).
Professor Giacomo Luigi Ciamician (of Bologna).
Professor Nicolas Egorof (of St. Petersburg).
Professor Antoine Paul Nicolas Franchimont, Ph.D. F.C.S. (of Leiden).
Professor Armand Emile Gautier (of Paris).
Professor Heinrich Gustav Kayser, Ph.D. (of Bonn).
Professor Wilhelm Korner, F.C.S. (of Milan).
Mr. Samuel Pierpoint Langley, F.R.S. (of Washington).
Dr. Oscar Liebreich (of Berlin).
Professor Gustave Leonard Van der Mensbrugghe (of Ghent).
Professor Albert A. Michelson (of Chicago).
Professor Henri Moissan, F.C.S. (of Paris).
Professor Raffaelo Nasini (of Padua).
Professor Walther Nernst (of Gottingen).
Professor Wilhelm Ostwald (of Leipzig).
Mr. Ernest Solvay (of Brussels).
Professor Robert H. Thurston (of Ithaca).
Professor Emilio Villari (of Naples).
Professor Jules Louis Violle (of Paris).
Dr. William L. Wilson (of Washington).

WEEKLY EVENING MEETING,

Friday, May 26, 1899.

The Right Hon. The Earl of Halsbury, M.A. D.C.L. F.R.S., Lord Chancellor, Manager, in the Chair.

Sir William Martin Conway, M.A.

Climbs and Explorations in the Andes.

[Abstract]

The object of my journey to South America, made in the latter part of 1898, was to investigate the physical geography of the Cordillera Real in Bolivia. I was accompanied by two Alpine guides, Antoine Maquignaz and Louis Pellissier. The Cordillera Real is a snowy range eighty miles in length, culminating at its north end in Mount Sorata and at the south in Illimani. It is not a volcanic range, nor were any signs of volcanic action met with at any part of its main axis. It consists principally of a core of crystalline rock, flanked to the westward by Silurian deposits and further out by Red Sandstones and Conglomerates, rising in low hills out of the plateau which stretches all along the foot of the range at an altitude of between 12,000 and 13,000 feet above the sea. This plateau was formerly the bed of a large inland sea, of which there only now remains the relatively small portion known as Lake Titicaca.

The two great peaks, Illimani and Sorata, were ascended, Illimani to its highest point, Sorata to within a couple of hundred feet or so from the top. At both ends the range is cut through by river valleys which drain to the eastward. That to the south is the valley of the La Paz River, which, rising on the slope of Mount Cacaaca, the midmost peak of the range, flows first to the south-east along the range, and then cuts right across it and flows into the River Beni, a tributary of the Amazon. To the north the sources of the Mapiri River lie actually at the foot of Mount Sorata, and some of the snow-slopes, belonging properly to the western face of the mountain, drain into it. But the actual cutting through of the range is not here complete. There still remains a low ridge, about 2000 feet higher than Lake Titicaca, which is not entirely cut through, though the eating-back process is going forward very rapidly, and the day is not far distant from a geologist's point of view when Lake Titicaca must be drained by this river.

The eastern face of the range was not visited on this journey. On the west the line of great mountains is flanked by vast slopes of *débris*, and here the signs of former glacial extension are easily

perceived. At one time, when the waters of the inland sea washed the foot of this side of the mountains, the glaciers probably descended into it; and, even since the drying up of this ancient sea, the glaciers have descended several miles further down than they now do. The high plateau is bordered to the west by an irregular range of peaks, chiefly of volcanic origin.

During the course of the expedition the main range was triangulated, and a sketch survey was made of the western slopes and of the peaks lying between Sorata and Illimani. The northern half of the Cordillera Real—the portion, that is to say, which lies between Mounts Sorata and Cacaaca—consists of a series of lofty snow mountains, many of them of very pointed form, which stand one beside another in close proximity, so that the passes over this part of the range seldom sink below the level of perpetual snow. South of Cacaaca the character of the range is different; the mountains are less precipitous in form and there are wide openings between them, dropping to levels little above that of the plateau. The mountains themselves are uninhabited, but the plateau bears a relatively dense population of Aymara Indians, whilst in the two towns of La Paz and Sorata the white population is considerable. La Paz is the commercial capital of Bolivia; Sorata occupies an important position on the main route leading from the plateau to the rich valleys and india-rubber forests to the eastward. This route is of great antiquity, for in the days of the Incas, and possibly long before them, the gold-bearing character of the river gravels in the valleys descending to the north-east and east from Mount Sorata was known to the natives.

Besides the collection of rock specimens made throughout the range, which are being examined by Professor Bonney, small collections were likewise made of the high-level plants, insects and birds, which have been sent to Kew and the British Museum respectively. The only animals found at relatively high levels were bizcachas, mountain deer and a few vicuñas. The flora of the high levels is very sparse at the time when the mountains are accessible to a traveller, though probably after the rainy season has set in there is a greater floral wealth than was observed by our expedition.

Bolivia is known to be a country rich in mineral deposits, but few signs of mineral wealth were observed at very high altitudes. It is not in the heart but on the flanks of the range that the chief mineral deposits have been discovered. Gold is found chiefly in the valleys running eastward from Mount Sorata, and in the La Paz valley and its branches. Important deposits of tin are known near Cacaaca; cobalt and antimony are likewise found in the same neighbourhood, but I do not think that any copper has been discovered actually in the Cordillera Real. The chief copper mines are those about Corocoro, whilst along the route from La Paz to Antofagasta there is immense and largely undeveloped mineral wealth of all kinds.

On leaving Bolivia, a brief visit was paid to the great mountain, Aconcagua, near the Chilian-Argentine frontier, and one of its

culminating points was climbed. It was ascended and thoroughly explored by the FitzGerald expedition in 1897. The only observations of any original importance there made by my expedition were in respect to a peculiar formation of snow locally known as *Nieves Penitentes. Nieves Penitentes* are named from a supposed resemblance to a crowd of white-robed penitents. They are an assemblage of cones of snow, or rather they would better be described as a field of snow whose surface consists of a multitude of cones in close proximity to one another, and varying in height from a few inches to a few feet, though at any moment the cones near together will probably all be of about the same height. It has been usual to ascribe the formation of this many-coned surface to the action of violent winds, which are conceived of as causing the snow to eddy and whisk about and thus build itself up into these peculiar spires. Such, however, in my opinion, is not the true explanation. To begin with, if winds were the cause, this conical formation of snow surface should be found in windy places such as Greenland and Spitsbergen, where, on the contrary, this form of surface is unknown. It has never been noticed in the Alps or the Caucasus, nor, as far as I know, in the northern regions of the Himalayas, but it is found in Mexico and in other parts of the Andes. The cones at the beginning of the warm season are very small; as the year advances they increase in size, by the deepening of the hollows between them, till they become eight feet or more in height. They are, in fact, formed by some process of melting, not by any process of building up. They never consist of new snow. A careful examination of them at many points about Aconcagua revealed the fact that they are not of circular but elliptical section, and that they do not stand vertically, but bend over towards the north. The major axis of the ellipse was in every case more or less east and west, sometimes with one end or the other brought around somewhat to the north in cases where the site was sheltered by some neighbouring eminence either from the morning or evening sun. A field of newly fallen snow may be regarded as having a flat surface, but the accidents of its structure will in process of melting soon cause inequalities to arise. These inequalities naturally take the form of pits and lumps, and when the sun is very nearly vertical at mid-day, it naturally produces more melting in the bottoms of the hollows than it does on the slopes leading down to them. These hollows once formed tend to deepen by melting, and the deeper they become, and the steeper the sides, so much the more does the deepening tendency increase. The originally round summits are sharpened by a similar process. The sun, always remaining to the north of the zenith at mid-day, causes the hollows to have a northward tilt. When the hollows have been so deepened and widened that they ultimately run into one another, there remains standing between them a series of cones, and these are the *Nieves Penitentes* of the Andes. Sometimes, where the snow bed is only a few feet deep, the hollows will be melted down to the sur-

face of the ground beneath, and then the cones may be observed standing upon the stony bed like so many pawns on a chessboard. It is needless to observe that when these *Penitentes* are several feet high they form no slight impediment to the advance of a traveller. Mr. Vines on Tupungato had to climb over 2000 feet of them.

After leaving the Aconcagua region the journey was continued by sea down the secluded and tempestuous Smyth's Channel, and a brief visit was paid to the snowy mountains of Tierra del Fuego. The ascent of Mount Sarmiento was accomplished to the foot of the final crags, but a violent storm then necessitated a hasty retreat. The glaciers of this southermost region present many analogies with those of Spitsbergen and other places in high latitudes.

GENERAL MONTHLY MEETING,

Monday, June 5, 1899.

His Grace The Duke of Northumberland, K.G. F.S.A., President, in the Chair.

Sir Edward Courtenay Boyle, K.C.B.
Robert Macnamara Cowie, Esq. L.R.C.P. M.R.C.S.
Henry Hardinge Cunynghame, Esq.
Mrs. Hardinge Cunynghame,
Henry Williams Grigg, Esq.
O. J. Kilvington, Esq.
Thomas Matthews, Esq.
Mrs. T. Lambert Mears,
William Beswick Myers-Beswick, Esq.
Theodore Charles Owen, Esq.
Mrs. R. Palmer Thomas,
Thomas Terrell, Esq. Q.C.
Mrs. Twopeny,
Rev. Charles Edward Wright, M.A.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to the Right Hon. Evelyn Ashley for his present of a letter written by

Count Rumford to Lady Palmerston. The following are copies of Mr. Ashley's letter and of Count Rumford's letter:—

Dear Sir, Broadlands, Romsey, *May 8th*, 1899.

Looking over some old papers I have come across the enclosed—namely, a letter from Count Rumford to Lady Palmerston, dated Feb. 2nd, 1799, and giving a sketch of the proposed scheme for a "Royal Institution." It may perhaps be of some interest to the Fellows on the occasion of their Centenary, and I therefore offer it for their acceptance.

Yours faithfully,
(Signed) Evelyn Ashley.

Count Rumford was a great friend of the Palmerstons of that day. I have many letters, &c., here from him.

Brompton Row, *2nd Feb.* 1799.

It would be difficult for you to form any just idea, my dear friend, of the pleasure I should have to be able to put the affairs of your little but useful Institution on the most perfect footing *immediately*, but that alas! is not in my power. To its being the more extensively useful it must be so arranged in all its parts, and more especially in all its *mechanical contrivances* to serve as a model for your neighbourhood. But the model of that model is not yet quite finished,—nor are all the moulds made that must serve for the castings at the founders. These things are however all in hand, and the moment they shall be finished, and that I shall have instructed thoroughly any one workman so that I can be sure that he will be able to execute the job without committing any fault. To show you that I have not been inattentive to the business I send you a drawing for your new kitchen which I made as soon as I got the plan of the buildings. I should have sent you the materials for it, and the workman to put them up, had they been ready.

I think if you read my *Second* Essay you will see that something may be done *en attendant.* Your Housekeeper may open a day school for children;—and with the information you will find in my *third* Essay and that published by the Society for bettering the condition of the Poor, she may contrive to give them useful employment, and a good warm soup for dinner to encourage their industry.

I wish it were in my power to give you specific directions in writing for all the details of the management of your Establishments. But have I not already explained these matters in my writings?

I am just at this moment engaged in a business that *may* prevent my intended journey to America, or at least determine to postpone it. We are considering of a scheme for forming in London a Public Institution for diffusing the knowledge and facilitating the introduction of useful mechanical improvements, and for teaching by means of regular courses of Philosophical Lectures and Experiments the application of Science to the common purposes of life. I shall depend on you, and on Lord Palmerston to give your support to this most useful and most interesting scheme.

It is proposed to put the execution of the plan, and the sole management of all affairs of the Institution, into the hands of *seven* Managers, one of whom to be—your most obedient Servant. As soon as the scheme shall be digested and properly drawn up in writing I shall not fail to send it to you. It is proposed that the subscribers of 50 guineas paid once for all should possess *for ever* all the property belonging to the Institution, and these shares to be transferable by sale, gift, legacy, &c. Each such subscriber to have *two* tickets, *transferable*, of admission to every part of the Institution—and also *two* tickets likewise *transferable* of admission to all Public and Private Lectures and Experiments. The choice of the Managers, and of a Committee of Visitors to be exclusively vested in Subscribers of this *first class.* Subscribers of the *second* class at 10 guineas, to be

free of the Institution for life, and the *third* class, at 2 guineas, to have the entrée for *one year*. But the tickets of the second and third classes *not to be transferable.*

One very interesting part of the Institution will be a grand collection of useful machines—all at work, and a collection of models of useful mechanical Inventions, with Workshops for giving instructions to Tradesmen and Manufacturers; and a magnificent Laboratory for making Chemical and Philosophical Experiments.

I shall be a subscriber of the first class, and I am not sure but I shall make my daughters subscribe also.

Tell me what you think of this scheme.

I am ever, as you well know,

For Yours most faithfully,

Lady Palmerston. (Signed) RUMFORD.

The Special Thanks of the Members were returned to Mr. Hugh Spottiswoode for his valuable gift to the Royal Institution of his father's collection of physical apparatus.

The Special Thanks of the Members were returned to Mrs. Wigan for her donation of £10 10*s.* and to Mr. Thomas A. Rogers for his donation of £10 10*s.* to the Fund for the Promotion of Experimental Research at Low Temperatures.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Memoirs of the Geological Survey of India, Vol. XXVIII. Part 1. 8vo. 1898.

The Meteorological Office—Meteorological Observations at Stations of the Second Order for 1895. 4to. 1899.

Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. 1° Semestre, Vol. VIII. Fasc. 8, 9. 8vo. 1899.

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American Geographical Society—Bulletin, Vol. XXXI. No. 2. 8vo. 1899.

Asiatic Society of Bengal—Journal, Vol. LXVII. Part 1, No. 4, Part 3, No. 2. 8vo. 1898–99.

Proceedings, 1898, Nos. 9–11; 1899, Nos. 1–3. 8vo.

The Kaçmīraçabdāmrta, Part 2. 8vo. 1899.

Astronomical Society, Royal—Monthly Notices, Vol. LIX. No. 7. 8vo. 1899.

Bankers, Institute of—Journal, Vol. XX. Part 5. 8vo. 1899.

Berlin, Royal Prussian Academy of Sciences—Sitzungsberichte, 1899, Nos. 1–22. 8vo.

British Architects, Royal Institute of—Journal, 3rd Series, Vol. VI. No. 13. 4to. 1899.

British Astronomical Association—Memoirs, Vol. VIII. Part 1. 8vo. 1899.

Journal, Vol. IX. No. 6. 8vo. 1899.

Camera Club—Journal for May, 1899. 8vo.

Canadian Institute—Proceedings, Vol. II. Part 1, No. 7. 8vo. 1899.

Chemical Industry, Society of—Journal, Vol. XVIII. No. 4. 8vo. 1899.

Chemical Society—Journal for May, 1899, and Supplementary Number, 1898. 8vo.

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Athenæum for May, 1899. 4to.
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Author for May, 1899. 8vo.
Bimetallist for May, 1899. 8vo.
Brewers' Journal for May, 1899. 8vo.
Chemical News for May, 1899. 4to.
Chemist and Druggist for May, 1899. 8vo.
Education for May, 1899.
Electrical Engineer for May, 1899. fol.
Electrical Engineering for May, 1899. 8vo.
Electrical Review for May, 1899. 8vo.
Electricity for May, 1899. 8vo.
Engineer for May, 1899. fol.
Engineering for May, 1899. fol.
Homœopathic Review for May, 1899. 8vo.
Horological Journal for May, 1899. 8vo.
Industries and Iron for May, 1899. fol.
Invention for May, 1899.
Journal of Physical Chemistry for April, 1899. 8vo.
Journal of State Medicine for May, 1899. 8vo.
Law Journal for May, 1899. 8vo.
Life-Boat Journal for May, 1899.
Lightning for May, 1899. 8vo.
London Technical Education Gazette for May, 1899. 8vo.
Machinery Market for May, 1899. 8vo.
Nature for May, 1899. 4to.
New Church Magazine for May, 1899. 8vo.
Nuovo Cimento for March and April, 1899. 8vo.
Photographic News for May, 1899. 8vo.
Physical Review for April, 1899. 8vo.
Public Health Engineer for May, 1899. 8vo.
Science Abstracts, Vol. II. Part 5. 8vo. 1899.
Science Siftings for May, 1899.
Travel for May, 1899. 8vo.
Tropical Agriculturist for May, 1899.
Zoophilist for May, 1899. 4to.

Florence, Biblioteca Nazionale Centrale—Bolletino, No. 321. 8vo. 1899.

Franklin Institute—Journal for May, 1899. 8vo.

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Geological Society—Geological Literature added to Library during 1898.
Quarterly Journal, No. 218. 8vo. 1899.

Harlem, Société Hollandaise des Sciences—Archives Néerlandaises, Série II. Tome II. Liv. 5. 4to. 1899.

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Johns Hopkins University—American Chemical Journal for May, 1899. 8vo.

Life-Boat Institution, Royal National—Annual Report for 1899. 8vo.

Manchester, Literary and Philosophical Society—Memoirs and Proceedings, Vol. XLIII. Part 1. 8vo. 1898–99.

Manchester Steam Users' Association—Boiler Explosions Acts: Reports 1037 to 1120. 8vo.

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Mersey River, The Acting Conservator—Report on the Present State of the Navigation of the Mersey (1898). 8vo. 1899.

Munich, Royal Bavarian Academy of Sciences—Sitzungsberichte, 1899, Heft 1. 8vo.
Neue Annalen der K. Sternwarte in München, Band III. 4to. 1898.

Navy League—Navy League Journal for May, 1899. 8vo.
Nova Scotian Institute of Science—Proceedings and Transactions, Vol. IX. Part 4. 8vo. 1898.
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Onnes, Prof. H. K.—Communications from the Physical Laboratory at the University of Leiden, Nos. 47, 48. 8vo. 1899.
Paris, Société Française de Physique—Bulletin, Nos. 132, 133. 8vo. 1899.
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Selborne Society—Nature Notes for May, 1899. 8vo.
Smithsonian Institution—Smithsonian Report for 1897. 8vo. 1898.
Society of Arts—Journal for May, 1899. 8vo.
Streatham Public Libraries—Eighth Annual Report, 1898. 8vo. 1899.
Tacchini, Prof. P. Hon. Mem. R.I. (the Author)—Memorie della Società degli Spettroscopisti Italiani, Vol. XXVIII. Disp. 2ª, 3ª. 4to. 1899.
United Service Institution, Royal—Journal for May, 1899. 8vo.
United States Department of Agriculture—Year Book of the Department of Agriculture for 1898. 8vo. 1899.
Experiment Station Record, Vol. X. No 8. 8vo. 1899.
North American Fauna, No. 14. 8vo. 1899.
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United States Patent Office—Official Gazette, Vol. LXXXVII. Nos. 5-8. 8vo. 1899.
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Vizagapatam, G. V. Juggarow Observatory—Report for 1897. 8vo. 1899.
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Zurich, Naturforschende Gesellschaft—Vierteljahrsschrift, Jahrg. XLIII. Heft 5. 8vo. 1899.
Neujahrsblatt, 101 Stück. 4to. 1899.

CENTENARY OF FOUNDATION, 1799–1899.

In the month of June, 1899, the Royal Institution of Great Britain completed one hundred years of its existence, the first meeting of its members in the building in Albemarle Street having been held on June 5, 1799.

The President, Managers and Professors, having decided that this event, so interesting and memorable in the life of the Institution, and in the history of Science in this country, should be duly celebrated, invited many eminent scientific representatives from other countries to take part in the proceedings of the Centenary Celebration.

Monday, June 5, 1899.

CENTENARY BANQUET

TO THE GUESTS OF THE INSTITUTION.

The guests were entertained by the President and Managers at a Centenary Banquet, held in the hall of the Merchant Taylors' Company, kindly lent for the purpose, on Monday, June 5, and had the honour of meeting His Royal Highness the Prince of Wales, K.G., Vice-Patron of the Royal Institution of Great Britain. His Grace the Duke of Northumberland, K.G., the President, in the Chair.

There were also present His Royal Highness the Duke of Cambridge, K.G., the Earl of Halsbury (Lord Chancellor), the Earl of Rosse, K.P., Lord Lister (President of the Royal Society), Lord Kelvin, Lord Iveagh, M.P., Lord Strathcona and Mount Royal, Sir John Lubbock, Bart., M.P., Professor A. Cornu (of Paris), Sir George Stokes, Bart., Henry White, Esq. (Secretary American Embassy), S. P. Langley, Esq. (of Washington), Lord Amherst of Hackney, Lord Blythswood, Professor Oscar Liebreich (the delegate from the German Chemical Society), George W. Barnard, Esq. (Master of the Merchant Taylors' Company), The Right Rev. the Lord Bishop of Bristol, Rear-Admiral A. K. Wilson, Sir James Crichton-Browne, Sir Frederick Bramwell, Bart., Lord Rayleigh, Professor Dewar, and about two hundred other Gentlemen.

The gallery behind the Chairman's seat was occupied by ladies throughout the evening.

The Duke of Northumberland (the President of the Royal Institution), in proposing "The Health of Her Majesty the Queen, Empress of India," remarked that it appealed in a peculiar manner to her Majesty's subjects at the present moment, because we had just had the satisfaction of celebrating her eightieth year, and congratulating her upon the health and strength which Providence had

bestowed upon her, as well as recording our sense of the admirable manner in which, through so lengthened a period, she had ruled over us. That night they drank her health also as Patron of the Royal Institution. She, like many of her illustrious predecessors, had shown that she realised the importance of the work which the Institution was formed to carry out, and how largely, either directly or indirectly, it tended to the welfare of her subjects. That welfare had ever been the dearest thing to the heart of her Majesty to secure, and it had been the guiding principle of her public conduct; and in addition to her private virtues, the knowledge and the appreciation of that fact had endeared her to the hearts of her people.

The toast having been duly honoured,

The Duke of Northumberland (the President), again rising, proposed "The Health of the Vice-Patron, the Prince of Wales, the Princess of Wales, and the rest of the Royal Family." They were much honoured, he said, by the presence of the Prince of Wales. The associations which his Royal Highness had with the Royal Institution went back a considerable time, for he believed that it was in the year 1855 that the Prince first attended a lecture at the Royal Institution. It was one of those juvenile lectures which Professor Faraday so well knew how to deliver. They were unfortunate that night in losing the presence of the Duke of York, who had been a member of the Institution for so many years, but they were fortunate in securing—in the capacity of a distinguished guest—the presence of the Duke of Cambridge.

The Prince of Wales said, I am most grateful to the Duke of Northumberland for the kind terms in which he has proposed this toast, and to you for the manner in which you have received it. I consider it a great privilege, and a great honour, to take part, as Vice-Patron of this Institution in the hundredth anniversary of its existence. As the Duke has said, I had an early acquaintance with the Royal Institution. Though it is nearly half a century ago, I have not forgotten that, as a boy, my brother, the Duke of Saxe-Coburg, and myself were sent by our father to London, just after Christmas, to attend those famous lectures, which were then given by the great Professor Michael Faraday. I have not forgotten the interest which we took in those lectures, and the clear way in which Professor Faraday explained to boys difficult scientific problems, and the beautiful way in which he showed us the chemical experiments which were then the order of the day. On an occasion of this description it is very difficult to say anything new with regard to the Institution, or anything that is not known by so distinguished and able an audience as is assembled here to-night. When one looks back, one recalls that one hundred years ago the Society was first formed by Sir Benjamin Thompson, who was better known, and who himself liked to be known, under the name of Count Rumford. It is remarkable, that the building in Albemarle Street, though much changed in architecture, is the identical one in which the lectures were given and the work was

carried on. Of course, this Society has gone through many vicissitudes, but fortunately many people assisted it in a pecuniary way, and only as recently as three years ago we were indebted to the liberality of Dr. Mond for the Davy Faraday Laboratory, at the opening of which, I myself had the pleasure of being present. When one looks back to the eminent men who have worked and lectured at the Institution, one thinks first of the name of Thomas Young, and of the great Humphry Davy—a man of whom all Englishmen are proud, and one of whose most remarkable scientific discoveries, perhaps, was that wonderful lamp which has saved the lives of thousands of miners. Then there is another name to which in this connection I must again refer, that of Professor Michael Faraday, whom I knew, and whom I shall ever associate in my mind with the Royal Institution; I should also mention his distinguished successor, Professor Tyndall. There is another name, which at the present day everyone looks up to as one very remarkable in every branch of science—that of Lord Rayleigh. The President of the Royal Institution, and many of us, I hope, will have the pleasure of listening to a lecture which he is to deliver to-morrow. As the Duke has kindly said, and I think very properly said, we are a large gathering. Still, on this occasion, speeches should be short—I will therefore not detain you longer. I thank you once more for having kindly listened to the words which I have said; and I assure the Duke and the members of this distinguished Society how highly I appreciate the opportunity which has been given me to take part in this Centenary banquet, and how pleased we are to see present so many distinguished foreign gentlemen connected with Science. To the Duke of Northumberland it must be especially gratifying that he and members of his family now, for upwards of fifty years, with the exception of eight years, when Sir Henry Holland was president, have occupied the chair as presidents of the Royal Institution. In the name of the Princess of Wales and other members of my family, I tender you my warmest thanks for the manner in which you have received this toast.

The Duke of Cambridge proposed "The Royal Institution of Great Britain." He said the Prince of Wales had already pointed out the salient points of the Institution, and, therefore, all that he could really do was to back up what the Prince had said. They were commemorating that night the Centenary of an Institution for diffusing knowledge, and facilitating the general introduction of useful mechanical inventions and improvements. Whilst during these one hundred years the march of intellect had largely increased in every part of the world, and nowhere more than in this country, there had also been a change in scientific matters to an extent which, having himself lived eighty years he could not have thought possible. In those one hundred years everything had changed, more or less, in every department of life, but nothing had gone forward more rapidly than science, which was still progressing in a sound and useful direction and which they of that Institution were helping to support and

fructify. He thought the world was greatly indebted to the Institution for the progress made, but the members of the organisation had always made progress with prudence and intelligence, and without rashness. Nowadays, they saw new things introduced which were not always carried out with the judiciousness they would desire; but their Institution was sound to the core, because it had gone on continuously, without rashness and with all that intelligence which was worthy of such an important Society. They rejoiced to find that their feelings were gradually becoming general throughout the world. There was reciprocity in science, and whilst they were delighted to see the representatives of science of other nations, they, he hoped, were ready at all times to hear and appreciate the endeavours made in this country of those engaged in scientific work. That was one of those principles which should go to regulate the world in the future. He was himself a soldier, and had been for years, and he felt they were greatly indebted as soldiers to that Institution for having introduced improvements which made wars, not impossible, but very much less likely than they used to be. At the same time, they were heart and soul loyal to the Crown, and loyal to themselves, and when they had occasion to show that loyalty they were quite ready as heretofore, as sailors and soldiers, to do their duty. Scientists had made war much less likely and much less possible, but he sincerely hoped that every endeavour made at the Hague Conference to modify the miseries of war by judicious and prudent regulations would be crowned with the success that it deserved. The progress of the world was extremely indebted to a large number of the eminent and great scientific men who surrounded him that evening, and he was sure they would all join in a hearty welcome to the present Duke of Northumberland, and would drink to the prosperity and the extension of the usefulness of that great body of men known as the Royal Institution. He gave the toast of "the Prosperity of the Royal Institution," and coupled it with the name of the Duke of Northumberland, who had succeeded his distinguished father, the late Duke of Northumberland, as President of the Institution.

The Duke of Northumberland, in acknowledgment of the toast, said that the appreciation expressed by his Royal Highness of the work of the Institution and the reception of the toast were signal proofs of the esteem in which the work of the Institution was held. It was a great honour that so many eminent representatives of foreign science had honoured with their presence the Centenary of the Institution. It was just one hundred years ago when the Institution entered upon its present premises. A long roll of names had lent lustre to their labours. Davy, Faraday, Young, Tyndall—above all they should remember their founder, Benjamin Thompson, Count Rumford, whom it was easy to criticise, but whose virtues had been productive of great results. The work of the Institution had been in large measure the carrying out of Count Rumford's ideas. It was said

that he intended an institution of a more practical or industrial character than the Institution now was. But changes had taken place. Facilities for communicating new discoveries were one hundred years ago few; competition was less keen; there was then much dislike of innovation and there was extreme jealousy with the working classes of any reduction of manual labour. It was thus necessary to popularise discoveries; and that was the aim of their founder. But now every such discovery was soon heralded to the public. Popular magazines had now articles on the manufacture of liquid air and other subjects of an abstruse character. Towards this wide diffusion of science the Royal Institution had largely contributed. Their principal objects were research, for which their laboratories gave such ample means and in respect of which special gratitude was due to Dr. Mond for his noble gift, and to Mr. Spottiswoode for his collection. The second object was to bring the results of research to the knowledge of those who could appreciate them, and these results were expounded in the evening lectures of the Institution. Thirdly, this knowledge was popularised by the afternoon lectures; and finally the rising generation were stimulated by the juvenile lectures to those who, it was hoped, were destined to take their part in future scientific investigation. They had an admirable library of the subjects which they sought especially to promote. They did not limit their interest to pure science, but literature and history had also been the subjects of admirable discourses by acknowledged masters. Lastly, he would venture to say that nothing would be heard in the Institution which could wound the most sensitive in the subjects of history, moral philosophy, or religion. Lord Rayleigh, Professor Dewar, and others had shown that the ordinary wants of mankind were not foreign to their purposes. Their gratitude on this occasion was due to the Merchant Taylors' Company for the use of their beautiful hall.

The toast of "The Guests" was proposed by the Lord Chancellor, who observed that he presumed the toast was entrusted to him as one who had sat as a very diligent listener at the feet of some of those Gamaliels of science he saw around him. It was unnecessary for him to add much to what had already been said by those taking part in the proceedings as to the importance of the Royal Institution, for all would recognise the incalculable benefits it had conferred on all who came within the sphere of its influence. He had some difficulty in proposing the toast, because he was not certain as to how many present belonged to the scientific family they designated the Royal Institution; but he would couple the toast with the names of two of their guests who came from over the sea—Mr. S. P. Langley, Secretary of the Smithsonian Institution, Washington, and Professor A. Cornu, Officier de la Légion d'Honneur, Membre de l'Institut, France.

Mr. S. P. Langley said, I could wish that another, whose words would carry more weight than mine, were here to respond to these

kindly expressions in the name of your guests, and especially of those from America; but I may at least feel that no one can speak with more sincerity of the pleasure those from the United States have in being here to-night, and in testifying to their grateful remembrance of all that American science owes to this, its mother-country.

We cannot forget that it was from the heart of the English people that those earliest colonists of New England came, who in the last century, in token of their ancestry, produced a Franklin and a Rumford, or forget that it is strictly true that American science, during the generation that followed the foundation of this Institution, grew up under almost exclusively English influence.

All your guests, without distinction of country, appreciate at its high value the example which this Institution has set, in uniting original research of the very greatest importance with the communication of its results in a form understanded of the people; but we in America are especially glad to remember that it was an English man of science, James Smithson, who, following your example of this twofold purpose, left his fortune to the United States to, in his own brief and pregnant words, found "an establishment for the increase and diffusion of knowledge among men."

The American Government accepted the trust in a way without precedent, placing by organic law the President of the United States at the head of the Institution, and giving the regency of its affairs to a board representing whatever is most eminent in the councils of the nation, under whose direction it has pursued the same double objects as your Institution, both of the increase of knowledge by original research, and its diffusion throughout the world. It has done this with a faithful regard to the wishes of its founder, who died before the fruition of his large purpose, so that this modest man of science did not live to see in the Smithsonian Institution his work enduring in an honoured name.

The American founder of your great Institution was more fortunate, in seeing at least the beginning of his work, but even of him it might be almost said that he "builded better than he knew," for while we are glad to remember that Rumford has thus associated his name with your early history, we must agree that if the seed he planted had not been sown in so good a soil, his earlier plan would not have grown into what the years have wrought, not only in bringing the most eminent contributions to the sum of human knowledge, but in their publication under royal patronage, for it is thus, in the worthiest sense that word may bear, continued under the gracious lady whose birthday has just been celebrated with rejoicings on both sides of the Ocean, which here, perhaps more than in any other land, has led all that is best in social life to take an interest in the exhibition of the results that such men as your Young, your Davy, your Faraday and your Tyndall have wrought.

It is known to all, on both sides of the Atlantic, that the work of these men is being continued by one who has added a new element

to our atmosphere, and has opened the way to others, and by another who has just been near to the boundaries of warmth and life, to bring back liquid hydrogen to your lecture table; and such work is still being so carried on, that in all your honoured years, none have been more pregnant in results of world-wide interest than those which close this, your first century.

If the naturalist tells us truly, that we can forecast the probable duration of the future life of any organic being from the time that it takes to reach adolescence, we shall see in the hundredth year of such performance and such promise, not age, but youth, and the expectation of a growth to continue through centuries to come.

Such at least, I think, is both the belief and the wish of your scientific kinsmen and guests, from beyond both the broad and narrow seas, and certainly of those present to-night, who return their thanks, through my imperfect voice, for the hospitality which has enabled them to be present, under such auspices, on so memorable occasion.

Professor Cornu, D.C.L. F.R.S., said, C'est en français que je vais répondre au nom des invités du Continent; je choisis le français parce que c'est la langue diplomatique: excellente raison! elle me dispense d'en donner une autre qui serait peut être encore meilleure.

L'histoire de la fondation de la Royal Institution, que j'ai relue ces jours derniers, m'a fait une impression profonde. Nous connaissions tous Benjamin Thompson, Comte de Rumford, comme un physicien pénétrant, auteur d'importantes découvertes sur la chaleur et la lumière; comme le précurseur des théories modernes de l'énergie, le premier qui ait osé dire devant la Société Royale *que la chaleur ne peut pas être autre chose que du mouvement.* Aussi la Société Royale a-t-elle consacré la memoire des fécondes études de Rumford par une médaille, l'une des plus hautes distinctions auxquelles les physiciens puissent aspirer.

Nous connaissions également le Comte de Rumford comme un ami sincère de l'humanité pauvre et souffrante.

Nous savions qu'il avait fondé l'Institution Royale pour la diffusion des sciences, le perfectionnement de l'industrie et l'accroissement du comfort de la vie domestique.

Mais j'ignorais, pour ma part, que sa pensée avait été plus haute encore et qu'en réclamant de ses contemporains la fondation d'une institution utile à la prospérité nationale il s'était élevé à une conception aussi généreuse que nouvelle de l'influence de la science sur le progrès social.

Écoutez ses belles paroles:

"But, in estimating the probable usefulness of this Institution, we must not forget the public advantages that will be derived from the general diffusion of a spirit of experimental investigation and improvement among the higher ranks of society.

"When the rich shall take pleasure in comtemplating and encouraging such mechanical improvements as are really useful, good taste, with its inseparable companion, good morals, will revive;

rational economy will become fashionable; industry and ingenuity will be honoured and rewarded; and the pursuits of all various classes of society will then tend to promote the public prosperity." *

Heureuses les nations qui savent comprendre ces généreuses idées !

Heureuses les nations qui sont capables, comme la Grande Bretagne, de fonder, de maintenir, et de faire prospérer des telles institutions !

Je crois être l'interprète de mes collègues du Continent en exprimant toute notre admiration pour l'œuvre rêvée et accomplie par Rumford et en buvant à la prospérité indéfiniment séculaire de l'Institution Royale, qui depuis un siècle, à travers tant d'événements inattendus, a conservé, grace aux illustres savants qu'elle choisit, toujours l'admirable esprit de son fondateur.

TUESDAY, JUNE 6, 1899.

H.R.H. THE PRINCE OF WALES, K.G., Vice-Patron, in the Chair.

COMMEMORATION LECTURE,

By the RIGHT HON. LORD RAYLEIGH, M.A. D.C.L. LL.D. F.R.S., Professor of Natural Philosophy *R.I.*

There were also present the Duke of Northumberland (President) the Duke of Devonshire, Lord Lister (Pres. R.S.) Lord Kelvin, Lord Amherst, Sir John Lubbock, Bart. M.P., Sir John Dorington, Bart. M.P., Sir Frederick Abel, Bart., Sir Edward Frankland, Sir Andrew Noble, Sir Henry Thompson, Bart., Sir William Crookes, Dr. J. H. Gladstone, Professor Silvanus P. Thompson, Sir James Crichton-Browne, Sir Frederick Bramwell, Bart., Dr. Ludwig Mond, and Professor Dewar.

LORD RAYLEIGH said that though his was intended to be a commemorative lecture, the idea of commemorating all the work that had been done at the Royal Institution was hopeless. To do so he would require, not one lecture, but many courses of lectures, even though much of it had been in chemistry, which did not fall within his province. Remembering that on other occasions he had spoken in detail of the achievements of Faraday and Tyndall, he thought on this occasion he would do well to go still further back in the century

* The Royal Institution: its Founder and its First Professors. By Dr. Bence Jones (1871), page 147.

and speak of Dr. Thomas Young, one of the earliest professors of the Institution. Young occupied a very high place in the estimation of men of science—higher, indeed, now than at the time when he did his work. His "Lectures on Natural Philosophy," containing the substance of courses delivered in the Institution, was a very remarkable book, which was not known as widely as it ought to be. Its expositions in some branches were unexcelled even now, and it contained several things which, so far as he knew, were not to be found elsewhere. The earlier lectures dealt with elementary mechanics, and the reader would find as sound an exposition of that science as could be imagined. It was to Young that they owed the term *energy*, now in everybody's mouth. Elastic resilience was better dealt with there than in any other treatise he knew of, for Young discussed the subject with remarkable ingenuity, showing that the phenomena exhibited by two bodies coming into collision were comprehended under it. If the velocity was moderate, all their motion might be taken up in them in the form of potential energy; but if it exceeded a certain limit their integrity could not be preserved. In the case of a grain of sand projected against a sheet of glass, another element, that of time, had to be considered, for it became a question of the propagation of the wave set up by the impact, and if the region traversed by the wave during collision, and alone available as the seat of potential energy, were too small the glass was bound to break. Young again discussed the problem of a ball supported on a column of air or water, and correctly explained that it preserved its stability and did not fall out of the stream owing to centrifugal force. In the province of sound Young was the originator of many of the most important principles on which the docrine was now expounded, but it was with optics that his name was most closely associated, for he and Fresnel were the builders of the great structure of the undulatory theory. This was a matter that was tolerably familiar. Lord Rayleigh thought he could best utilise the time at his disposal by mentioning some of the points in which Young's good work had been overlooked. In his time a question of discussion was the change of the focus of the eye for varying distances. One suggested explanation, that accommodation was effected by an alteration in the external convexity of the eye, Young proved to be wrong by drowning his eye in water. This virtually eliminated the convexity, yet the power of accommodation remained; and he therefore concluded it was due to a muscular alteration in the internal lens of the eye. He also described the phenomenon of astigmatism and showed his deep knowledge of optical theory by suggesting that its effects could be counteracted by the use of a slightly sloping lens. In the study of compound colours, or chromatics as it was then termed, Young's views were correct, though not universally accepted even yet. Lord Rayleigh showed a modification of the experiment by which he proved that the combination of green and red gave yellow, and illustrated the fact by a further experiment, not Young's, but following his suggestions, which demonstrated to the

audience that when the blue and yellow of the spectrum were cut off by solutions of litmus and bichromate of potash respectively the combination of the remaining red and green was obviously yellow. The lecturer next described Young's way of getting rid of the "false light," that interfered so greatly with the brilliance of the effects when Newton's rings were being obtained by means of two glass plates pressed together, and by analysing the colours from such plates with a prism he exhibited the original of a diagram in Young's book, which indicated the particular rays destroyed by interference. Young was singularly successful in the theory of cohesion and capillarity, in which some of his earliest work was done, and he was the first to deduce an estimate of molecular dimensions from *data* afforded by that theory. The size of the molecule, according to his calculations, was not very different from that admitted at the present day. In the theory of the tides he made great advances, and in explaining the circumstances which determine whether there shall be high or low water under the moon, he gave the general theory of forced vibrations. His views of heat were very interesting. He had the utmost contempt for the idea widely prevalent in his time that it was a separate entity, and expressed the hope that before long philosophers might return to a true conception of its nature as motion. Lord Rayleigh, in concluding his observations on Young, said that possibly he had left the impression that Young knew everything. In fact, it was seldom that he was wrong; but just to show that he was, after all, human, a passage might be quoted from his book in which he declared there was no immediate connection between magnetism and electricity! Speaking of work which had been done at the Institution by men who held no regular appointment in it, the lecturer noted that Wedgwood, in conjunction with Davy, was the first to produce anything that could be called a photograph, while instantaneous photography, such as was required for rapidly moving objects, was carried out for the first time by Fox Talbot in the laboratory of the Institution. Slides were exhibited illustrative of flying bullets, splashing milk, and breaking soap films, all taken by the electric spark. Towards the close of the lecture Lord Rayleigh showed one famous experiment of Faraday's, the rotation of the plane of polarised light by magnetism, which he observed had acquired a new interest from the recent discoveries of Dr. Zeeman. In illustration of Tyndall's work, he instanced the discovery of sensitive flames and their application to acoustical investigation. The analogue of a remarkable optical experiment, from which it appears that there is a bright spot at the centre of the shadow of a circular disc, was exhibited.

Sir James Crichton-Browne said,

May it please your Royal Highness,

The Royal Institution having resolved to mark its Centenary by adding to its roll of Honorary Members the names of some of the most eminent representatives of physical and chemical science on the

Continent and in America, I beg leave to present to you the gentlemen who have been selected for that distinction and who have honoured us with their presence here to-day.

All of them have done worthy and memorable work in the field of science, all of them are of world-wide reputation, and it is unnecessary therefore in an audience like this, and it might be tedious and embarrassing to them that I should recount the offices, achievements, publications and honours of each, and so I shall only present them nominally in asking your Royal Highness to admit them to the Honorary Membership.

The diplomas which have been prepared for them they will carry back with them to almost every country in Europe and to the United States of America—whence came the founder of the Royal Institution to these shores just one hundred and twenty-three years ago—and we feel sure that these diplomas will have an enhanced interest and value for all of them because they are bestowed by the hand of your Royal Highness.

Our conference here this afternoon accentuates the universal brotherhood of science, and so may perhaps do something to promote the concord of the nations.

I have the honour to present the following gentlemen :—Dr. Émile Ador (Geneva), Professor Joseph S. Ames (Baltimore), Professor Svante Arrhenius (Stockholm), Professor George F. Barker (Philadelphia), Professor Carl Barus (Providence, U.S A.), Professor Henri Becquerel (Paris), Dr. L. Bleekrode (The Hague), Professor Giacomo Luigi Ciamician (Bologna), Professor Nicolas Egorof (St. Petersburg), Professor Antoine Paul Nicolas Franchimont (Leiden), Professor Heinrich Gustav Kayser (Bonn), Professor Wilhelm Korner (Milan), Mr. Samuel Pierpoint Langley (Washington), Professor Oscar Liebreich (Berlin), Professor Gustave Leonard Van der Mensbrugge (Ghent), Professor Albert A. Micholson (Chicago), Professor Henri Moissan (Paris), Professor Raffaelo Nasini (Padua), Professor Walther Nernst (Göttingen), and Mr. Ernest Solvay (Brussels). The diploma of honorary membership will be forwarded to the following, who are unable to be present :—Professor Armand Émile Gautier (Paris), Professor Wilhelm Ostwald (Leipzig), Professor Robert H. Thurston (Ithaca), Professor Emilio Villari (Naples), Professor Jules Louis Violle (Paris), and Dr. William L. Wilson (Washington).

As it was thought essential that the Centenary of the Royal Institution should be celebrated in this place, its old, its first, and only home, it was found impossible from want of room to invite delegates from universities, colleges, societies and academies, as the managers would otherwise have wished to do; but notwithstanding that no invitations have been issued, two foreign learned societies have spontaneously sent addresses of congratulation. The German Chemical Society, and the German Society of Chemical Industry, felicitate the Royal Institution on the completion of one hundred years of its existence, generously acknowledge the splendid work it

has accomplished without Governmental aid, and simply by private enterprise and individual effort, and hope for it a continuance of that beneficent activity which has never flagged from the days of Rumford down to those of Rayleigh and Dewar.

The addresses are most gratefully received and will be published in the Proceedings of the Institution.

The Duke of Northumberland moved a vote of thanks to the Prince of Wales for presiding.

The Duke of Devonshire, in seconding the motion, said that the Royal Institution had contributed in no small degree to the extraordinary advance of science that the century had witnessed. It was entirely in accordance with the principles that guided his Royal Highness in public life that he should have taken a prominent part in the celebration of the Institution which had become a national one. It was obvious that such an Institution as that could not perform all the work of which it was capable unless it met with a large share of public support, and no small element in obtaining that support was the countenance of his Royal Highness.

The Prince of Wales said: I am deeply sensible of the kind words which have fallen from the lips of the Duke of Northumberland and the Duke of Devonshire. I need hardly assure them, nor any of you ladies and gentlemen present, that I shall always look back with the deepest pleasure and gratification on the fact that I have taken part in the Centenary of the Royal Institution. Having been acquainted with it from my earliest years, and having had the advantage of listening to many of the great scientific men who have given their lectures and shown their experiments in this room, I am glad to think that I have been present on this occasion to hear the interesting, able and exhaustive lecture which Lord Rayleigh has so kindly given us with his excellent experiments. He has been able to go over much ground in a very limited space of time. The interest that I take in this Institution, I assure you, will never be diminished. It has, as the Duke of Devonshire has said, become a national one. It is self-supporting, and during these hundred years has, no doubt, acquired an amount of scientific knowledge which has been appreciated not only by this country but by every part of the world. Amongst the most pleasant of my duties here to-day I count my having been asked to personally deliver the diplomas to the many distinguished gentlemen who come across the water to join with us in this Centenary Festival. As a Member and a Vice-Patron of the Institution, I beg to acknowledge our gratitude to them for having so kindly given us their cordial greetings and presence on this interesting occasion. I regret that I have not time for more. Let me again express my sincere thanks to you. I leave here with feelings of the deepest gratification at having been present on this occasion.

A vote of thanks was passed to Lord Rayleigh on the motion of Lord Lister, seconded by Lord Kelvin.

The following addresses and congratulations were received, together with letters of congratulation from many eminent scientific men:—

Translation.

The German Chemical Society begs to send its warmest and heartiest good wishes to the Royal Institution of Great Britain in celebration of the hundredth year of its existence.

We look back with admiration on the glorious history of this creation, which far-seeing men called into existence at a time when no similar Institution existed in any other country. True to its original design, the learned Members of the Institution, down to the present day, performed their task of advancing Science and stimulating the interest of educated people in it, thus promoting the well-being of the nation. May the lofty position which the Royal Institution of Great Britain has won for itself during the hundred years of its existence be fully maintained in future generations, for the good of Physical Science and the honour of the English nation.

H. LANDOLT, *President.*
FERD. TIEMANN } *Secretaries.*
ADOLF PINNER }

ROYAL INSTITUTION, LONDON.

The German Physical Society sends its hearty congratulations to the Royal Institution on the celebration of its Centenary, and in so doing recalls to mind the great forerunners Davy and Faraday, as well as their successor, Lord Rayleigh.

WARBURG.

Translation.

TO THE PRESIDENT AND COMMITTEE OF THE
ROYAL INSTITUTION OF GREAT BRITAIN, LONDON.

Moved by feelings of pride and of gratification the Association for the Promotion of the Interests of the Chemical Industry in Germany desires to associate itself with those who, from near and far, are sending their congratulations to the Royal Institution of Great Britain upon this, its Jubilee day.

Just as England was the first among the civilised countries of Europe to exploit the growing achievements of scientific investigation for the furtherance of the national prosperity, so also was the Royal Institution of Great Britain the first corporation to impart practical significance to the beautiful thought that Science is the common possession of the educated world.

It is true that other corporations have since followed in the path thus trodden for the first time, but none has been able to realise the

thought in more admirable form than the Royal Institution, whose methods and operations have served throughout a whole century as the model for all countries.

Supported by the unexampled devotion of its Members, buoyed up by the genius of the great intellects who have been attracted to its service, the Royal Institution has succeeded not only in fostering the love of Science among the educated, but also in extending greatly the scope of our knowledge.

The places where a Davy, a Faraday and a Tyndall operated will remain for all time sacred, not only to the British people, but to all other civilised nations. An eager energetic spirit must for ever dwell in those hallowed halls. May the coming century, to which we all look forward with such large hopes, weave new laurels in the rich wreath of fame which to-day the departing century joyfully deposits at the feet of the Royal Institution.

The Committee of the Association for
The Promotion of the Chemical
Industry in Germany.

Berlin, *5th June*, 1899.

Académie Royale des Sciences, des Lettres
et des Beaux-Arts de Belgique,
Bruxelles, *le* 27 *mai*, 1899.

Monsieur le Président,

J'ai l'honneur de porter à votre connaissance que l'Académie Royale de Belgique, désireuse de s'associer au centenaire de l'Institution Royale de Londres, vient de déléguer M. le Professeur G. Van der Mensbrugghe de l'Université de Gand, à l'effet de la représenter officiellement aux solennités des 5, 6 et 7 juin.

Veuillez agréer, Monsieur le Président, l'expression de mes sentiments les plus distingués.

Le Secrétaire Perpétuel de l'Académie,
(Signed) Le Chevalier Marchal.

À Monsieur le Président de l'Institution
Royale de la Grande Bretagne, à Londres.

Sir Frederick Bramwell, Honorary Secretary,
Royal Institution, Albemarle Street, W.

La Société physique et chimique russe à Pétersbourg, gardant la mémoire des travaux illustres de Rumford, Davy, Faraday et de leurs dignes successeurs, s'empresse de faire parvenir à l'Institut Royal de Londres ses félicitations à l'occasion de son centenaire et joint aux expressions de la plus profonde considération ses vœux pour un avenir non moins glorieux.

Secrétaire, Gorboff.
Président Honoraire, Mendeleieff.

ROYAL INSTITUTION OF GREAT BRITAIN, LONDON.

The Imperial Military Academy of St. Petersburg offers its congratulations to the Royal Institution of Great Britain on its hundred years anniversary, wishing that the splendid activity of the Institution, glorious by great scientific discoveries, should continue for many succeeding ages to the honour of England and to the benefit of mankind.

President, PASHUTIN.

SIR FREDERICK BRAMWELL, ROYAL INSTITUTION, LONDON.

Official duties prevent my accepting the invitation with which I was honoured by the Royal Institution; may it prosper in the century to come, and retain the same prominent position in the advancement of science as in the past.

(Signed) MENSCHUTKIN.

PROFESSOR DEWAR, ROYAL INSTITUTION, LONDON.

Most cordial congratulations with the Centenary of the Institution, to the glory of which you have added so much, and my best wishes for the continuing of your splendid work.

(Signed) KAMERLINGH ONNES.

TUESDAY, JUNE 6, 1899.

RECEPTION AT THE MANSION HOUSE.

The Right Hon. the LORD MAYOR OF LONDON and the LADY MAYORESS gave a RECEPTION to the Members and the guests of the Institution, in the evening, at the Mansion House, City.

WEDNESDAY, JUNE 7, 1899.

DR. AND MRS. MOND gave a GARDEN PARTY in the afternoon to the guests of the Institution at the Poplars, 20 Avenue Road, Regent's Park, N.W.

Wednesday, June 7, 1899.

His Grace the Duke of Northumberland, K.G., President, in the Chair.

COMMEMORATION LECTURE,

By Professor Dewar, M.A. LL.D. F.R.S. *M.R.I.*, Fullerian Professor of Chemistry *R.I.*

There were also present, the Honorary Members, together with Lord Kelvin, Lord Amherst, Sir George Stokes, Sir Andrew Noble, Dr. Ludwig Mond, Sir James Crichton-Browne, Sir Frederick Bramwell, Bart., Sir Frederick Abel, Bart., Sir William Crookes, and Lord Rayleigh.

Professor Dewar said:—

My colleague, Lord Rayleigh, in his Commemoration Lecture, dealt so admirably and exhaustively with some of the discoveries of our great predecessors in this Institution, that it will be unnecessary to pursue further the lines of historical treatment in this lecture. Instead of discoursing generally on the chemical side of the work of Davy and Faraday and their successors, it has seemed to me more appropriate to attempt some experimental demonstrations of the latest modern developments in a field of inquiry opened out to science by the labours of the two illustrious chemists just mentioned. With this object in view, my discourse this evening will be confined to the subject of liquid hydrogen. Davy said: "Nothing tends so much to the advancement of knowledge as the application of a new instrument. The native intellectual powers of man in different times are not so much the causes of the different success of their labours as the peculiar nature of the means and artificial resources in their possession." The new instrument of research, which, for the first time we have to experiment with before an audience, is the liquid form of the old inflammable air of Cavendish. Lavoisier towards the end of the last century had the scientific acumen to declare that in his opinion, "if the earth were suddenly transported into a very cold region, the air, or at least some of the aëriform fluids which now compose the mass of our atmosphere, would doubtless lose their elasticity for want of a sufficient temperature to retain them in that state. They would return to the liquid state of existence and new liquids would be formed, of whose properties we cannot at present form the most distant idea." Black, about the same time, in discussing the properties of hydrogen, makes the following suggestive observations: "We may now further remark with regard to inflammable air, that it is at present considered as one of the simple or elementary bodies in nature. I mean, however, the basis of it, called the Hydrogen by the French chemists; for the inflammable air itself, namely, hydrogen gas, is considered as a compound of that basis

and the matter of heat. What appearance and properties that basis would have, were it deprived of its latent heat and elastic form, and quite separated from all other matter, we cannot tell." The accuracy of the prophecy of Lavoisier has been experimentally verified, but until recently we had no distinctive answer to the riddle of Black. The object of this lecture will be an attempt to advance the solution of the problem suggested by Black a century ago. It is interesting to note how confident Faraday was that hydrogen would ultimately be obtained in the liquid and solid form. In the course of one of his lectures delivered in the year 1852, he said: "There is reason to believe we should derive much information as to the intimate nature of these non-metallic elements if we could succeed in obtaining hydrogen and nitrogen in the liquid or solid form. Many gases have been liquefied; one carbonic acid gas has been solidified; but hydrogen and nitrogen have resisted all our efforts of this kind. Hydrogen, in many of its relations, acts as though it were a metal; could it be obtained in a liquid or solid condition, the doubt might be settled. This great problem, however, has yet to be solved; nor should we look with hopelessness on this solution; when we reflect with wonder —and, as I do, almost with fear and trembling, on the powers of investigating the hidden qualities of these elements—of questioning them, making them disclose their secrets and tell their tales— given by the Almighty to man." It must be confessed, however, that later physicists and chemists were almost forced to conclude that the problem was a hopeless one. The full history of the liquefaction of hydrogen has been dealt with in a Friday Evening Discourse delivered in January of this year, so that all questions dealing with the work of other investigators may for the present be omitted in order to save time for the experimental illustrations.

This large spherical double-walled and silvered vacuum vessel contains one litre of liquid hydrogen. You observe it is lifted out of a large cylindrical vessel full of liquid air. In order to diminish the rate of evaporation it is necessary to surround the vessel in which the hydrogen is collected with liquid air. Under such conditions the rapidity of evaporation is about the same as that of liquid air when kept in a similar vessel in the ordinary way. In order to prove that hydrogen is present in the liquid form, the simplest experiment is to remove the cotton-wool plug which takes the place of a cork, and insert a metallic wire, to the end of which is attached a ball of asbestos for the purpose of absorbing the liquid. On bringing it quickly into the air and applying a light, it burns with the characteristic appearance of the hydrogen flame (Figure C, Plate III.). The liquid can readily be poured from one variety of vacuum vessel into another, so that by means of this unsilvered cylindrical form the appearance of the liquid and other experiments may be projected on a screen (Figure A, Plate III.) The liquid hydrogen appears in gentle ebullition and is perfectly clear, only there is a white solid deposit in the bottom of the tube, which is really solid air. This may be shown

by removing for an instant the cotton-wool stopper, when you see a snow of solid air falling in the liquid. It is easy to arrange a method of carrying liquid hydrogen in a small vacuum vessel in such a way as to prevent the access of air. This is shown in Plate I., where the vacuum vessel, after it has been filled by dipping it into the main supply by means of a supporting wire, is surrounded with a glass envelope, which becomes filled with an atmosphere of hydrogen gas constantly maintained, thereby preventing the access of air. That the density of the liquid is very small and is altogether unlike liquid air is shown by dropping small pieces of cork, which float readily in the latter liquid, but sink instantly in the hydrogen (Figure B, Plate III.). The real density of the liquid is only one-fourteenth that of water, so that it is by far the lightest known liquid. This small density explains the rapidity with which the liquid is cleared on the entrance of the air snow. The relative smallness of the gas bubbles produced in the actively-boiling liquid which causes an appearance of opalescence, is really due to the small surface tension of the liquid hydrogen. The coefficient of expansion of liquid hydrogen is some five times greater than that of liquid oxygen, and is comparable with that of carbonic acid, about 5° from its critical point. The latent heat of evaporation is about 190 units, and the specific heat of the liquid is very high, and so far as my experiments go, leads me to the value 6. This is in very marked contrast to the specific heat of liquid oxygen, which is about 0·5. The extraordinary lowness of its boiling point is at once apparent by cooling a piece of metal in the liquid and then removing it into the air, when it will be seen to condense for a moment solid air on its surface which soon melts and falls as a liquid air. This may be collected in a small cup, and the production of oxygen demonstrated by the ignition of a red-hot splinter of wood after the chief portion of the nitrogen has evaporated. If a long piece of quill tubing sealed at one end, but open at the other, is placed in the liquid, then the part that is cooled rapidly fills with liquid air. On stopping any further entrance of air by closing the end of the tube, the liquid air quickly becomes solid, showing in the interior a hollow spindle from contraction, in passing from the liquid into the solid form (Figure E, Plate III.). On bringing the tube containing the solid from the liquid hydrogen bath into the air we observe liquid air running from the surface while the solid air inside is seen to melt (Figure D, Plate III.). Here is a tube into which liquid oxygen has been poured. On placing it in liquid hydrogen it freezes to a clear blue ice. Liquid nitrogen under similar circumstances forms a colourless ice. If instead of an open tube in free air we employ a closed vessel of about a litre capacity to which the quill tube is attached, then, on repeating the experiment, the same results follow, only the volume of the liquid air formed agrees with the total quantity present in the vessel. This suggests that any air left in the closed vessel must have a very small pressure. This is confirmed by attaching a mercurial gauge to

any vessel containing air, when it will be seen the vacuum produced by hydrogen cooling is equal to that of a Torricellian vacuum (Plate II.). To reach such a high exhaustion the solid oxygen and nitrogen, at the boiling point of hydrogen must be practically non-volatile or have an exceedingly small vapour pressure. If the ordinary air contains free hydrogen, helium, &c. which are non-condensable in this way of working, then the vacuum would not be so high as with pure oxygen or nitrogen. This method may be used to separate the incondensable gases from the air. Such air vacua when examined spectroscopically show the lines of hydrogen, helium and neon. We may now employ this process to produce high vacua, and test their exhaustion by the character of the electric discharge. Vacuum tubes which have been prepared in this way show extraordinary resistance to the passage of the electric discharge; they also show the marked phosphorescence of the glass, characteristic of Crookes tubes (Figures F and G, Plate III.). It is, however, the rapidity with which such high exhaustions can be attained that is so interesting. You will observe that this large Geissler tube, previously exhausted to some three inches pressure, will, when the end part is immersed in liquid hydrogen, pass through all the well-known changes in the phases of striation: the glow on the poles; the phosphorescence of the glass; in the space of a fraction of a minute. From this it follows that theoretically we need not exhaust the air out of our double-walled vessel when liquid hydrogen has to be stored or collected. This makes a striking contrast to the behaviour of liquid air under similar circumstances. The rapid exhaustion caused by the solidification of the air on the surface of a double-walled unexhausted test-tube, when liquid hydrogen is placed in it, may be shown in another way. Leave a little mercury in the vessel containing air, just as if it had been left from making a mercurial vacuum. Now we know mercury, in such a vacuum, can easily be made to distil at the ordinary temperature when we cool a part of the vessel with liquid air, so that we should expect the mercury in the unexhausted test-tube to distil on to the surface cooled with the liquid hydrogen. This actually takes place. A rough comparison of the relative temperatures of boiling hydrogen and oxygen may be made by placing two, nearly identical, hydrogen gas thermometers operating at constant pressure side by side and cooling each with one of the liquids (Plate IV.). It will be seen that the contraction in the thermometer cooled with liquid hydrogen elevates the liquid some six times higher than that of the corresponding liquid column of the thermometer placed in the liquid oxygen. A constant volume hydrogen thermometer constructed as shown in Plate V. gave the boiling point of 21° absolute or $-252°$ C., and a similar helium thermometer gave the same result. The critical temperature is about 32° absolute or $-241°$ C., and the critical pressure about 15 atmospheres. If a closed vessel is full of hydrogen gas at atmospheric pressure, then, unlike the air vessels, it shows no condensation when a part of it is cooled in liquid hydrogen. To produce

liquefaction we must increase the pressure of the gas, or reduce the boiling point of the liquid hydrogen by exhaustion. Pure hydrogen liquefied in a closed vessel is perfectly clear, showing no trace of colour or any appearance of absorption bands in the position of the spectrum lines. Electric sparks passing in the liquid when examined with the spectroscope show the ordinary line spectrum without any reversals. The vapour of boiling hydrogen is about fifteen times denser than that of the ordinary gas, thus bringing it up to the density of air. The liquid hydrogen, at its boiling point, is about sixty times denser than the vapour coming off. In the case of oxygen the density of the liquid is 255 times that of the vapour at its boiling point.

If a piece of cotton wool in the form of a little ball is attached to a thread, placed in liquid hydrogen, and then brought into the magnetic field, it is found to be strongly magnetic. This is simply due to the condensation of solid and liquid air in the pores of the wool. This substance we know is magnetic on account of the oxygen it contains. Pure liquid hydrogen is not magnetic, but when the solid air snow is in suspension in the fluid, then the magnetic character of the latter becomes apparent when the vessel is placed in the magnetic field.

All the phosphorescent effects produced at low temperatures formerly described are intensified at the much lower temperature of boiling hydrogen. To stimulate phosphorescence at the temperature of liquid air, ultra-violet light had to be employed, and then the solid body, organic or inorganic, allowed to rise in temperature. It was during the rise of temperature that the marked luminous emission took place. Amongst inorganic bodies the platino-cyanide of ammonia is very remarkable in this respect, and generally the group in organic chemistry known as the ketonic bodies. In the case of bodies cooled in liquid hydrogen, it appears that some show phosphorescence by simple stimulation with the light coming from an ordinary carbon filament electric lamp. The light in this case coming through glass contains only, we may say, the visible spectra, so that the ultra-violet rays are not now essential. It is strange to find photographic action still relatively considerable. At the boiling point of liquid air the photographic intensity is reduced by 80 per cent. of the value at the ordinary temperature. The photographic effect on a sensitive film immersed in liquid hydrogen as compared with the same placed in liquid air is as one to two, so that 10 per cent. of the action at ordinary temperatures still remains. As every kind of chemical action so far examined is non-existent at this extreme temperature, these experiments suggest that the cause of the photographic action may be essentially physical. No better illustration could be given of the rapid diminution of chemical action at low temperatures than to remind you that fluorine gas, the most active elementary body, under such conditions, may be liquefied and kept in glass vessels.

The effect of a temperature of 21° absolute on the electric re-

Plate I.

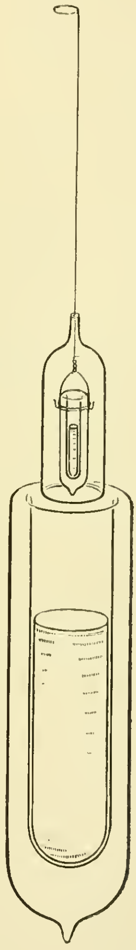


Plate II.

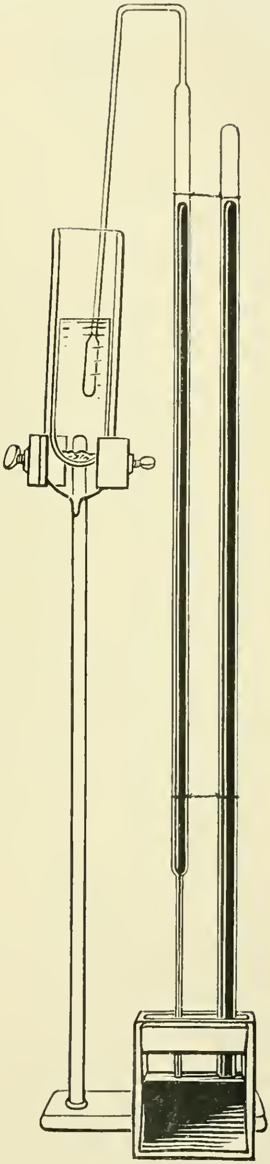


Plate III.

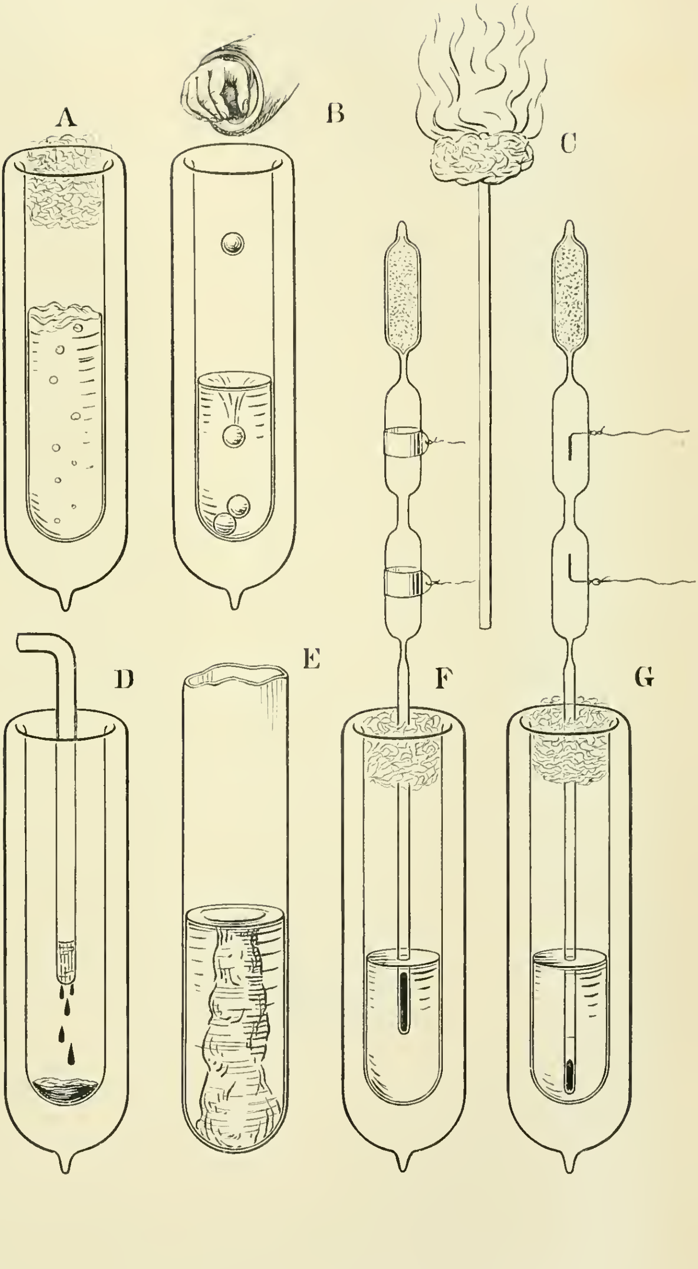


Plate IV.

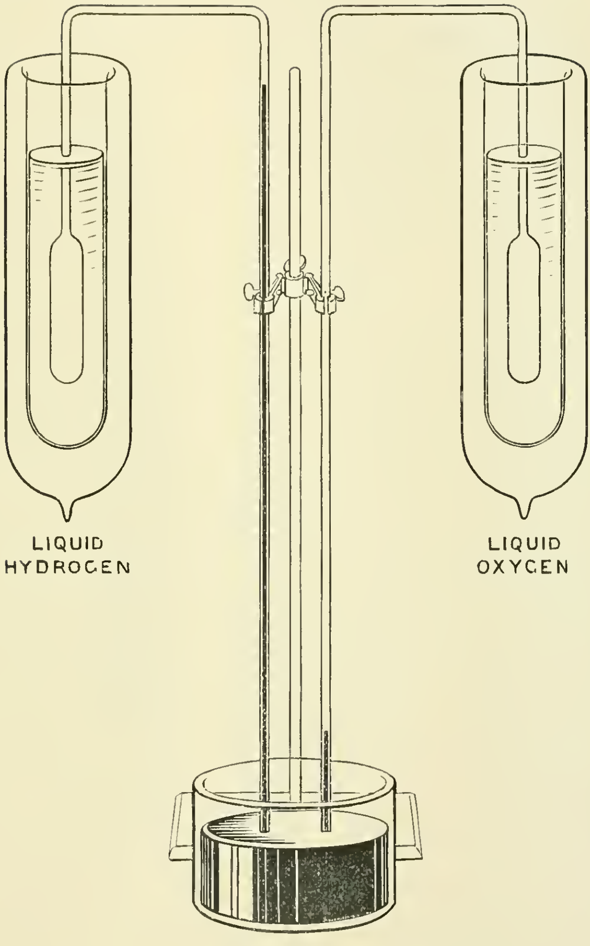
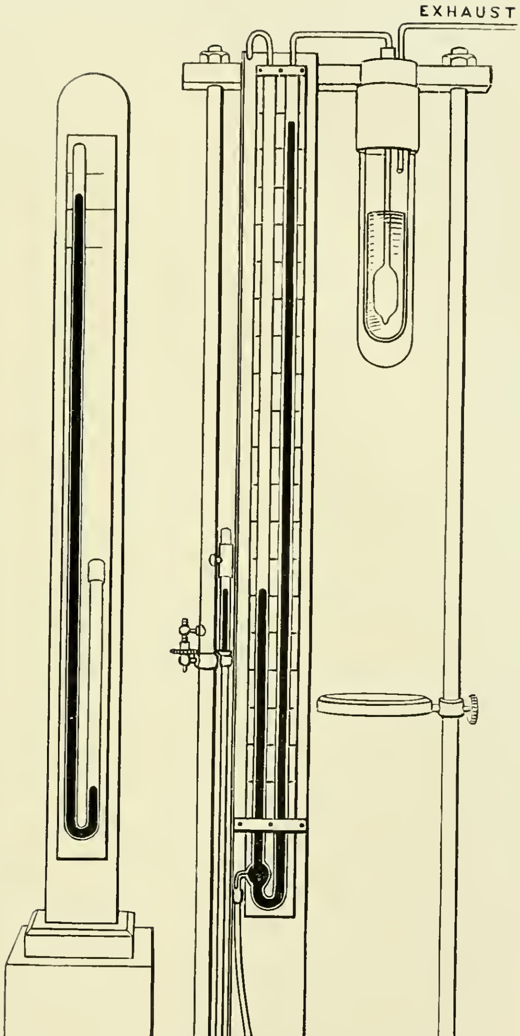


PLATE V.



sistance of the pure metals is a problem of great interest. In passing from the melting point of ice to the boiling point of hydrogen, pure platinum loses resistance till only $\frac{1}{40}$ remains, and in the case of electrolytic copper the remaining resistance is only $\frac{1}{57}$ of what it was at starting. Such results suggest the approach to the condition, of what may be called relatively perfect electric conductivity as the zero of absolute temperature is approached.

Liquid hydrogen is a non-conductor of electricity, and as regards being an insulator for currents of high potential, it is comparable to that of liquid air. The properties of the liquid we have witnessed in no way suggest the metallic character that chemists like Faraday, Dumas and Graham anticipated; and, for the future, hydrogen must be classed with the non-metallic elements.

The liquefaction of hydrogen has been the consequence of some ten years' devotion to low temperature research. To many it may seem that the results have been indeed costly in more ways than one. The scientific worker who prepares the way for future development in this sort of inquiry generally selects complicated methods, and is attracted or diverted into many by-paths of investigation. He may leave to his successors any credit that may be attached to cheapness and ease of production of the agent of research—results that must invariably follow. Liquid hydrogen is an agent of research which will enable us to examine into the properties of matter at the lowest-maintained temperature ever reached by man. Much work has still to be accomplished. One of the most fascinating problems of the study of low temperatures has been materially advanced. The interval separating us from the zero of absolute temperature has been reduced to practically one-fourth the value that it stood at when liquid air was the cooling agent. We can produce in pure Helium instantaneous temperatures bringing us still nearer the goal. Now we can maintain a temperature within less than 16° of this zero, and the investigator who will make the further attempt to reduce this distance by an equivalent amount, thereby reaching a steady temperature of 4° or 5° absolute, will indeed face a problem of almost insuperable difficulty. Well, let us take comfort in an aphorism of Davy's: "Fortunately for the active and progressive nature of the human mind, even experimental research is only a method of approximation to truth."

The success of the demonstration has been largely due to the unremitting exertions of my chief assistant, Mr. Robert Lennox, and to the valuable aid given by Mr. J. W. Heath.

Lord Kelvin, in moving a vote of thanks to Professor Dewar for his brilliant, beautiful, and splendidly interesting lecture, said that if those present wished to measure the importance of the occasion, let them think what Count Rumford, or Davy, or Faraday would have thought, could they have been present. They could not have hoped for their scientific dreams and prophecies to be so splendidly verified

within the century. The end of experiment in research at low temperatures had by no means been reached, and perhaps in a few years substances yet unknown and more refractory than hydrogen would have been found which would bring the experimenter to within five degrees of the absolute zero.

The vote was seconded by Sir George Stokes and carried by acclamation.

Professor Dewar, in reply, referred in appreciative terms to the part taken in the liquefaction of hydrogen by his assistant, Mr. Lennox. For himself, his chief function had been to get the wherewithal to carry on the experiments, and without the assistance he had received from numerous friends they would have been absolutely impossible.

Diplomas of honorary membership were next presented to Professor Cornu and Professor Newcomb.

Sir Frederick Bramwell proposed a vote of thanks to the Duke of Northumberland for the manner in which he had performed his duties as President of the Institution.

This was seconded by Dr. Mond and supported by Professor Barker, on behalf of the guests from beyond the sea, who, he said, had been royally entertained, listening to lectures such as the world had never before heard, and witnessing experiments such as it had never seen.

The proceedings ended with the reply of the Duke of Northumberland.

Professor and Mrs. Dewar gave a Reception after the lecture in their rooms to the guests of the Institution.

Thursday, June 8, 1899.

By invitation of the teachers of Natural Science in the University of Oxford, the Guests of the Institution visited the University and had Luncheon in Christ Church Hall. Upon five of the guests the honorary degree of D.C.L., was conferred.

GENERAL MONTHLY MEETING,

Monday, July 3, 1899.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

The Lord Kinnaird,
A. F. Lindemann, Esq.
Hon. W. J. Ward,

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Sir Henry Thompson, Bart. for his donation of £25, and to Mr. Henry Vaughan for his donation of £20, to the Fund for the Promotion of Experimental Research at Low Temperatures.

The cordial thanks of the Members were returned to the Master and Wardens of the Merchant Taylors' Company; to the Lord Mayor and Lady Mayoress; to Dr. and Mrs. Mond; to Dr. and Mrs. Dewar; to Professor William Odling and to the Teachers of Natural Science at Oxford, for their hospitality to the Members and guests of the Royal Institution during the recent Centenary Celebrations. The Managers reported that they had received gratifying assurances from their guests that the Centenary Celebrations, as a whole, were highly appreciated and considered not unworthy of the past history of the Royal Institution, and of good augury for that new century of scientific work to which it has now to apply itself. Thus Professor Cornu, on his return to Paris, reported to the French Academy of Sciences as follows:—

"La Royal Institution of Great Britain, fondée en 1799 par Benjamin Thompson, Comte de Rumford, fêtait les 5, 6 and 7 juin, le Centenaire de sa fondation; S. A. R. le Prince de Galles, Vice-Patron de l'Institution, a gracieusement demandé qu'on lui présentât nos confrères et leur a remis, dans l'une des séances commémoratives, le diplôme de membre honoraire de l'Institution Royale. Lord Rayleigh et M. James Dewar ont rappelé, dans deux remarquables Commemoration Lectures, les principales découvertes faites dans les laboratoires de l'Institution Royale, par Thomas Young, Sir Humphry Davy, Michael Faraday, John Tyndall.

"Les expériences les plus intéressantes ont été exécutées; en particulier, celles qui se rapportent à l'interférence des sons et à l'hydrogène liquide ont excité un véritable enthousiasme. Nous avons pu mesurer ainsi l'immense chemin parcouru depuis un siècle, grâce aux efforts déployés dans cette belle Institution.

"Enfin, l'Université d'Oxford a convié tous les savants étrangers présents à Londres à visiter ses collèges, plus de cinq fois séculaires, qui renferment des richesses d'une valeur inestimable.

"Les deux Universités de Cambridge et d'Oxford ont témoigné à nos confrères leur sentiments d'estime et confraternité scientifiques en leur conférant des titres de docteur honoraire.

"Nous rapportons donc de notre séjour parmi les savants anglais non seulement l'impression de la plus cordiale hospitalité, mais encore une véritable admiration pour la manière dont ils cultivent et honorent la science. L'histoire de ces Universités, et particulièrement celle de l'Institution Royale de la Grande-Bretagne, offre un example bien instructif: on voit par quelle méthode une nation, jalouse de s'élever au premier rang du progrès scientifique, d'encourager les recherches élevées et d'en faire comprendre les applications, parvient au but qu'elle s'est proposé.

"Elle choisit à chaque époque les savants les plus illustres, leur donne à la fois l'indépendance et les moyens matériels sans lesquels aujourd'hui on ne saurait réaliser de grandes découvertes.

"Il y a là un sujet de méditations et d'études pour ceux qui ont l'honneur de diriger le mouvement scientifique et qui s'efforcent de maintenir notre pays au rang élevé que ses traditions lui imposent."

The Managers also reported that the Centenary Celebrations had not affected the pecuniary resources of the Royal Institution, as all expenses in connection therewith had been defrayed by private contributions. A balance remaining over of the sums contributed, amounting to £87, has been paid over to the Fund for the Promotion of Experimental Research.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—The Moghul Architecture of Fathpur-Sikri. By E. W. Smith. 4to. 1898.

The Lords of the Admiralty—Report of the Astronomer Royal to Board of Visitors, 1899. 4to.

The Meteorological Office—Hourly Means for 1895. 4to. 1899.

The British Museum Trustees—Antiquities from the City of Benin. By C. H. Read. fol. 1899.
Facsimiles from Royal, etc. Autographs. Edited by G. F. Warner. fol. 1898.
Catalogue of Bronzes. By H. B. Walters. 8vo. 1899.
Catalogue of Greek Coins. By W. Wroth. 8vo. 1899.

Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. 1_o Semestre, Vol. VIII. Fasc. 10. 8vo. 1899.

American Academy of Arts and Sciences—Proceedings, Vol. XXXIV. Nos. 15-17. 8vo. 1899.

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Bankers, Institute of—Journal, Vol. XX. Part 6. 8vo. 1899.

Boston Society of Medical Sciences—Journal, April and May, 1899. 8vo.

British Astronomical Association—Journal, Vol. IX. No. 7. 8vo. 1899.

Brough, B. H. Esq. (the Author)—The Jubilee of the Austrian Society of Engineers. 8vo. 1899.

Brymner, Douglas, Esq. (the Archivist)—Report on Canadian Archives, 1898. 8vo. 1899.

Cambridge Philosophical Society—Proceedings, Vol. X. Part 2. 8vo. 1899.
Transactions, Vol. XVII. Parts 3, 4. 8vo. 1899.

Camera Club—Journal for June, 1899. 8vo.

Canada, Meteorological Service—Report of the Meteorological Service of Canada for 1896. 2 vols. 4to. 1898.

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Chemical Society—Journal for June, 1899. 8vo.
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Cracovie, l'Académie des Sciences—Bulletin International, 1899, Nos. 4, 5. 8vo.
Editors—American Journal of Science for June, 1899. 8vo.
Analyst for June, 1899. 8vo.
Anthony's Photographic Bulletin for June, 1899. 8vo.
Astrophysical Journal for May, 1899.
Athenæum for June, 1899. 4to.
Author for June, 1899. 8vo.
Bimetallist for June, 1899. 8vo.
Brewers' Journal for June, 1899. 8vo.
Chemical News for June, 1899. 4to.
Chemist and Druggist for June, 1899. 8vo.
Education for June, 1899.
Electrical Engineer for June, 1899. fol.
Electrical Engineering for June, 1899. 8vo.
Electrical Review for June, 1899. 8vo.
Electricity for June, 1899. 8vo.
Engineer for June, 1899. fol.
Engineering for June, 1899. fol.
Homœopathic Review for June, 1899. 8vo.
Horological Journal for June, 1899. 8vo.
Industries and Iron for June, 1899. fol.
Invention for June, 1899.
Journal of State Medicine for June, 1899. 8vo.
Law Journal for June, 1899. 8vo.
Lightning for June, 1899. 8vo.
London Technical Education Gazette for June, 1899.
Machinery Market for June, 1899. 8vo.
Modern Machinery for June, 1899. 8vo.
Motor Car Journal for June, 1899. 8vo.
Nature for June, 1899. 4to.
New Church Magazine for June, 1899. 8vo.
Nuovo Cimento for May, 1899. 8vo.
Photographic News for June, 1899. 8vo.
Physical Review for May, 1899. 8vo.
Popular Science Monthly for June, 1899. 8vo.
Public Health Engineer for June, 1899. 8vo.
Science Abstracts, Vol. II. Part 6. 8vo. 1899.
Science Siftings for June, 1899.
Terrestrial Magnetism for June, 1899. 8vo.
Travel for June, 1899. 8vo.
Tropical Agriculturist for June, 1899.
Zoophilist for June, 1899. 4to.
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Florence, Biblioteca Nazionale Centrale—Bollettino, Nos. 322–324. 8vo. 1899.
Franklin Institute—Journal for June, 1899. 8vo.
Geographical Society, Royal—Geographical Journal for June, 1899. 8vo.
Hudleston, W. H. Esq. F.R.S. M.R.I. (the Author)—The Eastern Margin of the North Atlantic. 8vo. 1899.
Imperial Institute—Imperial Institute Journal for June, 1899.
Johns Hopkins University—American Chemical Journal for June, 1899. 8vo.
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Proceedings, No. 415. 8vo. 1899.
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Mathematisch-Physische Classe—
Berichte, 1899, No. 3. 8vo.
Selborne Society—Nature Notes for June, 1899. 8vo.
Sutherland, D. A. Esq. (the Author)—The Petroleum Industry of Roumania. 8vo. 1899.
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Tacchini, Prof. P. Hon. Mem. R.I. (the Author)—Memorie della Società degli Spettroscopisti Italiani, Vol. XXVIII. Disp. 4. 4to. 1899.
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Farmers' Bulletin, No. 93. 8vo. 1899.
United States Geological Survey—Eighteenth Annual Report, 1896–97, Parts 1–5. 4to. 1897–98.
United States Patent Office—Official Gazette, Vol. LXXXVII. Nos. 9–12. 8vo. 1899.
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Whitty, Rev. J. I. (the Author)—Palestine Exploration. 8vo. 1899.
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Zurich, Naturforschende Gesellschaft—Vierteljahrsschrift, Jahrg. XLIV. Heft 1, 2. 8vo. 1899.

GENERAL MONTHLY MEETING,

Monday, November 6, 1899.

His Grace The Duke of Northumberland, K.G. President, in the Chair.

H.H. The Thakore Sahib of Gondal, D.C.L. LL.D.
Geoffrey Foster Barrett, Esq.
John B. Broun-Morison, Esq., F.S.A.
A. Henry Savage Landor, Esq.
Thomas Cunningham Porter, Esq., M.A.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to "A Lady Member" for her donation of £100, and to Mr. George Matthey, F.R.S. for his donation of £100 to the Fund for the Promotion of Experimental Research at Low Temperatures.

The Special Thanks of the Members were returned to Miss Elinor Busk and Miss Frances Busk, for a Portrait of Mr. George Busk, F.R.S., Treasurer of the Royal Institution from 1873 to 1886.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Lords of the Admiralty—Report of Cape Observatory for 1898. fol. 1899.
Independent Day Numbers for 1901 at Cape Observatory. 8vo. 1898.
The Governor-General of India—General Report on Work of Geological Survey of India, 1898–99. 8vo. 1899.
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Mathematisch-Physische Classe—
Berichte, 1899, No. 4. 8vo.
Abhandlungen, Band XXV. No. 3. 8vo. 1899.
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Berichte, 1899, Nos. 1–3. 8vo.
Abhandlungen, Band XVIII. No. 5. 8vo. 1899.
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St. Pétersbourg, L'Académie Impériale des Sciences—Bulletin, Tome VIII. No. 5; Tome IX. Nos. 1–5; Tome X. Nos. 1–4. 8vo. 1898–99.
Mémoires, Tome VII. No. 4; Tome VIII. Nos. 1–5. 4to. 1898–99.
Statistical Society, Royal—Journal, Vol. LXII. Parts 2, 3. 8vo. 1899.
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Bihang, Vol. XXIV. 8vo. 1899.
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Teyler Museum, Haarlem—Archives, Série II. Vol. VI. Part 3. 8vo. 1899.
Tommasina, T. Esq. (the Author)—Sur un curieux phénomène d'adhérence des limailles métalliques sous l'action du courant électrique. 4to. 1899.
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United States Army, Surgeon-General's Office—Index Catalogue, Second Series, Vol. IV. 4to. 1899.
United States Department of Agriculture—Experiment Station Record, Vol. X. No. 11. 8vo. 1899.
Monthly Weather Review for April–July, 1899. 4to.
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United States Geological Survey—Atlas to Monograph No. XXXI. fol. 1898.
Tables of the Mineral Products of the United States, 1888–1897. fol 1898.
Nineteenth Annual Report, Parts 1, 4, 6. 4to. 1898–99.
Monographs, Nos. XXIX. XXXI. XXXV. 4to. 1889.

United States Patent Office—Official Gazette, Vol. LXXXVII. No. 13; Vol. LXXXVIII.; Vol. LXXXIX. Nos. 1–4. 8vo. 1898–99.

University of London—Calendar, 1899–1900. 8vo. 1899.

Upsal, Royal Society of Sciences—Nova Acta, Third Series, Vol. XVIII. Fasc. 1. 4to. 1899.

Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1899, Heft 5–7. 8vo.

Victoria Institute—Journal, Nos. 121–124. 8vo. 1899.

Victoria, The Agent-General for—Victoria, Its Mines and Minerals. fol. 1899.

Vienna, Geological Institute, Imperial—Jahrbuch, Band XLIX. Heft 1. 8vo. 1899.

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Zoological Society of London—Proceedings, 1899, Parts 2, 3. 8vo.
Transactions, Vol. XV. Parts 2, 3. 4to. 1899.

GENERAL MONTHLY MEETING,

Monday, December 4th, 1899.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Professor Henry E. Armstrong, Ph.D. LL.D. F.R.S.
John Herbert Bowman, Esq.
John Storrs Brookfield, Esq. B.A. M.D.
John B. Carrington, Esq.
W. Brodrick Cloete, Esq.
Lionel Leigh Smith, Esq. M.A.

were elected Members of the Royal Institution.

The following letter from the Clerk of the Goldsmiths' Company was read:—

"Goldsmiths' Hall, London, E.C.
November 16*th*, 1899.

"Dear Sir,—I am directed to inform you that the attention of the Court of the Goldsmiths' Company having been drawn to the fact that the Royal Institution of Great Britain has lately celebrated its Centenary, they have, in order to mark their sense of the importance of that event, been pleased to make to the Institution the further grant of £1000, for the continuation and development of original research, and especially for the prosecution of further investigations of the properties of matter at temperatures approaching that of the absolute zero of temperature.

"I enclose a cheque for this amount, and I shall feel obliged to you to acknowledge the receipt.

I am, dear Sir,
Your obedient Servant,
(Signed) Walter S. Prideaux.

The Hon. Secretary,
The Royal Institution of Great Britain."

The following Resolution was then passed:—

"That the Members of the Royal Institution of Great Britain, in General Meeting assembled, having been informed that the Court of the Goldsmiths' Company have made a donation of £1000 to the Funds of the Royal Institution in commemoration of its Centenary, and in aid of the investigations which are being carried on in its Laboratories into the properties of matter at low temperatures, desire to express to the Court their profound and grateful appreciation of this second munificent manifestation of their practical interest in the work of the Institution—a manifestation which has been made on this occasion at once reminiscent of past services to science and prescient of services yet to come."

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Annual Progress Report of the Archæological Survey Circle, N.W.P. and Oudh, up to June 30, 1899. fol.

The Meteorological Office—Meteorological Charts of the Southern Ocean between the Cape of Good Hope and New Zealand. fol. 1899.
Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. 2° Semestre, Vol. VIII. Fasc. 8, 9. 8vo. 1899.
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Annuaire, 1898-99. 8vo.
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Dallmeyer, Thomas R. Esq. F.R.A.S. M.R.I. (the Author)—Telephotography. 8vo. 1899.
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Chemist and Druggist for Nov. 1899. 8vo.
Education for Nov. 1899.
Electrical Engineer for Nov. 1899. fol.
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Electrical Review for Nov. 1899. 8vo.
Electricity for Nov. 1899. 8vo.
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Lightning for Nov. 1899. 8vo.
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Royal Institution of Great Britain.

WEEKLY EVENING MEETING.

Friday, January 19, 1900.

His Grace The Duke of Northumberland, K.G., President, in the Chair.

The Rt. Hon. Lord Rayleigh, M.A. D.C.L. LL.D. F.R.S. *M.R.I.*
PROFESSOR OF NATURAL PHILOSOPHY R.I.

Flight.

Lord Rayleigh first considered the question what people generally meant when they spoke of a flying machine, and concluded that size had a great deal to do with their conception, which was usually of a machine big enough to carry a man by whom it could be controlled: otherwise the flying machine had been invented long since by Penaud. The main problem of the flying machine was the problem of the aeroplane. What were the forces that acted on a plane exposed to the wind? This was also the vital problem of kites, of which he mentioned some of the practical applications by Franklin, Archibald, Baden-Powell, and others; but kites were always anchored to the ground, and as soon as we cast ourselves adrift from the ground the problem became essentially different, for it was then necessary to consider how maintenance in the air could be managed. Now some birds seemed to maintain themselves in the air with little effort. What was the nature of the "soaring" or "sailing flight" by which a big bird maintained himself with but little flapping of the wings? There had been much discussion about this point, often foolish because of misunderstandings between the disputants. However, the science of mechanics enabled it to be laid down with certainty that a bird could no more maintain himself without motion of the wings in a uniform wind moving horizontally than in air at perfect rest. It was entirely a question of relative motion. If, then, a bird was seen to be maintaining himself without flapping, it was certain the air was not moving horizontally and uniformly. But there might be rising currents of air upon which he was supported, and these were much more common than was often supposed. In other cases where it was difficult to imagine the existence of such currents, an explanation might be sought in the non-uniformity of the wind. For example, it was mechanically possible for a bird just at the point of transition between two different *strata* of wind to maintain its position by taking advantage of the different velocities. The albatross, he believed, did something of this sort. Langley, again, had pointed out how the bird could turn to account the internal work of the wind by taking advantage of its gustiness. Leaving this subject, the lecturer discussed the general question of the action of the wind on an aeroplane. He first

showed one or two experiments illustrating the curious effects that might be obtained from a plane exposed obliquely to wind. In one of these it was seen that a light piece of sheet brass, evenly pivoted in, and nearly filling up, an aperture through which air was issuing under pressure, tended to set itself square to the aperture so as to block it as much as possible, but, if started, it continued to rotate in either direction, emitting a roaring sound. This phenomenon had never been properly explained, nor had the somewhat analogous action of a piece of card, which, when dropped, reached the ground with a rotatory motion. As to the pressure of the wind on a horizontal plane surface, if the latter was falling vertically at the rate, say, of four miles an hour, and also moving horizontally at, say, 20 miles an hour, did the horizontal motion make a difference to the pressure that existed at its under surface? It might be argued that it did not; but the argument was fallacious, and the truth was that the horizontal motion much increased the pressure under a vertically falling plane, a fact on which depended the possibility of flight, natural and artificial. Lord Rayleigh showed how this point might be illustrated, and even investigated, by means of a simple variation of the ordinary windmill. This was a light wheel having six vanes, each of which could be set at any desired angle, and it was used by setting four at a particular angle, and finding at what angle the other two must be placed so as to compensate the rotation of the wheel produced by the former when it was moved quickly through the air.* He next observed that not only was there pressure underneath a bird's wing or an aeroplane, but that the suction above was not an unimportant matter; and he performed an experiment to show the reality of this suction, about which he said there had been some scepticism. Turning to flight on a large scale, he remarked that it was a natural question to ask, Was it possible for a man to raise himself from the ground by working a screw with his own muscular power only? The investigation was not difficult, and the answer was that it was quite impracticable for him to do so. Artificial flight was a question of the speed of the horizontal motion. A bird did not use a revolving mechanism like a screw to propel itself, but he had no doubt that a revolving mechanism was the most suitable for artificial flying-machines. Whether the difficulties of these would be surmounted he did not know, but he was disposed to agree with Mr. Maxim that it was mainly a question of time and much money. Still, he did not think flight would ever be a safe mode of conveyance for those who were desirous of going out for a day's shopping, for it was hard to see how alighting on the ground could ever be rendered quite free from danger. But, as Mr. Maxim once remarked, the first use of flying-machines would be for military purposes, and they had not yet succeeded in making war quite safe.

* This apparatus was more fully described in the Wilde Lecture (Manchester Memoirs, vol. xliv., Part 4, pp. 1–26), where also some other matters here referred to are treated in greater detail.

WEEKLY EVENING MEETING,

Friday, January 26, 1900.

Sir Frederick Bramwell, Bart., D.C.L. LL.D. F.R.S., Honorary Secretary and Vice-President, in the Chair.

The Hon. Charles A. Parsons, M.A. F.R.S. M. Inst. C.E.

Motive Power—High-Speed Navigation Steam Turbines.

Twenty centuries ago the political power of Greece was broken, although Grecian civilisation had risen to its zenith. Rome was growing continually stronger, and was rapidly gaining territory by absorbing weaker states. Egypt, older in civilisation than either Greece or Rome, fell, but two centuries later, before the assault of the younger states, and became a Roman province. Her principal city at this time was Alexandria, a great and prosperous city, the centre of the commerce of the world, the home of students and of learned men, its population the wealthiest and most civilised of the then known world.

It is among the relics of that ancient Egyptian civilisation that we find the first records of the early history of the steam engine. In Alexandria, the home of Euclid, and possibly contemporary with Archimedes, Hero wrote his 'Spiritalia seu Pneumatica.' It is doubtful if Hero was the inventor of the contrivances and apparatus described in his work; it is more probable that they were devices generally known at the time. Nothing in the text, however, indicates to whom the several machines are to be ascribed. Two of these machines are of special interest. The first utilised the expansive force of air in a closed vessel heated externally, the pneumatic force being applied upon the surface of water in other vessels, and the hydraulic force utilised for opening the doors of a Grecian temple and working other pseudo-magic contrivances.

Then after describing several forms of cylindrical boilers, and the use of the steam jet for accelerating combustion, he comes to the first of a type of steam engine, the steam turbine, which is the subject of our discourse this evening.

This is a veritable steam engine. The cauldron contains water, and is covered by a steam-tight cover, a globe is supported above the cauldron by a pair of tubes, one carrying a pivot, and the other opening directly through the trunnion joint into the sphere; short bent pipes are attached to diametrically opposite points on the equator. The steam generated in the cauldron passes up into the sphere and

issues tangentially from the bent pipes, and by the reaction causes the sphere to rotate.

It seems uncertain whether this machine was ever more than a toy, or whether it was used by the Greek priests for producing motion of apparatus in their temple; but from our experience within the last twenty years it appears that, with some improvements in design and construction, it could have been applied to perform useful work at the date of Hero, and further that, when so improved, it might have claimed a place among economical steam engines, even up to the middle of the present century.

A few years ago I had an engine constructed to test the capabilities of this class of reaction steam turbine, the only difference between this engine and Hero's being that the sphere was abolished, as a useless incumbrance, the arms were made of thin steel tube of oval form, so as to offer the least resistance to their motion, and the whole was enclosed in a cast-iron case which was connected to a condenser. When supplied with steam at a pressure of 100 lbs. per square inch, and a vacuum in the case of 27″ of mercury, a speed of 5000 revolutions per minute was attained, and an effective power was realised of 20 horse, and the consumption of steam was only 40 lbs. per brake horse-power. By this very creditable performance, I was encouraged to further test the system, and constructed a compound reaction engine, in which the steam was caused to pass successively through three pairs of arms on one hollow shaft, each pair being contained in a separate compartment through which the shaft passed, suitable metallic packing preventing the passage of steam from one compartment to the next. The performance of this engine was, however, not superior to that of the single two-arm Hero's engine, for the simple reason that the excessive resistance to motion of the arms in the denser steam of the compartments more than neutralised the gain from the compound form. The performance of this engine was, however, sufficiently good to have it placed on a par with many ordinary steam engines in the middle of the present century.

The great barrier to the introduction of Hero's engine was undoubtedly the excessive speed of revolution necessary to obtain economical results, and with the crude state of mechanical engineering at that time, it would have been a matter of some difficulty to construct the turbine engine with sufficient accuracy of workmanship for satisfactory results, to say nothing of the necessary gearing for applying the power to ordinary useful purposes.

The next steam engine mentioned in history, which is capable of practical and useful development, is Bianca's in 1629. It is of the simplest form, a jet of steam from a steam boiler impinges on a paddle-wheel and blows it round. This form of engine has since 1889 been developed by Dr. De Laval, of Stockholm, with great ingenuity, and is extensively used for moderate powers on the Continent. The speed is, however, necessarily very high in order to obtain economy in steam, and spiral reduction gearing is used in

order that the speed of revolution may be reduced for the application of the power. The improvements that have been made in Bianca's steam turbine by De Laval are firstly, the ordinary steam jet is replaced by a diverging conical jet, which permits of the expansion of the steam before it emerges from the jet, and so transforming the potential energy of the high-pressure steam into kinetic energy of velocity in the direction of flow.

Secondly, the crude paddle-wheel of Bianca is replaced by a wheel of the strongest steel, fringed round the periphery with little cupped blades of steel, somewhat analogous to the buckets of a Pelton water-wheel.

Lastly, the steel wheel is mounted on a long and somewhat elastic shaft, to allow of its easy and free motion, and on one extremity of this shaft is mounted the pinion of the spiral reduction gear.

The speeds of revolution of the steam-wheels of De Laval's turbine are from 10,000 to 30,000 revolutions per minute, according to the size, involving peripheral speeds up to 1200 feet per second, or about one-half the speed of the projectile from a modern cannon. Such speeds are necessary to obtain power economically from the high-pressure steam jet, issuing from 3000 to 5000 feet per second as calculated by Rankine.

It is somewhat remarkable that not till a century after Bianca, the piston or ordinary reciprocating engine made its first appearance, in about the year 1705, and has since become one of the chief factors in the great mechanical and engineering growths of the last century. During this period the steam turbine seems to have been, practically speaking, neglected, which is somewhat remarkable in view of the numerous attempts of inventors to construct a rotary engine, attempts which had no practical results.

In the year 1884, the advent of the dynamo-electric machine, and the development of mechanical and electrical engineering, created an increased demand for a good high-speed engine. Engineers were becoming more accustomed to high speeds of revolution, for the speed of dynamos was at this time from 1000 to 2000 revolutions per minute, of centrifugal pumps from 300 to 1500, and wood-working machinery from 3000 to 5000; and Sir Charles Wheatstone had made a tiny mirror revolve at a speed of 50,000 revolutions per minute for apparatus for measuring the velocity of light. The problem then presented itself of constructing a steam turbine, or ideal rotary engine, capable of working with good economy of steam at a moderate speed of revolution, and suitable for driving dynamos without the intervention of reduction gearing. To facilitate the problem, the dynamo was also considered with the view of raising its speed of revolution to the level of the lowest permissible speed of the turbine engine. In other words, to secure a successful combination, the turbine had to be run as slowly as possible, and the dynamo speed had to be raised as much as possible, and up to the same speed as the turbine, to permit direct coupling.

In 1884 preliminary experiments were commenced at Gateshead-on-Tyne, with the view of ascertaining by actual trial, the conditions of working equilibrium and steady motion of shafts and bearings at the very high speeds of rotation that appeared to be essential to the construction of an economical steam turbine of moderate size. Trial shafts were run in bearings of different descriptions up to speeds of 40,000 revolutions per minute; these shafts were $1\frac{1}{2}$ inches in diameter and 2 feet long, the bearings being about $\frac{3}{8}$ inch in diameter. No difficulty was experienced in attaining this immense speed, provided that the bearings were designed to have a certain small amount of "give" or elasticity; and after the trial of many devices to secure these conditions, it was found that elasticity, combined with frictional resistance to transverse motion of the bearing bush, gave the best results, and tended to damp out vibrations in the revolving spindle. This result was achieved by a simple arrangement; the bearing in which the shaft revolved was a plain gun-metal bush with a collar at one end and a nut at the other; on this bush were threaded thin washers, each being alternately larger and smaller than its neighbour, the small series fitting the bush and the larger series fitting the hole in the bearing block, these washers occupying the greater part of the length of the bush. Lastly, a wide washer fitted both the bush and block, forming a fulcrum on which the bush rested; while a spiral spring between the washers and the nut on the bush pressed all the washers tightly against their neighbours. It will be seen now that, should the rotating shaft be slightly out of truth (which it is impossible to avoid in practice), the effect is to cause a slight lateral displacement of the bearing bush, which is resisted by the mutual sliding friction of each washer against its neighbour. The shaft itself being slightly elastic, tends to centre itself upon the fulcrum washer before mentioned, under the gyrostatic forces brought into play by the rapid revolutions of the shaft and influenced by the frictional resistance of the washers, and so the shaft tends to assume a steady state of revolution about its principal axis, or the axis of the mass, without wabbling or vibration. This form of bearing was exclusively used for some years in turbine engines aggregating some thousands of horse-power, but it has since been replaced by a simpler form fulfilling the same functions. In this later form the gun-metal bush is surrounded by several concentric tubes fitting easily within each other with a very slight lateral play; in the interstices between the tubes the oil enters, and its great viscosity when spread into thin films has the result of producing great frictional resistance to a rapid lateral displacement of the bearing bush; the oil film has also a centring action, and tends under vibration to assume a uniformity of thickness around the axis, thus centring the shaft, and like a cushion damping out vibrations arising from errors of balance. This form of bearing has been found to be very durable and quite satisfactory under all conditions.

Having tested the bearings up to speeds above those contemplated

in the steam turbine, the next problem was the turbine itself. The laws regulating the flow of steam being well known (which was not the case in Hero's time), various forms of steam turbine were considered, and it appeared desirable to adopt in principle some type that had been both successful in the water turbine, and also easily adapted to a multiple or compound formation, a construction in which the steam should pass successively through a series of turbines one after the other.

The three best known of water turbines are the outward flow, the inward flow, and the parallel flow, and of these the latter appeared to be the best adapted for the multiple or compound steam turbine, for reasons which will afterwards appear.

The object in view being to obtain a good coefficient of efficiency from the steam with a moderate speed of revolution and diameter of turbine wheel, it becomes essential that the steam shall be caused to pass through a large number of successive turbines, with a small difference of pressure urging it through each individual turbine of the set, so that the velocity of flow of the steam may have the proper relation to the peripheral velocity of the turbine blades to secure the highest degree of efficiency from the steam, conditions analogous to those necessary for high efficiency in water turbines. A large diameter of turbine wheel, it is true, would secure a moderate speed of revolution, but this may be dismissed at once for the simple reason that the frictional resistance of such a disc revolving at the immense peripheral velocity, in the exhaust steam, would make it a most inefficient engine.

In the year 1884, a compound steam turbine engine of 10 horse-power and a modified high-speed dynamo were designed and built for a working speed of 18,000 revolutions per minute. This machine proved to be practically successful, and subsequently ran for some years doing useful work, and is now in the South Kensington Museum.

This turbine engine consisted of two groups of fifteen successive turbine wheels, or rows of blades, on one drum or shaft within a concentric case on the right and left of the steam inlet, the moving blades or vanes being in circumferential rows projecting outwardly from the shaft, and nearly touching the case, and the fixed or guide blades being similarly formed and projecting inwardly from the case and nearly touching the shaft. A series of turbine wheels on one shaft were thus constituted, each one complete in itself, like a parallel flow water turbine, but unlike a water turbine, the steam after performing its work in each turbine passed on to the next, preserving its longitudinal velocity without shock, gradually falling in pressure on passing through each row of blades and gradually expanding. Each successive row of blades was slightly larger in passage-way than the preceding, to allow for the increasing bulk of the elastic steam, and thus its velocity of flow was regulated so as to operate with the greatest degree of efficiency on each turbine of the series (Fig. 1).

All end pressure from the steam was balanced by the two equal series on each side of the inlet, and the revolving shaft lay on its bearings revolving freely without any impressed force except a steady torque urging rotation, the aggregate of the multitude of minute forces of the steam on each blade. It constituted an ideal rotary engine; but it had faults. The comparatively high speed of rotation that was necessary for so small a size of engine as this first example, made it difficult to prevent, even with the bearings described, a certain spring or whipping of the massive steel shaft, so that considerable clearances were found necessary, and leakage and loss of efficiency resulted. It was, however, perceived that all these defects would decrease as the size of the engine was increased, with a corresponding reduction of rotational velocity, and consequently efforts were made towards the construction of engines of larger size, which

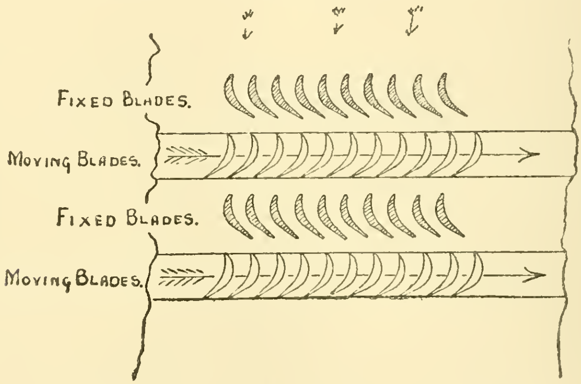

Fig. 1.—Fixed and Moving Blades of Turbine.

resulted, in 1888, in several turbo-alternators of 120 horse-power being supplied for the generation of current in electric lighting stations, and at this period the total horse-power of turbines at work reached in the aggregate about 4000, all of which were of the parallel flow type and non-condensing.

In 1889, in consequence of partnership difficulties and the temporary loss of patents, the radial flow type of turbines was reluctantly adopted. This type of turbine consists of a series of fixed discs with interlocking flanges at the periphery, forming, when placed together coaxially, a cylindrical case, with inwardly projecting annular discs. On the shaft are keyed a similar set of discs, the faces of the fixed and moving disc lie a short distance apart. From the faces of the fixed disc project the rows of guide-blades which nearly touch the moving disc, and from the moving disc project the rows of moving blades which nearly touch the fixed disc.

The steam is admitted into the case between the balance piston on the left and the first fixed disc, and passes outwards through the rows of fixed and moving blades between the first fixed and moving discs; then inwards towards the shaft at the back of the first moving disc, then again outwards between the second fixed and moving discs, and so on to the exhaust; the action being the same as in the parallel flow type.

In 1892, this type was the first to be adapted to work in conjunction with a condenser. The first condensing turbine of the radial flow type was of 200 horse-power, and at a speed of 4800 revolutions per minute, drove an alternator of 150 kilowatts output. It was tested by Professor Ewing, and the general result of the trials was to demonstrate that the condensing steam turbine was an exceptionally economical heat engine. With a steam pressure of 100 lbs., the steam being moderately superheated, and a vacuum of 28 inches of mercury, the consumption was 27 lbs. per kilowatt hour, which is equivalent to about 16 lbs. of steam per indicated horse-power. This result marked an era in the development of the steam turbine, and opened for it a wide field, including some of the chief applications of motive power from steam. At this period turbine alternators of the condensing type were placed in the Newcastle, Cambridge and Scarborough Electric Supply Companies' Stations, and soon afterwards several of 600 horse-power of the non-condensing parallel flow type were set to work in the Metropolitan Companies' Stations, where the comparative absence of vibration was an important factor. Turbine alternators and turbine dynamos of 2500 horse-power are now in course of construction in England and the United States, and larger sizes are in prospect.

A turbo-alternator manufactured at Heaton Works, Newcastle-on-Tyne, for the Corporation of Elberfeld in Germany, was tested a few days ago by a committee of experts from Germany, Professor Ewing being also present, with the following remarkable results. At the full load of 1200 kilowatts, and with a steam pressure of 130 lbs. at the engine, and 10° C. of superheat, the engine driving its own air pumps, the consumption of steam was found to be at the rate of 18·8 lbs. per kilowatt hour. To compare this figure with those obtained with ordinary piston engines of the highest recorded efficiencies, and assuming the highest record with which I am acquainted of the ratio of electrical output to the power indicated in the steam engine, namely 85 per cent., the figure of 18·8 lbs. per kilowatt in the turbine plant is equivalent to a consumption of 11·9 lbs. per indicated horse-power, a result surpassing the records of the best steam engines in the production of electricity from steam.

Turbine engines are also used for generating electrical current for the transmission of power, the working of electrical tramways, electrical pumping and coaling, and similar purposes. They are also used for coupling directly to and driving fans for producing forced and induced draught for general ventilating purposes, also for driving

centrifugal pumps for lifts up to 200 feet, and screw pumps for low lifts.

The most important field, however, for the steam turbine is undoubtedly in the propulsion of ships. The large and increasing amount of horse-power and the greater size and speed of the modern engines tend towards some form which shall be light, capable of perfect balancing and economical in steam. The marine engine of the piston type does not entirely fulfil all these requirements, but the compound turbine engine, as made in 1892, appeared to be capable of doing so, and of becoming an ideal marine engine. On the other hand, an element of uncertainty lay in the high speed of the turbine engine, and to couple it directly to a propeller of ordinary proportions would have led to failure.

In January 1894, a pioneer syndicate was formed to explore the problem, those chiefly associated in the undertaking being the Earl of Rosse, Christopher Leyland, John Simpson, Campbell Swinton, Norman Cookson, the late George Clayton, H. C. Harvey, and Gerald Stoney. It was deemed expedient, for reasons of economy and also of time (as many alterations were anticipated), to build as small a vessel as possible, but not so small as to preclude the attainment of an unprecedented high speed in the event of success. The *Turbinia* was constructed, her dimensions being 100 feet in length, 9 feet beam, 3 feet draught of hull, and 44 tons displacement. She was fitted with a turbine engine of 2000 actual horse-power, with an expansive ratio of a hundred-and-fifty-fold, also with a water-tube boiler of great power, of the express type, with small tubes. The turbine engine was designed to drive one screw shaft at a speed of from 2000 to 3000 revolutions per minute.

Many trials were made with screw propellers of various sizes and proportions, but the best speeds were quite disappointing, and it was clear that some radical defect lay in the propellers. This was corroborated by dynamometric measurements. The excessive slip of the propellers beyond the calculated amount, and their inefficiency, indicated a want of sufficient blade area upon which the thrust necessary to drive the ship was distributed—in other words, the water was torn into cavities behind the blades. These cavities contained no air, but only vapour of water, and the greater portion of the power of the engine was consumed in the formation and maintenance of these cavities instead of the propulsion of the vessel. This phenomenon was first noticed in the trials of the torpedo-boat *Daring*, by Messrs. Thornycroft and Mr. Barnaby, shortly before the commencement of the trials of the *Turbinia*, and was named "cavitation" by Mr. R. E. Froude.

This phenomenon has been investigated experimentally with propellers of small size working inside an oval tank, so as to represent approximately the conditions of slip ratio customary in fast ships. To enable the propeller to cause cavitation more easily the tank is closed and the atmospheric pressure removed from the surface of the water

above the propeller by an air pump. Glass windows are fitted for observation and illumination. Under these conditions the only forces tending to hold the water together and resist cavitation are the small head of water above the propeller, and capillarity. The propeller is 2 inches diameter and 3 inches pitch; cavitation commences at about 1200 revolutions and becomes very pronounced at 1500 revolutions. Had the atmospheric pressure not been removed, speeds of 12,000 and 15,000 revolutions per minute would have been necessary, rendering observations more difficult.

The arrangement we have now was kindly suggested by Mr. Heath, and is a decided improvement, the revolving disc with narrow slots synchronising approximately with the revolutions of the propeller. The propeller is now seen to rotate very slowly, it also permits of the projection of the phenomenon on the screen, which was not possible with my previous arrangement. The permanence of the vortices behind the blades is very striking. The inference to be drawn from these experiments seems to be that for fast speeds of vessels, wide thin blades, a coarse pitch ratio, and moderate slip, are desirable for the prevention of cavitation, and in order to obtain the best efficiency in propulsion of the vessel.

To return to the *Turbinia*, a radical alteration was deemed necessary. A new turbine engine was made, consisting of three separate engines, high pressure, intermediate pressure, and low pressure, each of which drove one screw shaft, the power of the engine was distributed over three shafts instead of concentrated on one, and three propellers were placed on each shaft. The result of these changes was marvellous. The vessel now nearly doubled her speed, 30 knots was soon reached, and finally $32\frac{3}{4}$ knots mean speed on the measured mile authenticated, or the fastest speed then attained by any vessel afloat. The economy of her engines was investigated by Professor Ewing, assisted by Professor Dunkerly: the consumption of steam per indicated horse-power for all purposes at 31 knots speed was found to be $14\frac{1}{2}$ lbs., or in other words, with a good marine boiler the coal consumption would be considerably under 2 lbs. per indicated horse-power, a result better than is obtained in torpedo-boats or torpedo-boat destroyers with ordinary triple expansion engines.

The vessel's reversing turbine gave her an astern speed of $6\frac{1}{2}$ knots, and she could be brought to rest in 36 seconds when running at 30 knots speed, and from rest she could be brought up to 30 knots in 40 seconds.

The *Turbinia* cruised from the Tyne to the Naval Review at Spithead, where she steamed on the day of the Review at an estimated speed of $34\frac{1}{2}$ knots. These results represent about 2300 indicated horse-power, and may be said to have been obtained without a very abnormal performance as regards the boiler; its total heating surface being 1100 square feet, and an evaporation of about 28 lbs. per square foot at the speed of $34\frac{1}{2}$ knots.

These speeds were not obtained by bottling up the steam and

opening the regulating valve on coming to the measured mile, but were maintained for many miles together with constant steam pressure, and as long as the fires were clean. On the other hand, the endurance of the engines themselves seems to be unlimited, all heavy pressures, including the thrust of the propellers, that would in ordinary engines come on the bearings, being counterbalanced by the steam pressure acting on the turbines.

It seems clear that the results obtained in the case of the *Turbinia* were almost entirely due to the economy in steam of the turbine engines, and the unusually small weight of the engines, shafting and propellers, in proportion to the power developed.

It may also be said that generally speaking every part of the machinery was as substantial as in naval vessels of the torpedo-boat class, yet she developed 100 horse-power per ton of machinery, and 50 horse-power per ton of total weight of vessel in working order.

The results of the *Turbinia* having been found satisfactory, the original company which built her was merged into a large company under the same directorate for carrying on the work on a commercial scale. At Wallsend-on-Tyne, the Parsons Marine Steam Turbine Company erected works, and in 1898 contracted with the Admiralty for a 31-knot torpedo-boat destroyer, the *Viper* (Fig. 2), which is of the same dimensions as the usual 30-knot vessels of this class, viz. 210 feet length, 21 feet beam, and about 350 tons displacement, but with machinery of much greater power than usual in vessels of this size ; they also contracted with Sir W. G. Armstrong, Whitworth and Co. for machinery for one of their torpedo-boat destroyers.

The turbine engines of these vessels are similar to those of the *Turbinia*, but are in duplicate, and consist of two distinct sets of engines on each side of the vessel. There are four screw shafts in all, entirely independent of each other, the two on each side being driven by one high and one low-pressure turbine respectively of about equal power ; the two low-pressure turbines drive the two inner shafts, and to each a small reversing turbine is also permanently coupled, and revolves idly with them when going ahead. The screw shafts are carried by brackets as usual, and two propellers are placed on each shaft, the foremost in each case having a slightly lesser pitch than the after one. The thrust from the screw shafts is entirely balanced by the steam acting on the turbines, so that there is extremely little friction.

The boilers, auxiliary machinery and condensers are of the usual type in such vessels, but their size is somewhat increased to meet the much larger horse-power to be developed, and to compensate for the lesser weight of the main engines, shafting, propellers, as well as the lighter structure of the engine beds. The boilers are of the Yarrow type, with a total heating surface of 15,000 square feet, and grate surface of 272 square feet, and the condensers have a cooling surface of 8000 square feet. The hull and all fittings are of the usual design.

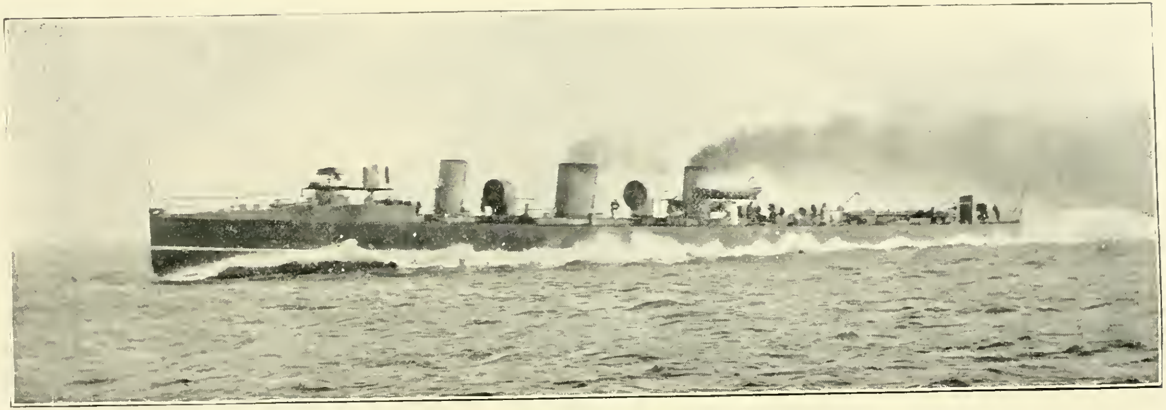

Fig. 2.—The 'Viper.'

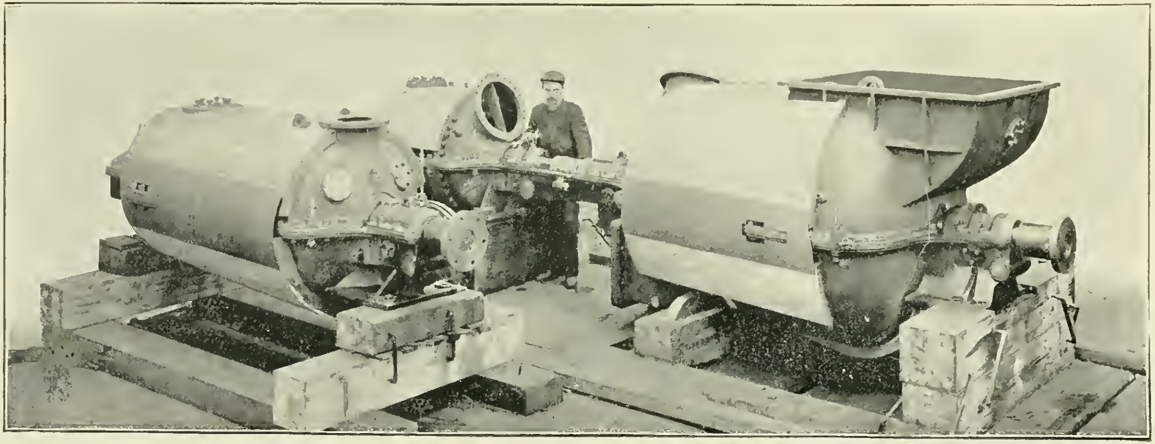

Fig. 3.—Turbine Engines of the 'Viper.'

Let us consider the machinery on one side of the vessel only: the steam from the boilers is admitted directly through a regulating valve to the high-pressure turbine driving one shaft, it then passes to the adjacent low-pressure turbine, driving its shaft independently, thence it flows to the condenser, and both the shafts then drive the vessel ahead; the reversing turbine revolves with the low-pressure shaft, and being permanently connected with the vacuum of the condenser no appreciable resistance is offered to its motion under these conditions. To go astern the ahead steam valve is closed and the astern valve opened, admitting the steam from the boilers to the reversing turbine, and reversing the direction of rotation of the inner screw shaft.

On the other side of the vessel the arrangement is the same, and it will be seen that she can be manœuvred as an ordinary twin-screw vessel, and with great facility and quickness.

On her second preliminary trial about three weeks ago, the mean speed of four consecutive runs on the measured mile reached 34·8 knots, and the fastest run was at the speed of 35·503 knots, which is believed to be considerably beyond the recorded speed of any vessel hitherto built. The vessel was scarcely completed at the time of this trial, and it is anticipated that still higher speeds will be realised on subsequent and official trials.* The speed of 35·5 knots, or nearly 41 statute miles, represents about 11,000 indicated horse-power in a vessel of 350 tons displacement, as compared with 6000 to 6500 developed in the 30-knot destroyers of similar dimensions and 310 tons displacement.

At all speeds there was very little vibration. Her speed astern is guaranteed to be 15½ knots.

The *Viper* has surpassed the *Turbinia* in speed, and is at the present time the fastest vessel afloat.

In regard to the general application of turbine machinery to large ships, the conditions appear to be more favourable in the faster class of vessels, such as cross-Channel boats, fast passenger vessels, liners, cruisers and battleships; in all such vessels the reduction in weight of machinery, and the economy in the consumption of coal per horse-power, are important factors; the absence of vibration is also a question of first importance, securing the comfort of passengers, and, in the case of ships of war, permitting of greater accuracy in sighting of the guns.

The model exhibited represents a proposed cross-Channel boat for the Dover and Calais or Newhaven and Dieppe routes. She is 270 feet length, 33 feet beam, 1000 tons displacement, and 8 feet 6 inches draught of water. She has spacious accommodation for 600 passengers, and with machinery developing 18,000 horse-power would have a sea speed of about 30 knots as compared with the speed of 19 to 22 knots of the present vessels of similar size and accommodation.

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* The *Viper* has since attained with full trial weights on board a mean speed of 36·58 knots, on a one-hour's full-power trial, the fastest runs being at the rate of 37·118 knots per hour.

It is perhaps interesting to examine possibilities of speed that might be attained in a special unarmoured cruiser, a magnified torpedo-boat destroyer of light build, with scanty accommodation for her large crew, but equipped with an armament of light guns and torpedoes. Let us assume that her dimensions are about double those of the 30-knot destroyers, or of the *Viper*, with plates of double the thickness, and specially strengthened to correspond with the increased size and speed; length 420 feet, beam 42 feet, maximum draught 14 feet, displacement 2800 tons, indicated horse-power 80,000. There would be two tiers of water-tube express boilers; these, the engines and coal bunkers, would occupy the whole of the lower portion of the vessel, the crew's quarters and armaments would be on the upper decks. There would be eight propellers of 9 feet in diameter, revolving at about 400 revolutions per minute, and her speed would be 44 knots. She could carry coal at this speed for about eight hours, and she would steam at from 10 to 14 knots, with a small section of the boilers and supplemental machinery more economically than other vessels of similar size, and of ordinary type and power, and when required all the boilers could be used, and full power exerted in about half an hour.

In the case of an Atlantic liner or a cruiser of large size, turbine engines would effect a reduction in weight of machinery, and also increased economy in fuel, tending either to a saving in coal on the one hand, or, if preferred, to some increase in speed on the same coal consumption per voyage.

In conclusion, it may be remarked that in the history of engineering progress, the laws of natural selection generally operate in favour of those methods which are characterised by the greater simplicity and greater economy, whether these advantages be great or small.

The progress in this undertaking has perhaps been slow, but many difficulties were met with besides those of a mechanical nature, and, as is generally the case, the success so far attained has been largely due to devoted colleagues and staff, and in the marine developments to the enterprising and generous financial assistance.

My thanks are due to the officials of this Institution for the kind assistance they have afforded me in the arrangement of the apparatus.

[C. A. P.]

WEEKLY EVENING MEETING,

Friday, February 2, 1900.

Alexander Siemens, Esq., M. Inst. C.E., Vice-President, in the Chair.

Signor G. Marconi, M. Inst. C.E.

Wireless Telegraphy.

When Ampère threw out the suggestion that the theory of a universal ether, possessed of merely mechanical properties, might supply the means for explaining electrical facts, which view was upheld by Joseph Henry and Faraday, the veil of mystery which had enveloped electricity began to lift. When Maxwell published, in 1864, his splendid dynamical theory of the electro-magnetic field, and worked out mathematically the theory of ether waves, and Hertz had proved experimentally the correctness of Maxwell's hypothesis, we obtained, if I may use the words of Professor Fleming, "the greatest insight into the hidden mechanisms of nature which has yet been made by the intellect of man."

A century of progress such as this has made wireless telegraphy possible. Its basic principles are established in the very nature of electricity itself. Its evolution has placed another great force of nature at our disposal.

We cannot pay too high a tribute to the genius of Heinrich Hertz, who worked patiently and persistently in a new field of experimental physics, and made what has been called the greatest discovery in electrical science in the latter half of the nineteenth century. He not only brought about a great triumph in the field of theoretical physics, but, by proving Maxwell's mathematical hypothesis, he accomplished a great triumph in the progress of our knowledge of physical agents and physical laws.

I cannot forbear saying one word as to the eminent electrician who was placed in his last home as recently as Saturday last, for it is manifest that several years ago Professor Hughes was on the verge of a great discovery, and, if he had persevered in his experiments, it seems probable that his name would have been closely connected with wireless telegraphy as it is with so many branches of electrical work, in which he gained so much renown and such great distinction.

The experimental proof by Hertz, thirteen years ago, of the identity of light and electricity, and the knowledge of how to produce, and how to detect these ether waves, the existence of which had been so far unknown, made possible true wireless telegraphy. I think I

may be justified in saying that for several years the full importance of the discovery of Hertz was realised but by very few, and for this reason the early development of its practical application was slow.

The practical application of wireless telegraphy at the *present* time is many times as great as the predictions of five years ago led us to expect in so short a time. The development of the art during the past three or four years, and its present state of progress, may perhaps justify the interest which is now taken in the subject. Yet only a beginning has been made, and the possibilities of the future can as yet be only incompletely appreciated. All of you know that the idea of communicating intelligence without visible means of connection is almost as old as mankind. Wireless telegraphy by means of Hertzian waves is, however, very young. I hope that if I pass over the story of the growth of this new art, as I have watched it, or do not attempt to prove questions of priority, no one will take it for granted that nothing is to be said on these subjects, or that all that *has* been said is entirely correct.

The time allowed for this discourse is too short to permit me to recount all the steps that have led up to the practical applications of to-day. I believe it will probably interest you more to hear of the problems which have lately been solved, and the very interesting developments which have taken place during the last few months.

I find that a great element of the success of wireless telegraphy is dependent upon the use of a coherer such as I have adopted. It has been my experience, and that of other workers, that a coherer as previously constructed—that is, a tube several inches long partially filled with filings enclosed by corks—was far too untrustworthy to fulfil its purpose. I found, however, that if specially prepared filings were confined in a very small gap (about 1 mm.) between flat plugs of silver, the coherer, if properly constructed, became absolutely trustworthy. In its normal condition the resistance of a good coherer is infinite, but when influenced by electric waves the coherer instantly becomes a conductor, its resistance falling to 100 or 500 ohms. This conductivity is maintained until the tube is shaken or tapped.

I noticed that by employing similar vertical and insulated rods at both stations it was impossible to detect the effects of electric waves of high frequency, and in that way convey the intelligible alphabetical signals, over distances far greater than had been believed to be possible a few years ago.

I had formerly ascertained (see paper read before the Institution of Electrical Engineers by G. Marconi, March 1899) that the distance over which it is possible to signal with a given amount of energy varies approximately with the square of the height of the vertical wire, and with the square root of the capacity of a plate, drum, or other form of capacity area which may be placed at the top of the wires.

The law governing the relation of height and distance has already been proved correct up to a distance of 85 miles. Many months ago

it was found possible to communicate from the North Haven, Poole, to Alum Bay, Isle of Wight, with a height of 75 feet, the distance being 18 miles. Later on two installations with vertical wires of double that length, i.e. 150 feet, were erected at a distance of 85 miles apart, and signals were easily obtained between them. According to a rigorous application of the law, 72 miles ought to have been obtained instead of 85 ; but as I have previously stated, the law has been proved only to be approximately correct, the tendency being always on what I might call the right side ; thus we obtain a greater distance than the application of the law would lead us to believe. There is a remarkable circumstance to be noted in the case of the 85 miles signalling. At the Alum Bay station the mast is on the cliff, and there is no curvature of the earth intervening between the two stations ; that is to say, a straight line between the base of the Haven and Alum Bay stations would clear the surface of the sea. But in the case of the 85 miles the two stations were located on the sea-level, and between them exists a hill of water, owing to the earth's curvature, amounting to over 1000 feet. If those waves travelled only in straight lines, or the effect was noticeable only across open space, in a direct line, the signals would not have been received, except with a vertical wire 1000 feet high at both stations.

While carrying out some experiments nearly three years ago at Salisbury, Captain Kennedy, R.E., and I tried numerous forms of induction coils wound in the ordinary way, that is, with a great number of turns of wire on the secondary circuit, with the object of increasing, if possible, the distance or range of transmission ; but in every case we observed a very marked decrease in the distance obtainable with the given amount of energy and height. Similar results were obtained some months later, I am informed, in experiments carried out by the General Post Office engineers at Dover.

In all our above-mentioned experiments the coils used were those in which the primary consisted of a smaller or larger number of turns of comparative thick wire, and the secondary of several layers of thinner wire. I believe I am right in saying that hundreds of these coils were tried, the result always being that by their employment the possible distance of signalling was considerably diminished instead of being increased. We eventually found an entirely new form of induction coil that would work satisfactorily, and that began to increase the distance of signalling.

The results given by some of the new form of induction coils have been remarkable. During the naval manœuvres I had an opportunity of testing how much they increased the range of signalling with a given amount of energy and height. When working between the cruisers *Juno* and *Europa*, I ascertained that when the induction coil was omitted from the receiver, the limit distance obtainable was seven miles, but with an improved form of induction coil included, a distance of over sixty miles could be obtained with certainty. This demonstrated that the coils I used at that time increased the possible distance nearly

tenfold. I have now adopted these induction coils, or transformers, at all our permanent stations.

A number of experiments have been carried out to test how far the Wehnelt brake was applicable in substitution for the ordinary make and brake of the induction coil at the transmitting station; but although some excellent results have been obtained over a distance of forty miles of land, the amount of current used, and the liability of the brake getting fatigued or out of order, have been obstacles which have so far prevented its general adoption.

As is probably known to most of you, the system has been in practical daily operation between the East Goodwin Lightship, and the South Foreland Lighthouse since December 24, 1898, and I have good reason for believing that the officials of Trinity House are convinced of its great utility in connection with lightships and lighthouses. It may be interesting to you to know that, as specially arranged by the authorities of Trinity House, although we maintain a skilled assistant on the lightship, he is not allowed to work the telegraph. The work is invariably done by one of the seamen on the lightship, many of whom have been instructed in the use of the instrument by one of my assistants. On five occasions assistance has been called for by the men on board the ship, and help obtained in time to avoid loss of life and property. Of these five calls for assistance, three were for vessels run ashore on the sands near the lightship, one because the lightship herself had been run into by a steamer, and one to call a boat to take off a member of the crew who was seriously ill.

In the case of a French steamer which went ashore off the Goodwins, we have evidence, given in the Admiralty Court, that by means of one short wireless message, property to the amount of 52,588*l.* was saved; and of this amount, I am glad to say, the owners and crews of the lifeboats and tugs received 3000*l.* This one saving alone is probably sufficient in amount to equip all the lightships round England with wireless telegraph apparatus more than ten times over. The system has also been in constant use for the official communication between the Trinity House and the ship, and is also used daily by the men for private communication with their families, etc.

It is difficult to believe that any person who knows that wireless telegraphy has been in use between this lightship and the South Foreland day and night, in storm and sunshine, in fog and in gales of wind, without breaking down on any single occasion, can believe, or be justified in saying, that wireless telegraphy is untrustworthy or uncertain in operation. The lightship installation is, be it remembered, in a small damp ship, and under conditions which try the system to the utmost. I hope that before long the necessary funds will be at the disposal of the Trinity House authorities, in order that communication may be established between other lightships and lighthouses and the shore, by which millions of pounds' worth of property and thousands of lives may be saved.

At the end of March 1899, by arrangement with the French

Government, communication was established between the South Foreland Lighthouse and Wimereux, near Boulogne, over a distance of thirty miles, and various interesting tests were made between these stations and French war ships. The maximum distance obtained at that time, with a height of about 100 feet on the ships, was forty-two miles. The commission of French naval and military officers who were appointed to supervise these experiments, and report to their government, were in almost daily attendance on the one coast or the other for several weeks. They became intensely interested in the operations, and I have good reasons to know made satisfactory reports to their government. I cannot allow this opportunity to pass without bearing willing testimony to the courtesy and attention which characterised all the dealings of these French gentlemen with myself and staff.

The most interesting and complete tests of the system at sea were, however, made during the British naval manœuvres. Three ships of the "B" fleet were fitted up, the flagship *Alexandra* and the cruisers *Juno* and *Europa*. I do not consider myself quite at liberty to describe all the various tests to which the system was put, but I believe that never before were Hertzian waves given a more difficult or responsible task. During these manœuvres I had the pleasure of being on board the *Juno*, my friend, Captain Jackson, R.N., who had done some very good work on the subject of wireless telegraphy before I had the pleasure of meeting him, being in command. With the *Juno* there was usually a small squadron of cruisers, and all orders and communications were transmitted to the *Juno* from the flagship, the *Juno* repeating them to the ships around her. This enabled evolutions to be carried out even when the flagship was out of sight. This would have been impossible by means of flags or semaphores. The wireless installations on these battleships were kept going night and day, most important manœuvres being carried out and valuable information telegraphed to the Admiral when necessary.

The greatest distance at which service messages were sent was 60 nautical miles, between the *Europa* and the *Juno*, and 45 miles, between the *Juno* and the *Alexandra*. This was not the maximum distance actually obtained, but the distance at which, under all circumstances and conditions, the system could be relied upon for certain and regular transmission of service messages. During tests messages were obtained at no less than 74 nautical miles (85 land miles).

As to the opinion which naval experts have arrived at concerning this new method of communication, I need only refer to the letters published by naval officers and experts in the columns of 'The Times' during and after the period of the autumn manœuvres, and to the fact that the Admiralty are taking steps to introduce the system into general use in the navy.

As you will probably remember, victory was gained by the "B"

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fleet, and perhaps I may venture to suggest that the facility which Admiral Sir Compton Domville had of using the wireless telegraph in all weathers, both by day and night, contributed to the success of his operations.

Commander Statham, R.N., has published a very concise description of the results obtained in the 'Army and Navy,' illustrated, and I think it will be interesting if I read a short extract from the admirable description he has published:—

"When the reserve fleet first assembled at Tor Bay, the *Juno* was sent out day by day to communicate at various distances with the flagship, and the range was speedily increased to over 30 miles, ultimately reaching something like 50 miles. At Milford Haven the *Europa* was fitted out, the first step being the securing to the main topmast head of a hastily prepared spar carrying a small gaff or sprit, to which was attached a wire, which was brought down to the starboard side of the quarter-deck through an insulator and into a roomy deck house on the lower after-bridge which contained the various instruments.

"When hostilities commenced the *Europa* was the leading ship of a squadron of seven cruisers despatched to look for the convoy at the rendezvous. The *Juno* was detached to act as a link when necessary and to scout for the enemy, and the flagship of course remained with the slower battle squadron. The *Europa* was in direct communication with the flagship long after leaving Milford Haven, the gap between reaching to 30 or 40 miles before she lost touch while steaming ahead at a fast speed. (This difference between the ranges of communication on these ships was owing to the *Juno* having a higher mast than the *Alexandra*.)

"Reaching the convoy at four o'clock one afternoon, and leaving it and the several cruisers in charge of the senior captain, the *Europa* hastened back towards another rendezvous, where the Admiral had intended remaining until he should hear whether the enemy had found and captured the convoy; but scarcely had she got well ahead of the slow ships when the *Juno* called her up and announced the Admiral coming to meet the convoy. The *Juno* was at this time fully 60 miles distant from the *Europa*.

"Now imagine," says Commander Statham, "a chain of vessels 60 miles apart. Only five would be necessary to communicate some vital piece of intelligence a distance of 300 miles, receive in return their instructions, and act immediately all in the course of half an hour or less. This is possible already. Doubtless a vast deal more will be done in a year or two or less, and meanwhile the authorities should be making all necessary arrangements for the universal application of wireless telegraphy in the navy."

The most important results, from a technical point of view, obtained during the manœuvres were the proof of the great increase of distance obtained by employing the transformer in the receiver, as already explained, and also that the curvature of the earth which inter-

vened, however great the distance attained, was apparently no obstacle to the transmission. The maximum height of the top of the wire attached to the instruments above the water did not on any occasion exceed 170 feet, but it would have been geometrically necessary to have had masts 700 feet high on each ship in order that a straight line between their tops should clear the curved surface of the sea when the ships were 60 nautical miles apart. This shows that the Hertzian waves had either to go over or round the dome of water 530 feet higher than the tops of the masts, or to pass through it, which latter course I believe would be impossible.

Some time after the naval manœuvres, with a view to showing the feasibility of communicating over considerable distances on land, it was decided to erect two stations, one at Chelmsford and another at Harwich, the distance between them being 40 miles. These installations have been working regularly since last September, and my experiments and improvements are continually being carried out at Chelmsford, Harwich, Alum Bay, and North Haven, Poole.

In the month of September last, during the meetings of the British Association in Dover and of the Association Française pour l'avancement de Science in Boulogne, a temporary installation was fixed in the Dover Town Hall, in order that members present should see the practical working of the system between England and France. Messages were exchanged with ease between Wimereux, near Boulogne, and Dover Town Hall. In this way it was possible for the members of the two associations to converse across the Channel, over a distance of 30 miles.

During Professor Fleming's lecture on the 'Centenary of the Electric Current,' messages were transmitted direct to and received from France, and *viâ* the South Foreland Lighthouse to the East Goodwin Lightship. An interesting point was that it was demonstrated that the great masses of the Castle Rock and South Foreland cliffs lying between the Town Hall, Dover, and the lighthouse did not in the least degree interfere with the transmission of signals. The result was, however, by no means new. It only confirmed the results of many previous experiments, all of them showing that rock masses of very considerable size intervening between two stations do not in the least affect the freedom of communication by ether wave telegraphy. (See 'Journal of the Institution of Electrical Engineers,' April 1899, p. 280.)

It was during these tests that it was found possible to communicate direct from Wimereux to Harwich or Chelmsford, the intervening distance being 85 miles. This result was published in a letter from Professor Fleming addressed to the 'Electrician' on September 29. The distance from Wimereux to Harwich is approximately 85 miles, and from Wimereux to Chelmsford also 85 miles, of which 30 miles are over sea and 55 over land. The height of the poles at these stations was 150 feet, but if it had been necessary for a line drawn between the tops of the masts to clear the curvature of the earth they

would have had to have been over 1000 feet high. I give these results to show what satisfactory progress is being made with this system.

In America, wireless telegraphy was used to report from the high seas the progress of the yachts in the International Yacht Race, and I think that occasion holds the record for work done in a given time, over four thousand words being transmitted in the space of less than five hours on several different days.

Some tests were carried out for the United States Navy; but, owing to insufficient apparatus, and to the fact that all the latest improvements had not been protected in the United States at that time, it was impossible to give the authorities there such a complete demonstration as was given to the British authorities during the naval manœuvres. Messages were transmitted between the battleship *Massachusetts* and the cruiser *New York* up to a distance of 36 miles.

A few days previous to my departure from America the war in South Africa broke out. Some of the officials of the American line suggested that, as a permanent installation existed at the Needles, Isle of Wight, it would be a great thing, if possible, to obtain the latest war news before our arrival on the *St. Paul* at Southampton. I readily consented to fit up my instruments on the *St. Paul*, and succeeded in calling up the Needles station at a distance of 66 nautical miles. By means of wireless telegraphy, all the important news was transmitted to the *St. Paul* while she was under way, steaming twenty knots, and messages were despatched to several places by passengers on board. News was collected and printed in a small paper called the 'Transatlantic Times' several hours before our arrival at Southampton.

This was, I believe, the first instance of the passengers of a steamer receiving news while several miles from land, and seems to point to a not far distant prospect of passengers maintaining direct and regular communication with the land they are leaving and with the land they are approaching, by means of wireless telegraphy.

At the tardy request of the War Office, we sent out Mr. Bullocke and five of our assistants to South Africa. It was the intention of the War Office that the wireless telegraph should only be used at the base and on the railways, but the officers on the spot realised that it could only be of any practical use at the front. They therefore asked Mr. Bullocke whether he was willing to go to the front. As the whole of the assistants volunteered to go anywhere with Mr. Bullocke, their services were accepted, and on December 11 they moved up to the camp at De Aar. But when they arrived at De Aar, they found that no arrangements had been made to supply poles, kites or balloons, which, as you all know, are an essential part of the apparatus, and none could be obtained on the spot. To get over the difficulty, they manufactured some kites, and in this they had the hearty assistance of two officers, viz. Major Baden-Powell and Captain

Kennedy, R.E., who have often helped me in my experiments in England. (Major Baden-Powell, it will be remembered, is a brother of the gallant defender of Mafeking.)

The results which they obtained were not at first altogether satisfactory, but this is accounted for by the fact that the working was attempted without poles or proper kites, and afterwards with poles of insufficient height, while the use of the kites was very difficult, the kites being manufactured on the spot with very deficient material. The wind being so variable, it often happened that when a kite was flying at one station there was not enough wind to fly a kite at the other station with which they were attempting to communicate. It is therefore manifest that their partial failure was due to the lack of proper preparation on the part of the local military authorities, and has no bearing on the practicability and utility of the system when carried out under normal conditions.

It was reported that the difficulty of getting through from one station to another was due to the iron in the hills. If this had not been cabled from South Africa, it would hardly be credible that any one should have committed himself to such a very unscientific opinion. As a matter of fact, iron would have no greater destructive effect on these Hertzian waves than any other metal, the rays apparently getting very easily round or over such obstacles. A fleet of thirty ironclads did not affect the rays during the naval manœuvres, and during the yacht race I was able to transmit my messages with absolute success across the very high buildings of New York, the upper stories of which are iron.

However, on getting the kites up, they easily communicated from De Aar to Orange River, over a distance of some seventy miles. I am glad to say that, from later information received, they have been able to obtain poles, which although not quite high enough for long distances are sufficiently useful. We have also sent a number of Major Baden-Powell's kites, which are the only ones I have found to be of real service.

Stations have been established at Modder River, Enslin, Belmont, Orange River and De Aar, which work well and will be invaluable in case the field telegraph line connecting these positions should be cut by the enemy.

It is also satisfactory to note that the military authorities have lately arranged to supply small balloons to my assistants for portable installations on service waggons.

While I admire the determination of Mr. Bullocke and our assistants in their endeavour to do the very best they could with most imperfect local means, I think it only right to say that if I had been on the spot myself I should have refused to open any station until the officers had provided the means for elevating the wire, which, as you know, is essential to success.

Mr. Bullocke and another of our assistants in South Africa has been transferred with some of the apparatus to Natal to join General

Buller's forces, and it is likely that before the campaign is ended wireless telegraphy will have proved its utility in actual warfare. Two of our assistants bravely volunteered to take an installation through the Boer lines into Kimberley; but the military authority did not think fit to grant them permission, as it probably involved too great a risk.

What the bearing on the campaign would have been if working installations had been established in Ladysmith, Kimberley and Mafeking, before they were besieged, I leave military strategists to state. I am sure you will agree with me that it is much to be regretted that the system could not be got into these towns prior to the commencement of hostilities.

I find it hard to believe that the Boers possess any workable instruments. Some instruments intended for them were seized by the authorities at Cape Town. These instruments turned out to have been manufactured in Germany. Our assistants, however, found that these instruments were not workable. I need hardly add that as no apparatus has been supplied by us to any one, the Boers cannot possibly have obtained any of our instruments.

I have spoken at great length about the things which have been accomplished. I do not like to dwell upon what may, or will, be done in the immediate or more distant future, but there is one thing of which I am confident—viz. that the progress made this year will greatly surpass what has been accomplished during the last twelve months; and, speaking what I believe to be sober sense, I say, that by means of the wireless telegraph, telegrams will be as common, and as much in daily use, on the sea as at present on land.

[G. M.]

GENERAL MONTHLY MEETING,

Monday, February 5, 1900.

Sir James Crichton-Browne, M.D. F.R.S., Treasurer and Vice-President, in the Chair.

The Hon. Evelyn Ellis,
The Hon. Everard Feilding, LL.B.
The Hon. Francis Robert Henley,
Cecil Elsdale Newton, Esq.
The Hon. Richard Oliver,

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Mr. Charles Hawksley for his Donation of £100, to Dr. Frank McClean for his Donation of £25, and to Sir Andrew Noble, K.C.B., for his Donation of £100 to the Fund for the Promotion of Experimental Research at Low Temperatures.

The Chairman reported the decease of Professor David Edward Hughes, F.R.S., a Manager of the Royal Institution, on the 22nd of January last. The following Resolution was submitted:—

Resolved:—That the Managers desire to express to Mrs. Hughes their heartfelt sympathy in her sad bereavement, and the deep regret of the Managers in losing, by the death of Professor Hughes, a most valued Colleague.

Professor Hughes became a Member of the Royal Institution in 1882. He delivered a Discourse on 'Theory of Magnetism' (illustrated by experiments) on February 8th, 1884. As a Manager and Vice-President he has rendered important services to the Institution, and his scientific researches, which have received world-wide recognition, have done much to promote the objects of the Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

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WEEKLY EVENING MEETING,

Friday, February 9, 1900.

Alfred B. Kempe, Esq., M.A. F.R.S., Vice-President, in the Chair.

Professor J. Reynolds Green, Sc.D. F.R.S.

Symbiosis and Symbiotic Fermentation.

The features of socialism are realised perhaps more fully in the vegetable world than in any other sphere of life. What we generally call a plant and think of as an individual may with much more appropriateness be considered a colony, composed of units which are essentially similar, which live together in enormous numbers, and which divide up the work of the community among themselves with the greatest completeness; securing thereby to the greatest extent possible the well-being of the colony, so that by this thorough co-operation of all its constituent members it comes to possess so marked an individuality that its composite character escapes our observation.

With few exceptions every plant is divided up into a number of cavities of varying size and shape, limited by delicate membranes of varying thickness and texture. These chambers are the dwelling places of the units of which I have spoken, which may be seen by careful examination to occupy them. The units are known as *protoplasts*; each consists of a small piece of living substance possessing a certain degree of structure or differentiation. Each is in communication with its intermediate neighbours by very delicate filaments which extend through the separating walls, so that all the protoplasts of a plant are in actual connection with each other. The shape and disposition of the protoplast within its chamber varies a good deal; some fill the whole space, others occupy only part of it, and in the majority of cases they line the cell membrane as a colourless transparent film, having a large cavity filled with water in the centre.

In studying the manner of the growth and development of almost every plant we find this view of its structure so placed before us that we cannot escape the conclusion with which I started. Every plant which is developed sexually begins its career as a single protoplast without any cell-wall, and from this simple individual the most complex plant-body is gradually constructed by the process of multiplication of protoplasts. The protoplasts from which various plants start their development are not alike in all cases. In some of the simplest sea-weeds we find them free-swimming bodies, making their

way in the water by means of vibratile filaments or cilia. After a time they come to rest, secrete for themselves a protecting membrane or cell-wall, and then by repeated division form a long filament. Each protoplast in this thread is exactly like all the others, both in structure and in properties and powers. In the higher plants the first protoplast of the new individual or colony is not motile, and is enclosed in some kind of cavity or receptacle, which differs in different groups of plants. Its behaviour is, however, similar to that of the one we have examined, and by repeated division of itself and of those to which it gives origin, the complex plant is constructed. In the larger plants a great deal of subsequent change or differentiation takes place, which can be traced directly to the division of labour which the massive size of the organism necessitates.

In nature this general principle of co-operation is found to be more wide-spread than this. We find inter-relations of varying degrees of complexity existing between distinct plants, which co-operate to a greater or less extent with each other for their mutual advantage. The relationship is not confined to any particular group of plants, nor is any corresponding complexity of structure or organisation necessary for the establishment of such an association. We find examples of the co-operation of flowering plant with flowering plant, of fungus with fungus or alga, and of flowering plant with either of the latter.

The intimacy of the relationship is not the same in all cases; indeed the beneficial character of the association varies very greatly. Sometimes it is almost all on one side; sometimes each constituent organism benefits equally; in many cases there is a preponderance of advantage on one side or the other.

In cases in which the advantages of the alliance are more or less reciprocal we apply the term *symbiosis* to the association. It is important to bear in mind this limitation, as there are many cases of the close association of two plants in which the advantages reaped by one are attended with disadvantage and often serious injury to the other. This feature is characteristic of what is known as *parasitism.*

There are also instances to be met with in which the close connection of the two plants is attended by almost negative results, neither gaining much advantage from their association. Of this, an example is afforded by a few cryptogamous plants, particularly *Anthoceros* and *Azolla*, and by certain phanerogams, e.g. *Gunnera* and *Cycas*, in cavities in the tissues of which particular Algæ take up their residence. In these cases neither organism appears to benefit by the association, except that the alga finds a shelter and a home, protected from difficulties incident to a watery environment. There is no establishment in these cases of any co-operation between the organisms, no formation of anything resembling a conjoint plant. It is usual to apply the term *commensalism* to such an association as this.

Among the flowering plants a true symbiosis is exhibited by a group, the chief members of which are met with in the natural orders Scrophulariaceæ and Santalaceæ. They are herbaceous plants of small size, which flourish in pastures or other situations where there is a rich development of herbage. Some species are found growing freely in woods. Of these, the little plant known as *Thesium humifusum* affords a typical example. It grows on rough ground usually among grass, and develops a fairly large root system, which extends to some depth in the soil. When the branches of its roots come into contact with the roots of other plants in the same earth, a little swelling is developed on them at the point of contact, and from this swelling certain outgrowths of the epidermal cells proceed, which penetrate into the tissues of the other root, establishing a very intimate relationship between them, so intimate in fact that ultimately the tissues of the two roots become indistinguishable in the zone common to both. By the relationship thus set up, food materials elaborated by one of the symbionts can pass into the other, and thus each can co-operate with the other for the common good. The degree to which each partner benefits is not always the same, but the relationship proves to be mutually advantageous. The association of the two always depends upon the root of the Thesium fastening upon that of some other plant. It never receives an attachment in turn. The same thing is true of the members of the Scrophulariaceæ which have been alluded to, conspicuous among which are *Rhinanthus*, *Melampyrum*, and *Euphrasia.* On account of this somewhat one-sided way of establishing the symbiosis, these plants are frequently spoken of as *root-parasites.* It is better perhaps not to employ that term, as the alliance appears to be mutually beneficial, at any rate to a considerable extent.

An equally interesting instance of a similar relationship is presented by the mistletoe. This plant has been generally somewhat vaguely described as parasitic on the oak, the apple, the poplar, and other trees. The degree of its parasitism has, however, been very much exaggerated, if that term should be applied to the plant at all. The mistletoe always grows from seed which is carried from the parent plant by birds, and deposited upon a branch of one of these trees. The seed germinates where it is dropped upon the branch, and a bulky hemispherical radicle is pressed against the bark. This grows and flattens into a strongly-marked disc, from which a projection is put out which penetrates the soft tissue of the cortex and reaches the wood. Lateral outgrowths from this projection or *sinker* then grow at right angles and burrow along the cortex of the tree. From these new sinkers again are sent down into the soft tissues. They grow slowly, and as the branch which they penetrate becomes thicker by the activity of the cambium layer, the new wood thus formed surrounds the sinkers, and the latter thus become embedded in it. A very complete union of the tissues of the mistletoe and the host is thus established. The mistletoe is a slow-growing plant, and the

full development of this relationship is only gradually brought about.

The advantages of the symbiosis appear on the whole to be greater on the side of the mistletoe than on that of its host. During the longer portion of the year the latter is in full foliage, and no doubt the invader derives considerable nutriment from it by the passage of the latter into the sinkers from the parenchyma and the bast of the host. It must, moreover, draw from the wood of the latter the water and the mineral salts which it needs for the working of its own chlorophyll apparatus. The mistletoe, however, is an evergreen plant, while its host loses its leaves as autumn gives way to winter. The advantage then becomes transferred to the host plant, which derives supplies of nutrient material from the mistletoe, the latter being able to elaborate it from the raw materials it absorbs on the one hand from the air, and on the other from the wood to which its sinkers are so closely connected.

Instances of symbiotic association between members of the higher and of the lower plants are not at all infrequent. One of the most curious relationships is exhibited by the young roots of a number of our woody plants, some shrubs, and other trees. Among the former we find large numbers of the *Heaths* and *Rhododendrons*, while the trees are represented by the *Firs*, *Oaks*, *Beeches*, *Willows* and *Poplars*, and many others. The roots of these plants when taken up carefully from the soil are found to be covered with a dense feltwork of fungal hyphæ, constituting, in some cases, a mantle of considerable thickness. The filaments of the fungus not only cover the outside of the roots but penetrate into the cortex and ramify at first between the cells, later on entering them and branching copiously in their interior. The thickness of the mantle varies a good deal in different cases, but its composition is much the same in all. From it delicate threads or solitary hyphæ extend outwards and ramify among the particles of soil, appearing almost exactly like the long root-hairs with which plants are usually supplied. There is a constant setting up of this symbiotic relationship going on in the soil; as a young root grows out it finds itself in mould which is permeated by hyphal threads or contains spores which are ready to produce them. The young hypha makes its way into the young root, forcing itself between certain of the cells, and once inside it ramifies freely, being nourished by the juices of the root. As long as the latter continues to grow or even to exist the mycelium accompanies it, becoming larger and thicker during its development, and sometimes forming a considerable mass of hyphæ in which the root is enveloped.

The symbiosis in this case is not at first apparent. The advantages appear to be altogether on the side of the fungus, which evidently thrives very luxuriantly at the expense of the nutrient materials which it extracts from the cells of the cortex of the root into which we have seen it penetrating. The loss of this particular nutrient matter is, however, of very small relative importance. The

tree is vigorous, usually, indeed, in proportion to the development of the fungus, a fact which shows that the advantages of the connection must at any rate be mutual.

Observation of the structure of the young root shows us that unlike normal terrestrial roots, it does not give rise to any root-hairs. If the mycelial mantle is prevented from developing, as may be done by cultivating a plant from seed by the method of water-culture, or if the fungus is removed from a root on which it has become established, the plant has no power of flourishing and very speedily dies. If the mycelium is suffered to play its part this neglect to produce root-hairs is not attended with any ill-results. In fact the mycelium takes up the function of absorbing water which is discharged in other terrestrial plants by the root-hairs. These filaments we have seen ramify in the soil, coming in contact with its ultimate particles just as the root-hairs do; they are in communication also with the interior of certain cells of the cortex, in which they break up and form a kind of network. They thus serve for the supply of water and dissolved mineral matter to the tree, which in turn supports them by the elaborated nutrient substances they derive from the cells in which they end.

A case of a more complete alliance is afforded by the great group of Cryptogamous plants known as the *Lichens*, the two constituents of which are of relatively equal systematic position, and which enter into the composition of the symbiont organism in almost equal amounts.

The Lichens are of very wide distribution and show extraordinary diversities of form. Many of them appear as incrustations on stones or wood, or the bark of trees, and resemble mere discolorations of the surface; others are thin and papery, somewhat resembling leaves in their texture, though not in their shape, the latter being very irregularly lobed and corrugated. Some again are of more sturdy habit resembling miniature shrubs, while yet others are fleshy or gelatinous cushions of very irregular form.

The thallus of a lichen, when cut, so as to show its section, is found to be composed of two constituents; a green or sometimes blue-green alga, imbedded in the midst of a mingled mass of hyphal fungal filaments. The order in which these two constituents are arranged is very varied, the alga being sometimes fairly evenly distributed throughout the thickness of the body or thallus, and in other cases confined to a particular region of it. There is a great variety possible in the species which form the lichen; a particular alga may co-operate with different fungi and a particular fungus with different algæ.

The gradual formation of a lichen by the actual growing together of its two constituents can often be observed. The green cells of *Protococcus* which are found in such quantity on the bark of elm trees, forming a green dusty-looking surface over the brown bark, are frequently found attached by or associated with hyphal filaments of some fungus, which coil round and enclose them. As the algal

cells divide and multiply, the hyphæ keep pace with them and the lichen is gradually formed. Filamentous algæ, such as *Scytonema*, enter into the composition of lichens in the same way.

When once formed the lichen can reproduce itself in a way which obviates the necessity for continual reconstruction. In the interior of the old thallus, generally near one surface, particular groups of the two constituents become separated from the rest. Each group consists of a few algal cells wrapped round by hyphal filaments. By the rupture of the thallus these collections, known as *soredia*, are liberated and grow into fresh lichen plants.

When we consider the physiological peculiarities of this association we find there is a very complete and satisfactory division of labour. The fungus is able to condense aqueous vapour, which is very necessary in the dry situations lichens occupy. It thus provides a solvent for much of the dust and other débris of its resting place, and having effected its solution absorbs it into the hypha and conducts it into the interior of the plant. This solution and absorption is facilitated by its power of excreting particular substances, such as certain vegetable acids. It thus carries raw material to the constructive algal cells of the interior. The fungus also secures the adhesion of the thallus to the substratum. The alga, on the other hand, by nature of its chlorophyll or green colouring matter, is able to construct food from the raw materials presented to it. It can absorb and decompose the carbon dioxide of the air, and when supplied with water and dissolved mineral substances, furnished by the fungus in the way described, can build up carbohydrates such as sugar. Both partners can no doubt take part in the processes connected with nitrogenous metabolism.

It is noteworthy that in such an alliance the algal cells grow more vigorously and become larger than similar ones which have no symbiotic partner.

A case of symbiosis which is more deeply interesting on account of its wide-reaching economical importance, is afforded by various plants belonging to the *Leguminosæ*, the *pea*, *bean* and *clover* family. In order to make clear the special phenomena which these plants present, it is necessary to diverge for a few moments to speak of a feature of vegetable protoplasm.

The food of plants is derived eventually from extremely simple bodies, which undergo a process of construction into more elaborate ones in their tissues before they can serve actually as food. The simple bodies absorbed are chiefly carbon dioxide from the air, water and various inorganic salts from the soil, the former being taken up by the leaves and other green parts, the latter by the root-hairs. From these simple bodies complex ones, among which especially must be mentioned sugar and proteids, are formed in the cells of the plant, conspicuous among which are those containing chlorophyll bodies or chloroplasts. Among the constituents of proteid matter, which is an absolutely essential part of the food of every living

organism, whether vegetable or animal, is the gas nitrogen. The source from which this element is derived by the plant has been ascertained to be some compound present in the soil, either a nitrate or nitrite, or a compound of ammonia. When we consider that the atmosphere surrounding the leaves of a plant contains about 80 per cent. of this gas, it seems surprising that this vast store should not be utilised. The most careful experiments have shown, however, that in the vast majority of instances atmospheric nitrogen is of no use to a plant.

During the past 10 or 15 years, however, various physiologists have determined that in the case of the leguminous plants mentioned just now, more nitrogen can be found in the body of the plant in the course of very careful quantitative experiments, than can have been derived from the medium in which it has been cultivated. Soil, plant and manurial applications have been all most carefully analysed. The results have been so startling, and so contrary to received opinion, that they have been very carefully checked by many distinguished observers, but their absolute accuracy has been found indisputable. Clearly then, these leguminous plants can in some way appropriate the nitrogen of the atmosphere. A careful investigation of the phenomena presented by these plants during their growth, led to the discovery of the existence of a form of symbiosis, to which the phenomenon is due. All the details of it are not yet clear, and no doubt much time will elapse before the steps of appropriation are fully known. When one of these plants, growing in ordinary soil, is removed from the earth and its root-system is carefully washed, it presents the appearance of much-branching roots, on which certain tubercular outgrowths are visible. These occur, both upon the main tap root and upon the branches, and are present in considerable numbers, arising usually in those parts of the root where the root-hairs occur.

A tubercle cut across shows that its axial mass consists of large polyhedral cells getting smaller towards the apex, where they form a mass of rapidly-growing tissue. Several layers of compressed cells surround them.

In the formation of these bodies a tubular structure seems to penetrate one of the root-hairs, and to make its way into the cells just under the surface of the root, in which it branches somewhat freely. It can be seen ramifying in the substance of the young tubercle, the growth of which is apparently due to a hypertrophy of the tissue caused by the stimulus of the irritation of its presence. In the large-celled tissue the parasite becomes rampant, and indeed, the tubes of which it at first consists, can be seen throughout the tubercle. In older tubercles the tube can be seen to end, at first blindly, in the centre of the cells, and from the blunt end by repeated branching and constriction of the branches, an enormous number of extremely small bodies are cut off, which accumulate in the protoplasm of the cells. They are of various shapes, sometimes straight

and rod-like, at others formed like a V or a Y. The latter have been seen to arise from the branching of originally straight ones. As the tubercle still grows, there goes on an enormous coincident development of these minute bodies which, from their resemblance to bacteria, have been called *bacteroids.* When the root perishes at the end of the summer, most of the plants being annuals, these bacteroids are liberated from the cells in which they are formed, the latter decaying. The soil, consequently, in which these plants have been growing, contains them in large numbers. The original infection of the plant is no doubt brought about by one of these organisms in the soil coming into contact with the root-hair, into which it makes its way as already described.

We know but little so far of the steps by which the nitrogen is made to enter into combination. It depends upon the bacteroid or the tubular structure to which it gives rise, for if the soil is carefully sterilised no such appropriation takes place. If such a soil is afterwards watered with an extract of an unsterilised soil in which some of the plants have been growing, the tubercles will be speedily produced and the gain of nitrogen made evident. The same result can be obtained by inoculating a root with a bacteroid obtained from a pure culture of the organisms.

There is some evidence which points to the fungus or schizophyte as having the power to fix the nitrogen. In some cases this appears to be done in the sheath of the tube as it penetrates the tissue of the root. In some cases these tubes have not been observed, so that this cannot be the only locality. It seems probable that it goes on also in the cells which we have seen are filled with the bacteroids. In any case the appropriation seems to be done by the lowly partner in the symbiosis, which thus provides valuable nutrient matter for the leguminous plant. The latter cannot independently fix the nitrogen, whether it is in symbiosis or not.

The advantages which the green plant affords to its fellow symbiont are such as have been described in other cases already.

These cases of symbiosis are on the whole not very difficult to explain, and we can trace more or less fully the influence of the one plant upon the other. Associations of organisms are found also lower down in the scale of life and their relations are much more obscure. The symbiosis seems in many cases to be shared by several organisms, but most probably in nearly all cases some of these are only casual intruders which have nothing to do with the true association of the others.

We find many instances of a relation of this kind among the fungi and schizomycetes which set up various fermentations. It is necessary here to define carefully what we mean by the terms symbiosis and symbiotic fermentation when used in connection with these organisms. The idea of mutual co-operation for the common benefit, which has been traceable through all the relationships so far considered, cannot be seen so completely in these cases, no doubt because

their metabolic processes are not at present understood. Probably when all is known we shall find this is the result of the association, but at present it cannot be said to be established. We must substitute for it in connection with fermentation the idea of power in particular directions possessed by the conjoint organisms, which is not exhibited by either symbiont separately.

We must guard ourselves still further. There are many fermentative processes carried on by microbes in which many such organisms take part simultaneously or successively. Prominent among these we have the phenomena of putrefaction. In this process many organisms take part, some decomposing the original material, for instance, meat; others attacking the products of the activity of the first, and so on. It is evident that there is here a very great complexity, and many of the organisms benefit from, and are indeed dependent upon the activity of the others. But there is at the same time a conspicuous independence among them, and many of them would not be seriously missed if they should not happen to be present. We may rather compare such a series of actions with the struggle for existence which is exemplified by a number of plants growing together in a somewhat confined area, and competing therefore for the advantages which the environment presents.

Nor does a true symbiotic fermentation arise in the case of two organisms which live together, one being fed by the materials which the first produces. A curious case is known in which three microbes have been found together, of which one, *Bacillus ramosus*, decomposes proteid or gelatinous matters, setting free ammonia; the second, *Nitrosomonas*, converts the ammonia into nitrites, or salts of nitrous acid; the third, *Nitrobacter*, forms nitric acid or its compounds from this. These are consecutive fermentations and can only go on in the order named, so long as all three organisms are present. They apparently do not influence each other in any way apart from making their particular products; Nitrosomonas will make nitrous acid from ammonia if it is supplied to it from any other source than the activity of the Bacillus, and is, therefore, not dependent upon the presence of the latter as such. The Nitrobacter is to the same extent independent of the Nitrosomonas. True symbiotic fermentation involves a much closer relationship between the organisms which take part in it, and the general reactions incident to their associated life are much more complex, the influence of one organism upon the other modifying considerably the course of action of each.

The first of these cases of symbiosis which may be noticed is the fermentation set up by the so-called *Kephir*, which produces an aerated beverage largely used in the Caucasus. The Kephir exists in the form of, say, somewhat translucent lumps, which swell somewhat in water; they are known as Kephir grains.

When examined carefully these lumps are found to be composed of three separate organisms which can, by the usual culture methods, be separated from each other. The first of these is known as *Dis-*

pora ; it is a gelatinous bacterium, consisting of much-coiled filaments with strongly marked sheaths, which form what is technically known as a zooglœa. The gelatinous appearance of the grain is mainly due to this. The second is the ordinary bacterium which produces lactic acid from sugar, and the third is a yeast. These are both surrounded by the coiled filaments of the Dispora, in whose meshes they are entangled.

The Kephir sets up its fermentation in the milk of cows, goats, or sheep. The yeast and the bacterium either jointly or separately split up the lactose or milk sugar into two other sugars, galactose and glucose. The yeast then forms alcohol from the latter, and the bacterium lactic acid from the former. The action of the Dispora seems to be to change the casein of the milk, so that it is not precipitated or curdled by the lactic acid.

These general outlines only sketch the probabilities of the course of action which has not so far been definitely studied. The mode of development and the full function of the separate factors are not yet known.

Another organism, or rather association of organisms, constitutes what is known as the ginger-beer plant. It is the agent of fermentation in the preparation of the so-called "stone ginger-beer," which is a favourite beverage in the country districts of England, there being a pleasing delusion that it is non-alcoholic. The origin of this "plant" is obscure ; no one knows exactly where it came from, but it is preserved carefully by the villagers and handed on indeed from one to another.

This material is composed essentially of two organisms; one is a yeast known as *Saccharomyces pyriformis*, from its being somewhat pear-shaped ; the other a bacterium, to which the name *Bacterium vermiforme* has been given. In their association the bacterium forms long coiled filaments, in the meshes of which the yeast is entangled. As it occurs in use it is seldom pure, but is found to be contaminated by admixture with other yeasts and schizomycetes, which however have nothing to do with the fermentation it sets up, their presence being accidental. As it is met with it forms, like Kephir, translucent lumps of a jelly-like consistency which are slightly heavier than water.

The yeast appears to differ but little from the ordinary yeast of beer. When it is isolated it sets up a fermentation of cane-sugar and grape-sugar, which exhibits the usual features.

The bacterium is a very interesting form, and when it is isolated it can be made to grow in two different conditions:—(1) In the form of long rods, varying in number and invested by a common translucent, often wrinkled sheath. This is found to be capable of coiling and twisting in very curious ways, and can form a convoluted thread-like gelatinous body. (2) The constituent microbes can escape from the sheath and live and multiply freely without forming one. In many preparations the empty sheaths can frequently be found, some-

times of great length and much convoluted. This sheath is normally formed by the microbe, and the process can sometimes be watched. The bacterium sometimes partially escapes from the end as it is beginning to form it, and so the sheath continually elongates; the part behind the organism remaining empty. The conjoint organism can be synthesised from pure cultures of the two symbionts, but it is not very easy to make them come together, as the existence of the complete structure is dependent on the activity of them both.

The most efficient method is to prepare a glass vessel containing an appropriate nutrient fluid, and to suspend inside it another vessel of porous porcelain, so that the walls of the two do not come into contact. This porcelain pot must contain the same nutritive solution. The yeast must then be sown in the inner vessel and the bacterium in the outer one. In this condition the two organisms cannot come into contact, but the products of their activity can mix by diffusion through the porcelain vessel. The whole apparatus must then be kept immersed in an atmosphere of carbon-dioxide, so that no oxygen may gain access to the microbes. After a time, when both are growing vigorously, a little of the yeast must be transferred to the outer vessel, when the two will grow together into the conjoint form.

Another method, which does not, however, give such satisfactory results, is to prepare a good growth of the yeast in a culture-fluid containing bouillon and grape-sugar, and while its activity is at its height to inoculate the culture with some of the bacteria.

In the condition of symbiosis both members are more active than when they exist separately. The cells of the yeast bud more actively and the coils of the bacterium are formed more freely. More carbon-dioxide is evolved from the fermenting liquid. The details of the fermentation have not yet been examined, but certain points of interest as to the inter-action of the one with the other have been established.

The sheathing form of the bacterium can only be produced when oxygen is replaced by carbon-dioxide. The advantages of the protective sheath to the microbe seem apparent. In the symbiotic association the yeast absorbs the oxygen, and during its fermentative activity produces carbon-dioxide, thus providing the necessary conditions for the formation of the sheaths, that is, for the full development of the bacterium. When they are cultivated separately, the yeast appears to form some substance or substances which inhibit the formation of the sheaths. This does not occur when the symbiosis is established. It may be connected perhaps with the relative quantity of the two organisms present together in the free condition, or with some variation of the vital processes under the different conditions of cultivation, but the cause is at present obscure. The bacterium benefits by extractives or other substances excreted by the yeast, and the latter profits by the removal of these matters through the agency of the former.

A third organism which must be classed with both these occurs in Madagascar as a curious gelatinous-looking substance found attacking

the sugar-cane. In its external appearance it cannot very well be distinguished from either of the others. It consists again of a yeast and a bacterium which are associated in the same way as are the organisms in the ginger-beer plant. The yeast is not the same as in the latter, and the bacterium differs in some respects, though its structure and mode of behaviour are very much like those of *Bacterium vermiforme.* It is a sheathed organism which seems to have the power of casting the sheaths in the same way as has been described.

The result of the symbiotic fermentation is an effervescent liquid of distinctly acid taste, which if the fermentation is not prolonged makes a very pleasant beverage. If the action is allowed to go on for several weeks the percentage of the acids becomes so great that it is no longer possible to drink it.

The morphological features of the organism resemble to a very large extent those of the ginger-beer plant. A more detailed examination has been made of the products of the fermentation than in the former case, and certain features of the symbiosis have been ascertained. The organisms of which the plant consists can easily be separated and independent cultures made of both, so that the separate and conjoint fermentations can be studied.

The yeast, when cultivated alone, produces alcohol and of course carbon-dioxide, together with a little acetic and a little succinic acid. In these respects it appears to differ very little from the ordinary yeast of beer.

The bacterium can form no alcohol, but it gives rise to the formation of much larger quantities of both these acids. Besides these, if cultivated in solutions of cane-sugar it produces a very large quantity of two hemicelluloses, which when in a certain excess, sink to the bottom of the liquid and form a kind of gelatinous sediment. This viscous matter can be precipitated by alcohol from its watery solution. It appears to be the same material as the sheath of the organisms, and it is noteworthy that when it is formed the organisms possess no sheaths. The gelatinous substance is not the discarded sheaths of the bacterium, but seems to be formed in the culture liquid at the expense of the sugar. It is in fact a viscous fermentation of the latter.

If we compare the products of the fermentation of the two organisms separately, those of the same two microbes when present together in the same culture-fluid, but not in the symbiotic form, and finally those of the conjoint organism, we find that there is evidence of some subtle influence exerted by the one upon the other when they are in complete symbiosis, though it is difficult to suggest in what that influence consists.

Comparing cultures of them both separately with one in which both were present but independent, the nutrient medium being a mixture of cane-sugar and fruit-sugar, less acid was produced in the latter case in the proportion of 224 to 1175. The same quan-

tities of yeast and bacteria were employed in each experiment so that the results might be comparable. With grape-sugar the ratio was 117 to 112, which is a little in the opposite direction. When they were conjoined symbiotically the amount of acid produced was 474 compared with 224 when they were together but free, a mixture of cane and fruit sugars being the culture medium. With grape-sugar the proportions were 196 to 112; with fruit-sugar alone they produced when symbiotic about one-eighth as much acid as when they were free and separate.

Without entering upon the question of the advantages or disadvantages of the association, these figures show that the symbiotic relation greatly modified the progress and the results of the fermentation. The bacterium when in symbiosis with the yeast forms its sheaths readily in the presence of 2 per cent. of alcohol. When free it produced no sheaths in less than 5–8 per cent., and then only in long standing under the spirit. In 2 per cent. or less it only brought about the viscous sediment which has been described.

But little information has been gained as to the influence of the one on the other in the symbiosis. It is not one of the preparation of a suitable nutrient material for each other. The action of the bacterium being to form acid, if this were the case the yeast should be found capable of flourishing in such acid as it produces. But the reverse is the case. In the absence of the bacterium so small a quantity as ·25 per cent. of acetic acid is distinctly deleterious to it.

Conversely the alcohol which the yeast produces is not made use of by the bacterium in the production of the acid. It is utterly unlike *Bacterium aceti*, the so-called *vinegar plant* which oxidises alcohol to acetic acid. If alcohol is added to a fermentation which is conducted by the bacterium alone, that alcohol can be recovered unchanged when the fermentation has ceased. The source of the acid appears to be the sugar, and a preliminary alcoholic fermentation takes no part in the transformation.

There is presumably some physiological influence excited by the one organism upon the other, as the products of the fermentation are so different when the two are in the different relationships described; but what is the nature of that influence there is at present no evidence to show.

[J. R. G.]

WEEKLY EVENING MEETING,

Friday, February 16, 1900.

Sir Frederick Bramwell, Bart., D.C.L. LL.D. F.R.S., Honorary Secretary and Vice-President, in the Chair.

H. Warington Smyth, Esq., M.A. LL.M. F.R.G.S.

Life in Indo-China.

The lecturer said that no apology was needed for directing attention to the conditions which affect human life in one of the most important portions of Asia—the Indo-Chinese Peninsula. Situated midway between the two great Empires India and China, which in geographical extent, in population and wealth, as well as intellectual achievement, had, notwithstanding their want of political cohesion, been amongst the greatest that the world has seen; this great peninsula has drawn its civilisation first from one and then from the other; so much is this the case that wherever one travels in it, the best that it has is invariably traceable to the influence of Indian or Chinese modes of thought. It has of itself produced nothing of importance or of originality to the world. Its intellectual and moral life has come from without. Cut off by tracts of mountainous forest country in the far north-west and north-east, but little communication was ever able to take place directly overland with either empire; thus the sea has ever been the front door of Indo-China. The causes underlying the distinctiveness of the Indo-Chinese races become but gradually apparent to the traveller. In few portions of the world is he so impressed with the sense of the predomination of the physical forces of nature. Mr. Warington Smyth then proceeded to speak of the wonderful rivers of the Indo-Chinese Peninsula, describing their features and scenery up to the mountains. He then went on to refer to the other means of transport which are available to the inhabitants who live either back in the great plains away from navigable rivers, by streams which for the greater part of the year are navigable, or up in the mountains and in the torrent valleys of the highlands. The animal most utilised in the low country, where for half the year the tracts are under water, is the water buffalo, which is well known in India and elsewhere. Mr. Smyth mentioned as an interesting fact their deep-rooted aversion to the white man whom they scent afar off. On one occasion, when riding with a dozen Shan dignitaries, who had come out to meet him on entering the town, they had to gallop for their lives before a herd of so-called tame buffaloes who had discovered his presence at half a mile distant. A respected and dignified friend of his spent ten hours on the hottest day of the year on the not very commodious summit of

a telegraph pole awaiting the departure of a herd of buffaloes which waited impatiently below throughout the day. The buffalo, when he is not too sulky, is used for ploughing, for treading out the padi, and for drawing the big buffalo carts. But the ship of the jungle of Indo-China is the elephant. He is to be found in every upland village of the Lao country, swinging his trunk among the cocks and hens, or minding the baby in the back-yard. From the forest he slowly hauls the teak trees to the nearest river, or climbs patiently along the hill sides with his master's last crop of cotton or tobacco on his back. Mr. Smyth went on to speak of the methods of capture and training the elephants in different parts of the peninsula, and remarked that it is a singular fact which speaks well for the intelligence and humanity of the Asiatic, that not a single race which has come into contact with the elephant has failed to make use of his sagacity and strength by domesticating him. It is interesting to note that the price of the elephant in most parts of Indo-China is about that of the horse in this country, but it varies greatly with his age and attainments. In the teak districts, large sums amounting to over 200*l.* are paid for a good hauler. The lecturer briefly referred to the ponies and mules which are used for purposes of transport by the Mohammedan traders of the north.

He proceeded to refer to the people of Indo-China, and remarked that no country in the world presents so many different types, or provides such an interesting field for the ethnologist. It was complicated by the perpetual warfare which has to our own times been waged between the various races with ever-changing fortunes since the dawn of their history, and which has prevented settled government and has not given an opportunity for the development of peaceful industries. Mr. Warington Smyth passed over the semi-Chinese inhabitants of Tong-King, the Annamites, the Cambodians, the Malays, the Siamese and Burmans, and went on to speak more particularly of the Laos and the Shans, and other tribes about which information is not so easily accessible. All the Lao people had adopted Buddhism, but at the same time they retained a large admixture of spirit worship. Speaking of the tribe known as the Musur as the Lao call them, or Musho as the Burmese Shans style them, the lecturer remarked that it was stated on good authority that M. Pavie, the French Commissioner, during the Anglo-French Commission, was ready to claim the whole country on the ground that these were evidently French subjects, else they could not have been called by a name which was so evidently a corruption for Monsieur by their neighbours. The badness of the pun was said by unkind persons to be equalled only by the quality of the claims made by France to this district.

Going on to speak of the architecture of the Indo-Chinese Peninsula, Mr. Warington Smyth said that the traveller would notice two primary forms in all the Buddhist—and there was practically no other—architecture of Indo-China, the spire-like pagoda, taking various

shapes in different parts of the country, and the monastery chapel or chief building, in which the statue of Buddha sits sadly contemplative in the cool lofty height of the great roof. At once the most ancient, the most extensive, and the most magnificently executed of the buildings of Indo-China are those which centre round the great ruins of Angkor Wat and extend over an area of some twenty square miles in the immediate neighbourhood of the great lake of Cambodia. They consist of half-a-dozen main groups, most of them many miles apart in the great Cambodian plain, but which were, it appears, at one time mostly connected by stone causeways which carried the roads above the flood levels of the surrounding country, the remains of which still exist and are met with in unexpected places in the jungle. These groups consisted of cities, palaces and temples; and were built apparently between the sixth and tenth centuries. The most remarkable and the most perfect of them all is the great temple of Angkor, or Nakawn, Wat, which, for the gigantic boldness of its design, the perfection of its workmanship, and the delicacy of its detail, may well take rank as one of the greatest buildings of the world. Of the Kmer, of whose high artistic taste and architectural skill we have such remarkable evidence, very little that is accurate is known, except that they came from India, advancing into the country apparently by Hatien on the coast to the south-west [where extensive fifth century ruins still exist] and gradually spreading over the country further north even than Korat [15° N. Lat.]. But the evidence of the buildings themselves goes to show that towards the ninth century the decadence of the building race had set in and that the advance north of Korat and into Anam occurred during the period of decline. Originally professing Brahmanism, the evidence shows that Buddhism was introduced probably in the period just following the culmination of its power. But what catastrophe actually completed the ruin of the race, whether it was due to actual conquest, or to gradual absorption and decay, are points on which a great deal more light is wanted. Certainly, travelling in Cambodia, it is difficult to believe that the present dull-witted, unenterprising and essentially stupid Cambodians are the direct descendants of the highly intelligent and tasteful building race which immigrated from India. These buildings, it may be remarked, bear a distinct resemblance to the great remains at Pagan in Upper Burmah, which were destroyed by Kublai Khan in the thirteenth century—though the latter consist entirely of brickwork, while the best portions of the Cambodian remains are magnificently fitted sandstone blocks. And the architects of Siam have gone to Cambodia for their models, as will be seen in the great brick towns and the finest of the Buddhist remains at the old cities of Ayuthia, Sawankalok, and elsewhere. But none have ever rivalled Angkor Wat in one feature—that of its wonderful stone roofing; which has preserved the building alike against the assaults of the climate, and the insidious attacks of the roots of the peepul [*ficus indica*] and other destructive trees.

In conclusion, the lecturer went on to speak of the mineral wealth of the country. He said that the coal deposits of Tong-King are certainly extraordinary for the great thickness to which they attain, and recent trials go to show that Tong-King coal will soon have an assured position in the markets of the East, but these beds appear to be the only ones of really workable character. Gold occurs in very fine grains over wide areas in the alluvial sands of the great river systems, but no systematic hydraulic work has ever been attempted on a large scale. It is doubtful, he added, how far these deposits would repay European methods of working in a country where transport of machinery and commissariat is at present so expensive, and where the loss by sickness makes the labour question such a serious one. He also referred to the tin deposits in the Malay Peninsula and in the island of Junk-Ceylon. Speaking of the latter, he said that the whole island might be said to be one vast tin mine. Everybody talks of tin, everything smells of tin, the valleys have all been turned upside down for it, the mountains have been gashed into chasms which can be seen many miles away, and the jungle has been felled and cleared, all for the tin which underlies and pervades the whole.

Want of time prevented the ruby and sapphire deposits being more than touched upon by the lecturer.

[H. W. S.]

WEEKLY EVENING MEETING.

Friday, February 23, 1900.

His Grace The Duke of Northumberland, K.G. F.S.A. President, in the Chair.

Professor John H. Poynting, D.Sc. F.R.S.

Recent Studies in Gravitation.

The studies in gravitation which I am to describe to you this evening will perhaps fall into better order if I rapidly run over the well beaten track which leads to those studies, the track first laid down by Newton, based on astronomical observations, and only made firmer and broader by every later observation.

I may remind you, then, that the motion of the planets round the sun in ellipses, each marking out the area of its orbit at a constant rate, and each having a year proportional to the square root of the cube of its mean distance from the sun, implies that there is a force on each planet exactly proportioned to its mass, directed towards, and inversely as the square of its distance from the sun. The lines of force radiate out from the sun on all sides equally, and always grasp any matter with a force proportional to its mass, whatever planet that matter belongs to.

If we assume that action and reaction are equal and opposite, then each planet acts on the sun with a force proportional to its own mass; and if, further, we suppose that these forces are merely the sum totals of the forces due to every particle of matter in the bodies acting, we are led straight to the law of gravitation, that the force between two masses M_1 M_2 is always proportional to the product of the masses divided by the square of the distance r between them, or is equal to

$$\frac{G \times M_1 \times M_2}{r^2}$$

and the constant multiplier G is the constant of gravitation.

Since the force is always proportional to the mass acted on, and produces the same change of velocity whatever that mass may be, the change of velocity tells us nothing about the mass in which it takes place, but only about the mass which is pulling. If, however, we compare the accelerations due to different pulling bodies, as for instance that of the sun pulling the earth, with that of the earth pulling the moon, or if we compare changes in motion due to the different planets pulling each other, then we can compare their masses and weigh them, one against another and each against the

sun. But in this weighing our standard weight is not the pound or kilogramme of terrestrial weighings, but the mass of the sun.

For instance, from the fact that a body at the earth's surface, 4000 miles, on the average, from the mass of the earth, falls with a velocity increasing by 32 ft. / sec ^{2}, while the earth itself falls towards the sun, 92 million miles away, with a velocity increasing by about $\frac{1}{5}$ inch / sec ^{2}, we can at once show that the mass of the sun is 300,000 times that of the earth. In other words, astronomical observation gives us only the acceleration, the product of G $\times$ mass acting, but does not tell us the value of G nor of the mass acting, in terms of our terrestrial standards.

To weigh the sun, the planets, or the earth, in pounds or kilogrammes, or to find G, we must descend from the heavenly bodies to earthly matter, and either compare the pull of a weighable mass on some body with the pull of the earth on it, or else choose two weighable masses and find the pull between them.

All this was clearly seen by Newton, and was set forth in his System of the World (third edition, page 41).

He saw that a mountain mass might be used, and weighed against the earth by finding how much it deflected the plumb line at its base. The density of the mountain could be found from specimens of the rocks composing it, and the distance of its parts from the plumb line by a survey. The deflection of the vertical would then give the mass of the earth.

Newton also considered the possibility of measuring the attraction between two weighable masses, and calculated how long it would take a sphere a foot in diameter, of the earth's mean density, to draw another equal sphere, with their surfaces separated by $\frac{1}{4}$-inch, through that $\frac{1}{4}$-inch. But he made a very great mistake in his arithmetic, for while his result gave about 1 month the actual time would only be about $5\frac{1}{2}$ minutes. Had his value been right, gravitational experiments would have been beyond the power of even Professor Boys. Some doubt has been thrown on Newton's authorship of this mistake, but I confess that there is something not altogether unpleasing in the mistake even of a Newton. His faulty arithmetic showed that there was one quality which he shared with the rest of mankind.

Not long after Newton's death the mountain experiment was actually tried, and in two ways. The honour of making these first experiments on gravitation belongs to Bouguer, whose splendid work in thus breaking new ground does not appear to me to have received the credit due to it.

One of his plans consisted in measuring the deflection of the plumb line due to Chimborazo, one of the Andes peaks, by finding the distance of a star on the meridian from the zenith, first at a station on the south side of the mountain where the vertical was deflected, and then at a station to the west, where the mountain attraction was nearly inconsiderable, so that the actual nearly coincided with the geographical vertical. The difference in zenith

distances gave the mountain deflection. It is not surprising that, working in snowstorms at one station, and in sandstorms at the other, Bouguer obtained a very incorrect result. But at least he showed the possibility of such work, and since his time many experiments have been carried out on his lines under more favourable conditions. Now, however, I think it is generally recognised that the difficulty of estimating the mass of a mountain from mere surface chips is insurmountable, and it is admitted that the experiment should be turned the other way about and regarded as an attempt to measure the mass of the mountains from the density of the earth known by other experiments.

These other experiments are on the line indicated by Newton in his calculations of the attraction of two spheres. The first was carried out by Cavendish.

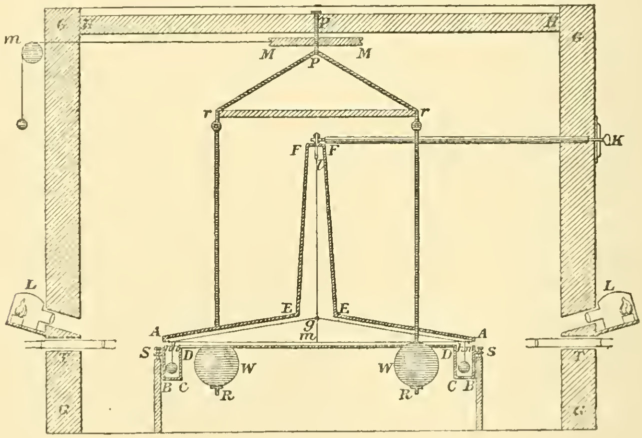

FIG. 1.—Cavendish's Apparatus.

In the apparatus (Fig. 1) he used two lead balls, B B, each 2″ in diameter. These were hung at the end of a horizontal rod 6′ long, the torsion rod, and this was hung up by a long wire from its middle point. Two large attracting spheres of lead, W W, each 12″ in diameter, were brought close to the balls on opposite sides so that their attractions on the balls conspired to twist the torsion rod round the same way, and the angle of twist was measured. The force could be reckoned in terms of this angle by setting the rod vibrating to and fro and finding the time of vibration, and the force came out to less than 1/3000 of a grain. Knowing M_1 M_2 and r the distance between them and the force G M_1M_2/r^2, of course Cavendish's result gives G, or knowing the attraction of a big sphere on a ball, and knowing the attraction of the earth on the same ball, that is its weight, the

experiment gives the mass of the earth in terms of that of the big sphere, and so its mean density. This experiment has often been repeated, but I do not think it is too much to say that no advance was made in exactness till we come to quite recent work.

By far the most remarkable recent study in gravitation is Professor Boys' beautiful form of the Cavendish experiment, a

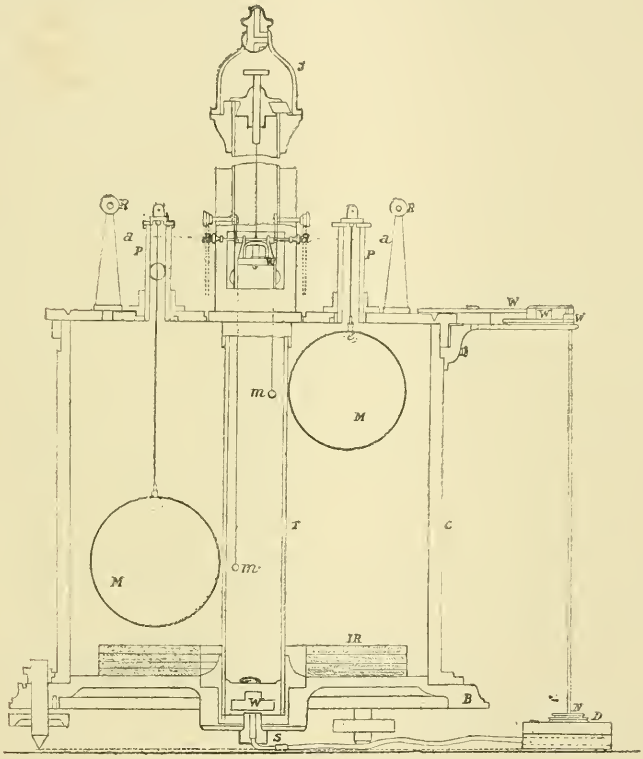

Fig. 2.—Boys' Apparatus.

research which stands out as a model in beauty of design and in exactness of execution (Fig. 2). But as Professor Boys has described his experiment already in this theatre * it is not necessary for me to more than refer to it. It is enough to say that he made the great discovery, obvious perhaps when made, that the sensitiveness of the apparatus is increased by reducing its dimensions. He therefore

* Proc. R.I. xiv. part ii. 1894, p. 353.

decreased the scale as far as was consistent with exact measurement of the parts of the apparatus, using a torsion rod, itself a mirror, only 2″ long, gold balls, *m m*, only $\frac{1}{4}''$ in diameter, and attracting lead masses, M M, only $4\frac{1}{4}''$ in diameter. The force to be measured was less than $1/5 \times 10^6$ grain.

The exactness of his work was increased by using as suspending wire one of his quartz threads. It would be difficult to overestimate the service he has rendered in the measurement of small forces by the discovery of the remarkable properties of these threads.

One of the chief difficulties in the measurement of these small gravitational pulls is the disturbances which are brought about by the air currents, which blow to and fro and up and down inside the apparatus, producing irregular motions in the torsion rod. These, though much reduced, are not reduced in proportion to the diminution of the apparatus.

A very interesting repetition of the Cavendish experiment has lately been concluded by Dr. Braun * at Mariaschein in Bohemia, in which he has sought to get rid of these disturbing air currents by suspending his torsion rod in a receiver which was nearly exhausted, the pressure being reduced to about $\frac{1}{200}$ of an atmosphere. The gales which have been the despair of other workers were thus reduced to such gentle breezes that their effect was hardly noticeable. His apparatus was nearly a mean proportional between that of Cavendish and Boys, his torsion rod being about 9″ long, the balls weighing 54 gms.—less than two ounces—and the attracting masses either 5 or 9 kgms. His work bears internal evidence of great care and accuracy, and he obtained almost exactly the same result as Professor Boys.

Dr. Braun carried on his work far from the usual laboratory facilities, far from workshops, and he had to make much of his apparatus himself. His patience and persistence command our highest admiration.

I am glad to say that he is now repeating the experiment, using as suspension a quartz fibre supplied to him by Professor Boys in place of the somewhat untrustworthy metal wire which he used in the work already published.

Professor Boys has almost indignantly disclaimed that he was engaged on any such purely local experiment as the determination of the mean density of the earth. He was working for the Universe, seeking the value of G, information which would be as useful on Mars or Jupiter or out in the stellar system as here on the earth. But perhaps we may this evening consent to be more parochial in our ideas, and express the results in terms of the mean density of the earth. In such terms then both Boys and Braun find that density 5·527 times the density of water, agreeing therefore to 1 in 5000.

* Denkschriften der Math. Wiss. Classe der Kais. Akad. der Wissenschaften Wien, lxiv. 1896.

There is another mode of proceeding which may be regarded as the Cavendish experiment turned from a horizontal into a vertical plane, and in which the torsion balance is replaced by the common balance. This method occurred about the same time to the late Professor U. Jolly and myself. The principle of my own experiment * will be sufficiently indicated by Fig. 3. A big bullion balance with a 4-foot beam had two lead spheres, A B, each about 50 lbs. in weight, hanging

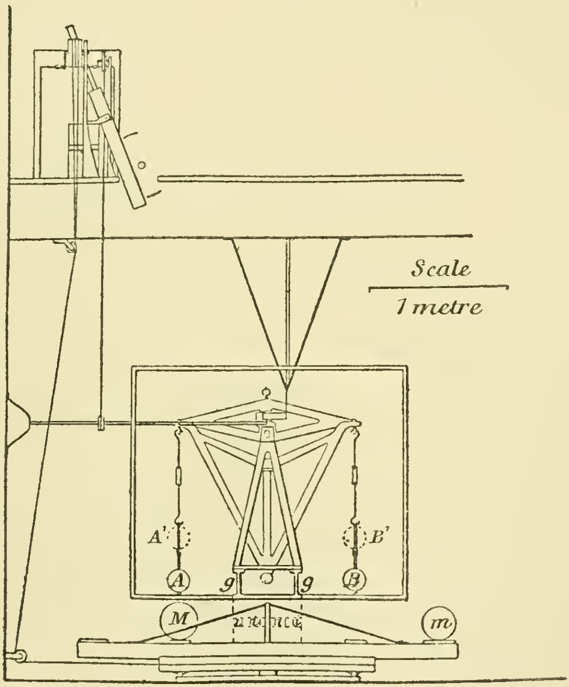

FIG. 3.—Common Balance Experiment (Poynting).

from the two ends in place of the usual scale pans. A large lead sphere, M, 1′ in diameter and weighing about 350 lbs., was brought first under one hanging weight, then under the other. The pull of the lead sphere acted first on one side alone and then on the other so that the tilt of the balance beam when the sphere was moved round was due to twice the pull. By means of riders the tilt and therefore the pull was measured directly as so much increase in weight. This increase, when the sphere was brought directly under the hanging

* Phil. Trans. 182, 1891, A, p. 565.

weight with 1' between the centres, was about $\frac{1}{5}$ mgm. in a total weight of 20 kgm. or about 1 in 100,000,000. If then a sphere one foot away pulls with $1/10^8$ of the earth's pull, the earth being on the average 20,000,000 feet away, it is easy to see that the earth's mass is calculable in terms of the mass of the sphere, and its density is at once deduced. The direct aim of this experiment, then, is not G, but the mass of the earth.

It is not a little surprising that the balance could be made to indicate such a small increase in weight as 1 in 100 million. But not only did it indicate, it measured the increase, with variations usually well within 1% of the double attraction, or to 1 in 5000 million of the whole weight, a change in weight which would occur merely if one of the spheres were moved $\frac{1}{40}$ inch nearer the earth's centre. This accuracy is only attained by never lifting the knife edges and planes during an experiment, thus keeping the beam in the same state of strain throughout, and, further, by taking care that none of the mechanism for moving the weights or riders shall be attached in any way to the balance or its case; two conditions which are absolutely essential if we are to get the best results of which the balance is capable.

Quite recently another common balance experiment has been brought to a conclusion by Professor Richarz and Dr. Krigar-Menzel* at Spandau, near Berlin. Their method may be gathered from Fig. 4. A balance of 23 cm., say 9-inch beam, was mounted above a huge lead pile about 2 metres cube, and weighing 100,000 kgm.

Two pans were supported from each end of the beam, one pan above, the other pan below the lead cube, the suspending wires of the lower pans going through narrow vertical tubular holes in the lead. Instead of moving the attracting mass, the attracted mass was moved. Masses of 1 kgm. each were put first, say, one in the upper right-hand pan, the other in the lower left-hand pan, when the pull of the lead block made the right hand heavier and the left hand lighter. Then the weights were changed to the lower right hand and the upper left hand, when the pulls of the lead pile were reversed. When we remember that in my experiment a lowering of the hanging sphere by $1\frac{1}{2}$ inches would give an effect as great as the pull I was measuring, it is evident that here the approach to and removal from the earth by over 2 metres would produce very considerable changes in weight, and, indeed, these changes masked the effect of the attraction of the lead. Preliminary experiments had, therefore, to be made before the lead pile was built up, to find the change in weight due to removal from upper to lower pan, and this change had to be allowed for. The quadruple attraction of the lead pile came out at 1·3664 mgm., and the mean density of the earth at 5·505.

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* Anhang zu den Abhandlungen der Königl. Preuss. Akad. der Wissenschaften zu Berlin, 1898.

This agrees nearly with my own result of 5·49, and it is a curious coincidence that the two most recent balance experiments agree very nearly at, say 5·5, and the two most recent Cavendish experiments agree at, say 5·53. But I confess I think it is merely a coincidence. I have no doubt that the torsion experiment is the more exact, though probably an experiment on different lines was worth making. And I am quite content to accept the value 5·527 as the standard value for the present.

And so the latest research has amply verified Newton's celebrated guess that "the quantity of the whole matter of the Earth may be five or six times greater than if it consisted all of water."

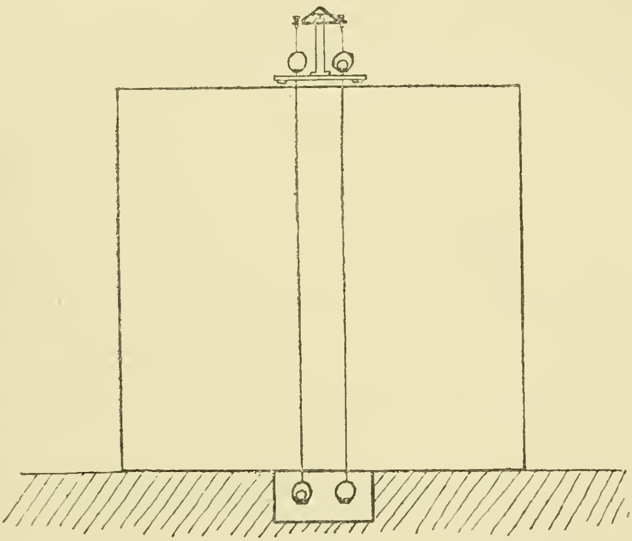

FIG. 4.—Common Balance Experiment (Richarz and Krigar-Menzel).

I now turn to another line of gravitational research. When we compare gravitation with other known forces (and those which have been most closely studied are electric and magnetic forces) we are at once led to inquire whether the lines of gravitative force are always straight lines radiating from or to the mass round which they centre, or whether, like electric and magnetic lines of force, they have a preference for some media and a distaste for others. We know, for example, that if a magnetic sphere of iron or cobalt or manganese is placed in a previously straight field, its permeability is greater than the air it replaces, and the lines of force crowd into it, as in Fig. 5. The magnetic action is then stronger in the presence of the sphere near the ends of a diameter parallel to the original course of the lines

of force, and the lines are deflected. If the sphere be diamagnetic, of water, or copper, or bismuth, the permeability being less than that of air, there is an opposite effect, as in Fig. 6, and the field is weakened at the end of a diameter parallel to the lines of force, and again the lines are deflected. Similarly, a dielectric body placed in an electric field gathers in the lines of force, and makes the field where the lines enter and leave stronger than it was before.

If we enclose a magnet in a hollow box of soft iron placed in a magnetic field, the lines of force are gathered into the iron and largely cleared away from the inside cavity, so that the magnet is screened from external action.

Now, common experience might lead us at once to say that there is no very considerable effect of this kind with gravitation. The evidence of ordinary weighings may, perhaps, be rejected, inasmuch as both sides will be equally affected as the balance is commonly used. But a spring balance should show if there is any large effect when used in different positions above different media, or in different

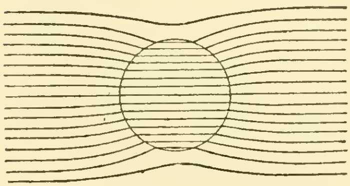

Fig. 5.—Paramagnetic sphere placed in a previously straight field.

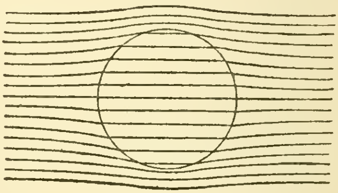

Fig. 6.—Diamagnetic sphere placed in a previously straight field.

enclosures. And the ordinary balance is used in certain experiments in which one weight is suspended beneath the balance case, and surrounded, perhaps, by a metal case, or perhaps, by a water-bath. Yet no appreciable variation of weight on that account has yet been noted. Nor does the direction of the vertical change rapidly from place to place, as it would with varying permeability of the ground below. But perhaps the agreement of pendulum results, whatever the block on which the pendulum is placed, and whatever the case in which it is contained, gives the best evidence that there is no great gathering in, or opening out of the lines of the earth's force by different media.

Still, a direct experiment on the attraction between two masses with different media interposed was well worthy of trial, and such an experiment has lately been carried out in America by Messrs. Austin and Thwing.* The effect to be looked for will be understood from Fig. 7. If a medium more permeable to gravitation is interposed

* Physical Review, v. 1897, p. 294.

between two bodies, the lines of force will move into it from each side, and the gravitative pull on a body, near the interposed medium on the side away from the attracting body, will be increased.

The apparatus they used was a modified kind of Boys' apparatus (Fig. 8). Two small gold masses in the form of short vertical wires, each ·4 gm. in weight, were arranged at different levels at the ends virtually of a torsion rod 8 mm. long. The attracting masses M_1 M_2 were lead, each about 1 kgm. These were first in the positions shown by black lines in the figure, and were then moved into the positions shown by dotted lines. The attraction was measured first when merely the air and the case of the instrument intervened, and then when various slabs, each 3 cm. thick, 10 cm. wide and 29 cm. high, were interposed. With screens of lead, zinc, mercury, water, alcohol or glycerine, the change in attraction was at the most about 1 in 500, and this did not exceed the errors of experiment. That is, they found no evidence of a change in pull with change of medium.

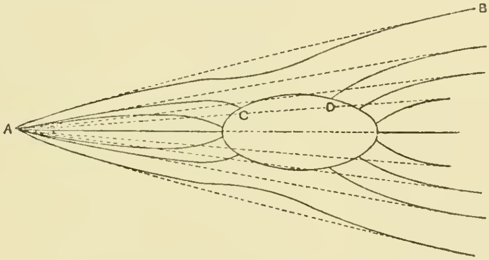

Fig. 7.—Effect of interposition of more permeable medium in radiating field of force.

If such change exists, it is not of the order of the change of electric pull with change of medium, but something far smaller. Perhaps, it still remains just possible, that there are variations of gravitational permeability comparable with the variations of magnetic permeability in media such as water and alcohol.

Yet another kind of effect might be suspected. In most crystalline substances the physical properties are different along different directions in a crystal. They expand differently, they conduct heat differently, and they transmit light at different speeds in different directions. We might, then, imagine that the lines of gravitative force spread out from, say a crystal sphere unequally in different directions. Some years ago, Dr. Mackenzie* made an experiment in America in which he sought for direct evidence of such unequal distribution of the lines of force. He used a form of apparatus like that of Professor Boys (Fig. 2), the attracting masses being calc spar spheres about 2 inches in diameter. The attracted masses in one experiment were small lead spheres about $\frac{1}{2}$ gm. each, and he

* Physical Review, ii. 1895, p. 321.

measured the attraction between the crystals and the lead when the axes of the crystals were set in various positions. But the variation in the attraction was merely of the order of error of experiment. In another experiment the attracted masses were small calc spar crystal cylinders weighing a little more than $\frac{1}{2}$ gm. each. But again there was no evidence of variation in the attraction with variation of axial direction.

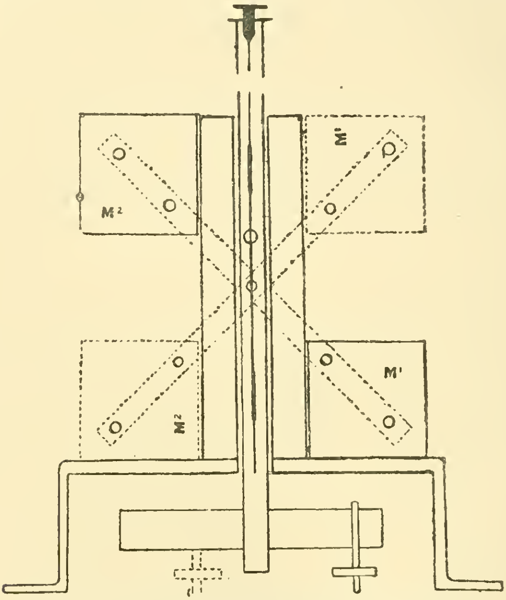

Fig. 8.—Experiment on gravitative permeability (Austin and Thwing).

Practically the same problem was attacked in a different way by Mr. Gray and myself.* We tried to find whether a quartz crystal sphere had any directive action on another quartz crystal sphere close to it, whether they tended to set with their axes parallel or crossed.

It may easily be seen that this is the same problem by considering what must happen if there is any difference in the attraction between two such spheres when their axes are parallel and when they are crossed. Suppose, for example, that the attraction is always greater when their axes are parallel, and this seems a reasonable supposition, inasmuch as in straightforward crystallisation successive parts of the crystal are added to the existing crystal, all with their axes parallel. Begin, then, with two quartz crystal spheres near each other with their axes in the same plane, but perpendicular to

* Phil. Trans. 192, 1899, A, p. 245.

each other. Remove one to a very great distance, doing work against their mutual attractions. Then, when it is quite out of range of appreciable action, turn it round till its axis is parallel to that of the fixed crystal. This absorbs no work if done slowly. Then let it return. The force on the return journey at every point is greater than the force on the outgoing journey, and more work will be got out than was put in. When the sphere is in its first position, turn it round till the axes are again at right angles. Then work must be done on turning it through this right angle to supply the difference between the outgoing and incoming works. For if no work were done in the turning, we could go through cycle after cycle, always getting a balance of energy over, and this would, I think, imply either a cooling of the crystals or a diminution in their weight, neither supposition being admissible. We are led, then, to say that if the attraction with parallel axes exceeds that with crossed axes, there must be a directive action resisting the turn from the crossed to the parallel positions. And conversely, a directive action implies axial variation in gravitation.

The straightforward mode of testing the existence of this directive action would consist in hanging up one sphere by a wire or thread, and turning the other round into various positions, and observing whether the hanging sphere tended to twist out of position. But the action, if it exists, is so minute, and the disturbances due to air currents are so great, that it would be extremely difficult to observe its effect directly. It occurred to us that we might call in the aid of the principle of forced oscillations, by turning one sphere round and round at a constant rate, so that the couple would act first in one direction and then in the other, alternately, and so set the hanging sphere vibrating to and fro. The nearer the complete time of vibration of the applied couple to the natural time of vibration of the hanging sphere, the greater would be the vibration set up. This is well illustrated by moving the point of suspension of a pendulum to and fro in gradually decreasing periods, when the swing gets longer and longer, till the period is that of the pendulum, and then decreases again. Or by the experiment of varying the length of a jar resounding to a given fork, when the sound suddenly swells out as the length becomes that which would naturally give the same note as the fork. Now, in looking for the couple between the crystals, there are two possible cases. The most likely is that in which the couple acts in one way while the turning sphere is moving from parallel to crossed, and in the opposite way during the next quarter turn from crossed to parallel. That is, the couple vanishes four times during the revolution, and this we may term a quadrantal couple. But it is just possible that a quartz crystal has two ends like a magnet, and that like poles tend to like directions. Then the couple will vanish only twice in a revolution, and may be termed a semicircular couple. We looked for both, but it is enough now to consider the possibility of the quadrantal couple only.

Our mode of working will be seen from Fig. 9. The hanging sphere, ·9 cm. in diameter and 1 gm. in weight, was placed in a light aluminium wire cage with a mirror on it, and suspended by a long

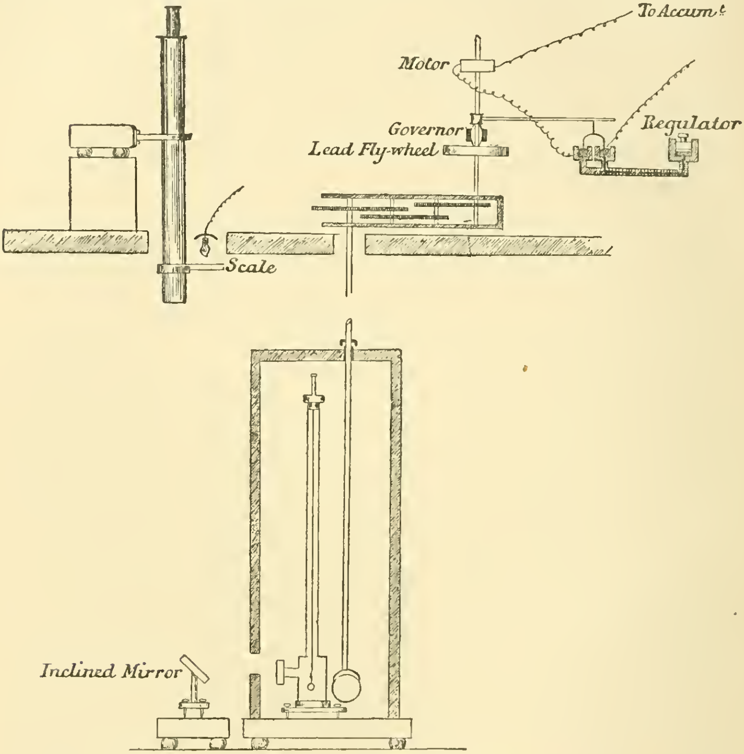

Fig. 9.—Experiment on directive action of one quartz crystal on another.

quartz fibre in a brass case with a window in it opposite the mirror, and surrounded by a double-walled tinfoiled wood case. The position of the sphere was read in the usual way by scale and telescope. The time of swing of this little sphere was 120 seconds.

A larger quartz sphere 6·6 cm. diameter and weighing 400 gms., was fixed at the lower end of an axis which could be turned at any desired rate by a regulated motor. The centres of the spheres were on the same level and 5·9 cm. apart. On the top of the axis was a wheel with 20 equidistant marks on its rim, one passing a fixed point every 11·5 seconds.

It might be expected that the couple, if it existed, would have the greatest effect if its period exactly coincided with the 120 second

period of the hanging sphere—i. e. if the larger sphere revolved in 240 seconds. But in the conditions of the experiment the vibrations of the small sphere were very much damped, and the forced oscillations did not mount up as they would in a freer swing. The disturbances, which were mostly of an impulsive kind, continually set the hanging sphere into large vibration, and these might easily be taken as due to the revolving sphere. In fact, looking for the couple with exactly coincident periods would be something like trying to find if

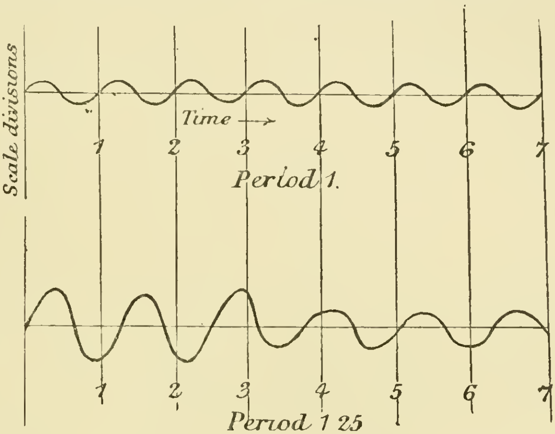

Fig. 10.—Upper curve a regular vibration. Lower curve a disturbance dying away.

a fork set the air in a resonating jar vibrating when a brass band was playing all round it. It was necessary to make the couple period, then, a little different from the natural 120 second period, and, accordingly, we revolved the large sphere once in 230 seconds, when the supposed quadrantal couple would have a period of 115 seconds.

Figs. 10 and 11 may help to show how this enabled us to eliminate the disturbances. Let the ordinates of the curves in Fig. 10 represent vibrations set out to a horizontal time scale. The upper curve is a regular vibration of range $\pm$ 3, the lower a disturbance beginning with range $\pm$ 10. The first has period 1, the second period 1·25. Now cutting the curves into lengths equal to the period of the shorter time of vibration, and arranging the lengths one under the other as in Fig. 11, it will be seen that the maxima and the minima of the regular vibration always fall at the same points, so that, taking 7 periods and adding up the ordinates, we get 7 times the range, viz. $\pm$ 21. But in the disturbance the maxima and minima fall at different points, and even with 7 periods only, the range is from $+$ 16 to $-$ 13, or less than the range due to the addition of the much smaller regular vibration.

In our experiment, the couple, if it existed, would very soon establish its vibration, which would always be there and would go through all its values in 115 seconds. An observer, watching the wheel at the top of the revolving axis, gave the time signals every 11·5 seconds, regulating the speed, if necessary, and an observer at the telescope gave the scale reading at every signal, that is, 10 times during the period. The values were arranged in 10 columns, each horizontal line giving the readings of a period. The experiment was carried on for about $2\frac{1}{2}$ hours at a time, covering, say, 80 periods. On adding up the columns, the maxima and minima of the couple effect would always fall in the same two columns, and so the addition would give 80 times the swing, while the maxima and minima of the

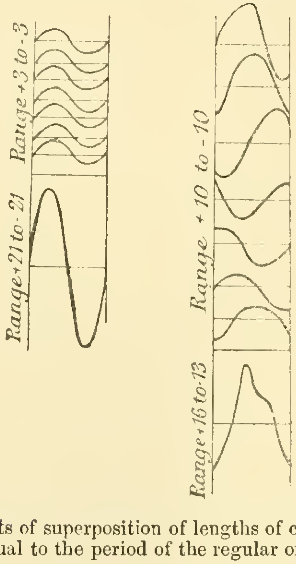

FIG. 11.—Results of superposition of lengths of curves in Fig. 10 equal to the period of the regular one.

natural swings due to disturbances would fall in different columns, and so, in the long run, neutralise each other. The results of different days' work might, of course, be added together.

There always was a small outstanding effect such as would be produced by a quadrantal couple, but its effect was not always in the same columns, and the net result of about 350 period observations was that there was no 115 second vibration of more than 1 second of arc, while the disturbances were sometimes 50 times as great.

The semicircular couple required the turning sphere to revolve in 115 seconds. Here, want of symmetry in the apparatus would come in with the same effect as the couple sought, and the outstanding result was, accordingly, a little larger.

But in neither case could the experiments be taken as showing a real couple. They only showed that, if it existed, it was incapable of producing an effect greater than that observed.

Perhaps the best way to put the result of our work is this: Imagine the small sphere set with its axis at 45° to that of the other. Then the couple is not greater than one which would take $5\frac{1}{4}$ hours to turn it through that 45° to the parallel position, and it would oscillate about that position in not less than 21 hours.

The semicircular couple is not greater than one which would turn from crossed to parallel position in $4\frac{1}{2}$ hours, and it would oscillate about that position in not less than 17 hours.

Or, if the gravitation is less in the crossed than in the parallel position, and in a constant ratio, the difference is less than 1 in 16,000 in the one case and less than 1 in 2800 in the other.

We may compare with these numbers the difference of rate of travel of yellow light through a quartz crystal along the axis and perpendicular to it. That difference is of quite another order, being about 1 in 170.

As to other possible qualities of gravitation, I shall only mention that quite indecisive experiments have been made to seek for an alteration of mass on chemical combination,* and that at present there is no reason to suppose that temperature affects gravitation. Indeed, as to temperature effect, the agreement of weight methods and volume methods of measuring expansion with rise of temperature is good, as far as it goes, in showing that weight is independent of temperature.

So while the experiments to determine G are converging on the same value, the attempts to show that, under certain conditions, it may not be constant, have resulted so far in failure all along the line. No attack on gravitation has succeeded in showing that it is related to anything but the masses of the attracting and the attracted bodies. It appears to have no relation to physical or chemical condition of the acting masses or to the intervening medium.

Perhaps we have been led astray by false analogies in some of our questions. Some of the qualities we have sought and failed to find, qualities which characterise electric and magnetic forces, may be due to the polarity, the + and −, which we ascribe to poles and charges, and which have no counterpart in mass.

But this unlikeness, this independence of gravitation of any quality but mass, bars the way to any explanation of its nature.

The dependence of electric forces on the medium, one of Faraday's grand discoveries for ever associated with the Royal Institution, was the first step which led on to the electromagnetic theory of light now so splendidly illustrated by Hertz's electromagnetic waves. The quantitative laws of electrolysis, again due

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* Landolt, Zeit. für Phys. Chem. xii. 1, 1894. Sanford and Ray, Physical Review, v. 1897, p. 247.

to Faraday, are leading, I believe, to the identification of electrification and chemical separation, to the identification of electric with chemical energy.

But gravitation still stands alone. The isolation which Faraday sought to break down is still complete. Yet the work I have been describing is not all failure. We at least know something in knowing what qualities gravitation does not possess, and when the time shall come for explanation all these laborious and, at first sight, useless experiments will take their place in the foundation on which that explanation will be built.

[J. H. P.]

WEEKLY EVENING MEETING,

Friday, March 2, 1900.

His Grace The Duke of Northumberland, K.G. F.S.A., President, in the Chair.

Major Ronald Ross, D.P.H. M.R.C.S.

Malaria and Mosquitoes.

Our knowledge of the disease called malarial fever first emerges from chaos in the seventeenth century, when, owing to the recent discovery of quinine, the great Italian physician, Torti, was able to differentiate this malady from other fevers, and to describe its symptoms with accuracy. Next century, Morton, Lancisi, Pringle and others observed the connection of the disease with stagnant water and low-lying ground, and first emitted the theory—which in one form or another has found general acceptance up to the present date—that the fever is due to a miasm which rises from the soil or water of malarious localities. The next great advance was made in the middle of the nineteenth century by Meckel, Virchow and Frerichs, who ascertained that the distinguishing pathological product of the disease is a black substance, which is distributed in collections of minute coal-black or brown granules in the blood and organs of patients, and which is called the *malarial pigment* or *melanin.* This line of research culminated in the great discovery of Laveran in 1880—to the effect that the melanin is produced within the bodies of vast numbers of minute parasites which live in the red blood-corpuscles of the patient.

Ray Lankester had already opened the science of the parasitology of the blood-corpuscle by his discovery of *Drepanidium ranarum* in frogs; and it was at once apparent that the parasites found by these two observers are somewhat nearly allied—that is, that Laveran's parasite is a *Protozoal* organism, and not a vegetable one like the pathogenetic organisms recently discovered by Pasteur, Lister, Koch and many others. And our knowledge of the subject was quickly increased by the discovery of similar hæmatozoa in certain species of reptiles, birds, monkeys and bats, and in cattle, by Danilewsky, Kruse, Labbé, Koch, Dionisi, Smith and Kilborne. In 1885 a further advance was made by Golgi, who ascertained that the human parasites propagate within the body of the host by means of ordinary asexual spore-formation; that the exacerbations of fever in a patient are coincident with the disruption of the clusters of spores produced by the organisms; and that there are at least three varieties of the parasites in man in Italy.

These observations were confirmed and extended by a large number of persons working in various parts of the world—most prominent among whom are Marchiafava, Celli, Vandyke Carter, Grassi, Osler, Bignami, Antolisei, Councilman, Mannaberg, Romanowsky, Labbé, Koch, Manson, Thayer and MacCallum. In short, the work of all these observers, and of many others scarcely less meritorious, has not only absolutely established the fact that the parasites are the cause of malarial fever, but has given us a very thorough knowledge both of the parasites themselves and of their pathological effects, direct and indirect; until the science of malaria—for it may almost be described as a science in itself—has become a brilliant exemplar of the modern methods of research as regards the science of disease in general.

But I am not here concerned with questions of pathology in malarial fever. At the conclusion of the labours to which I have just referred, we had, it is true, grasped the nature of the disease itself; but a question of the greatest moment still required an answer. We had studied side by side the morbid process and the parasites which cause it; but we had still to find out how infection is caused, how these parasites effect an entry. We had ascertained the life-history of the parasites within man, and of the kindred parasites within other animals; but, even after all these investigations, the life-history of the parasites *outside* man and *outside* other vertebrate hosts remained to be discovered. Until this was done our knowledge was not complete. It is now my privilege to describe the interesting theories and investigations which led to the solution of this great and difficult problem.

The *importance* of the problem need not be enlarged upon. In the British army in India during the year 1897, out of a total strength of 178,197 men, no less than 75,821 were admitted into hospital for malarial fever! Fortunately the death-rate of the disease is low in most places; but on the other hand the cases are so numerous that in the aggregate the mortality from malarial fever is very large indeed. For instance, in India alone, among the civil population (who do not take adequate treatment), the mortality from "fevers" during the single year 1897 amounted to the enormous total of 5,026,725—over five million deaths—being nearly ten times that due to any other disease. Although undoubtedly thousands of deaths are wrongly attributed to fever in these statistics, such figures can point only to a very great mortality due to malaria. Yet India on the whole is not nearly so malarious as many localities—such, for instance, as places on the coasts of Africa. In short, next perhaps to tuberculosis, malarial fever is admittedly the most important of human diseases.

But if the problem to which I refer was an important one, its solution presented difficulties which I, for one, formerly thought to be insuperable. It has been mentioned that Lancisi and Pringle connected the disease with stagnant water; and their views have

been generally endorsed by innumerable observations made since their time—by the general experience of mankind, by statistics, and by the fact that malaria can often be actually banished by means of drainage of the soil. But Laveran had now shown the disease to be due to a parasite of the blood. How reconcile these facts? There appeared to be but one way of doing so—namely, by supposing that the organism lives a free life in the water or soil of malarious places, from which it enters man by the respiratory or digestive tracts. To prove this it was necessary to discover it in the water or soil of malarious places. But how make this discovery? The organism is not a bacterium, but an animal parasite. It cannot be taken from the living blood and sown on the surface of a gelatine film. Experiments have proved that it can be inoculated from man to man by the intravenous injection of fresh infected blood; but this is a very different thing to cultivating it in an artificial medium. At all events, experiments in this line have always failed, and are not in the least likely to succeed. The parasites simply perish when taken from their natural habitation, the blood. It was therefore extremely unlikely that we should ever be able to follow up their life-history by this means—which has proved so successful as regards the bacteria. It remained only to find them in the soil or water by direct search. But how identify them among the host of Protozoa which live in these elements? Certainly not by their form or appearance. As known to us at that time, they were simply minute amœbæ ensconced in the red corpuscles and accurately adapted for such a life. Now red corpuscles do not exist in soil and water; if the parasites live in the latter, they must possess some other form to that which they possess in the blood, and the clue afforded by identity of appearance fails us. The only remaining method open to us would have been to attempt to produce infection by each one in turn of the numerous species of Protozoa found in the water and soil of malarious places—a task of great magnitude, and one which we now know would have failed. Indeed, it was actually attempted by several observers, and actually did fail.

Such was the state of things up to the end of the year 1894. Speaking for myself, I can well remember the hopeless feelings with which I then regarded the problem. Fortune, however, was to be kinder to us than I had dared believe. At this very moment the key to the solution of the problem had already been indicated by Dr. Patrick Manson.

I have said that since the original discovery of Ray Lankester, numerous hæmatozoa—or rather hæmocytozoa—have been found in man and various animals. All these are generally classed by zoologists in Leuckart's order of the Sporozoa, and are usually divided into three groups—groups which are not very closely related, except for the fact that all the organisms concerned are parasites of the red corpuscles of the blood. One group—found in reptiles—consists of parasites closely allied to the Gregarinidæ, another is found in oxen,

and is the cause of Texas cattle-fever; the third—for which I adopt the name of Hæmamœbidæ Wassielewski—is found in man, monkeys, bats and birds. It is to this third group—the Hæmamœbidæ—to which we must now direct our attention, because it includes the parasites of malarial fever. There are at least two known species found in birds, two in bats, one in monkeys, and three in man. The human parasites are those which respectively cause the three varieties of malarial fever—quartan, tertian, and remittent or pernicious fever. For these three species I adopt the names *Hæmamœba malariæ* (quartan), *Hæmamœba vivax* (tertian), and *Hæmomenas præcox* (remittent fever).* According to Metchnikoff the group belongs, or is allied, to the Coccidiidæ. All the species have a close resemblance to each other, and all contain the typical melanin of malarial fever. The youngest parasites are found as minute *amœbulæ* living within the red corpuscle and generally containing granules of this melanin (which, indeed, is derived by the parasite from the hæmoglobin of the corpuscle within which it makes its abode). The amœbulæ grow rapidly in size, until, after one or more days (according to the species) they reach maturity. At this point many of them become *sporocytes*—that is, give rise to ordinary spores by vegetative reproduction. These spores presently attach themselves to fresh corpuscles, become fresh amœbulæ, and so continue the life of the parasites indefinitely within the vertebrate host. Others of the amœbulæ, however, instead of becoming sporocytes like the rest, become *gametocytes.*

Now it is to these gametocytes that an extreme interest attaches, because it is to them, and to Manson's study of them, that we owe the solution of the malarial problem. Numerous observers had examined them before Manson's time, but all had failed in arriving at a correct idea as to their function. It had been often observed that they circulate in the blood of the vertebrate hosts without apparently performing any function at all. As soon, however, as they are drawn from the circulation—as when the blood containing them is made into a fresh specimen for microscopic examination—they undergo the most remarkable changes. They swell up and liberate themselves from the enclosing corpuscle; and then some of them are suddenly seen to emit a number of long *motile filaments.* These filaments can easily be watched struggling violently, and may sometimes be seen to break from the parent cell and to dart away among the corpuscles, leaving the residue of the gametocyte, with its melanin, an inert and apparently dead mass.

Now it is not to be supposed that such an extraordinary phenomenon as this—which was observed by Laveran during his first investigations—could be witnessed without exciting the liveliest curiosity. As a matter of fact a hot controversy rose regarding it. Laveran, Danilewsky and Mannaberg maintained that the phenomenon is a vital one—that the motile filaments are living organisms, and

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* Nature, August 3, 1899.

constitute a stage in the history of the parasite. Antolisei, Grassi, Bignami, and others of the Italian school, fell back upon the old theory —which we always like to employ when we cannot explain a phenomenon—that it is a regressive phenomenon, a disintegration of the parasite due to its death *in vitro.* Here, however, the controversy practically stayed. While the Italians, in conformity with their views, attached no signification to the motile filaments, Laveran, Danilewsky and Mannaberg, who held an opposite opinion, did not expressly or exactly state what their signification is. Mannaberg, indeed, held that they are meant to lead a saprophytic existence, but did not explain how they could escape from the body in order to do so.

It was reserved for Manson to detect the ultimate (though not the immediate) function of these bodies. He asked why the escape of the motile filaments occurs only after the blood is abstracted from the host (a fact agreed upon by many observers). From his study of these filaments, of their form and their characteristic movements, he rejected the Italian view that they are regressive forms; he was convinced that they are living elements. Hence he felt that the fact of their appearance only *after* abstraction of the blood (about fifteen minutes afterwards) must have some definite purpose in the life-scheme of the parasites. What is that purpose? It is evident that these parasites like all others must pass from host to host; all known parasites are capable not only of entering the host, but, either in themselves or their progeny, of leaving him. Manson himself had already pushed such methods of inductive reasoning to a brilliantly successful issue in discovering by their means the development of *Filaria nocturna* in the gnat. He now applied the same methods to the study of the parasites of malaria. Why should the motile filaments appear only after abstraction of the blood? There could be only one explanation. The phenomenon, though it is usually observed in a preparation for the microscope, is really meant to occur *within the stomach cavity of some suctorial insect, and constitutes the first step in the life-history of the parasite outside the vertebrate host.*

It is perhaps impossible for any one, except one who has spent years in revolving this subject, to understand the full value and force of this remarkable induction. To my mind the reasoning is complete and exigent. It was from the first impossible to consider the subject in the light in which Manson placed it without feeling convinced that the parasite requires a suctorial insect for its further development. And subsequent events have proved Manson to have been right.

The most evident reasoning—the connection between malarial fever and low-lying water-logged areas in warm countries—suggested at once that the suctorial insect must be the *gnat* (called *mosquito* in the tropics); and this view was fortified by numerous analogies which must occur at once to any one who considers the subject at all and which it is not necessary to discuss in this place.

Needless to say, since Manson's theory was proved to be right, it

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has been shown to be not entirely original. Nuttall, in his admirable history of the mosquito theory, demonstrates its antiquity. Eleven years before Manson wrote, King had already accumulated much evidence, based on epidemiological data, in favour of the theory. A year later (1884) Laveran himself briefly enunciated the same views, on the analogy with *Filaria nocturna*. Koch, and later, Bignami and Mendini, were also advocates of the theory—partly on epidemiological grounds and partly because of a possible analogy with the protozoal parasites of Texas cattle-fever which Smith and Kilborne had shown to be carried by a *tick*. Hence many observers had independently arrived at the same theory by different routes. But I feel it most necessary to point out here that there is a difference between a fortunate guess and a true scientific theory. Interesting and suggestive as were many of the hypotheses to which I have just referred, they were to my mind far from convincing. *Filaria nocturna*, and even *Apiosoma bigeminum*, are not in close enough relationship with the Hæmamœbidæ to admit of very forcible analogies in regard to the respective life-histories. The epidemiological arguments of King and Bignami (some of which were also used by Manson) were scarcely solid enough to support by themselves a theory of any weight. All these were hypotheses—little more: I can scarcely conceive a practical man sitting down to laborious researches on the strength of arguments like these. On the other hand, Manson's theory was what I have called it—an *induction*—a chain of reasoning from which it was impossible to escape.

I have wished to defend this work of Manson's because it has been much misunderstood and much misrepresented, and even (in a somewhat amusing manner) completely ignored by some who, though they once strongly opposed his theory, now, as soon as it has done its work, wished to forget it. It is true that he endeavoured to predict the history of the parasites a little too far, and that he was in error (as will presently appear) regarding the immediate nature of the motile filaments; but the core of his theory was invaluable. I have no hesitation in saying that it was Manson's theory, and no other, which actually solved the problem; and to be frank, I am equally certain that but for Manson's theory the problem would have remained unsolved at the present day.

Dr. Laveran's theory was unfortunately enunciated with great brevity; but it appears to me to have been really founded on many if not all the arguments independently advanced by King and Manson. To him we owe not only the discovery which made all these researches possible; but also an early and correct prediction as to the future life-history of the organisms with which his name will be inseparably connected.

To leave these interesting theories and to return to actual observations—I should begin by remarking that Manson thought the motile filaments to be of the nature of zoospores—that is, motile spores which escape from the gametocytes in the stomach cavity of the gnat,

and then occupy and infect the tissues of the insect. In this he was proved, two years later, to have been wrong. The motile filaments are not spores, but *microgametes*—that is, bodies of the nature of spermatozoa. I have said that some of the amœbulæ in the blood-corpuscles of the host become sporocytes, which produce asexual spores (nomospores); while other amœbulæ become gametocytes, which have no function within the vertebrate host. As soon, however, as these gametocytes are ingested by a suctorial insect they commence their proper functions. As their name indicates, they are sexual cells—male and female. About fifteen minutes after ingestion (in some species), the male gametocyte emits a variable number of microgametes—the motile filaments—which presently escape and wander in search of the female gametocytes. These contain a single *macrogamete* or ovum, which is now fertilised by one of the microgametes, and becomes a *zygote.* We owe this beautiful discovery to the direct observation of MacCallum (1897), confirmed by Koch and Marchoux, and indirectly by Bignami. Metchnikoff, Simond, Schaudinn and Siedlecki have also demonstrated what are practically sexual elements in some of the Coccidiidæ. Directly MacCallum's discovery was announced Manson saw the important bearing of it on the mosquito theory. Admitting that the motile filaments themselves do not infect the gnat, he at once observed that it was probably the function of the zygote to do so—and this time he was perfectly right.

I must now turn to my own researches. Dr. Manson told me of his theory at the end of 1884, and I then undertook to investigate the subject as far as possible. I began work in Secunderabad, India, in April 1895; and should take the present opportunity for acknowledging the continuous assistance which I received both from Dr. Manson and from Dr. Laveran, and later from the Government of India. Even with the aid of the induction, the task so lightly commenced was, as a matter of fact, one of so arduous a nature that we must attribute its accomplishment largely to good fortune. The method adopted—the only method which could be adopted—was to feed gnats of various species on persons whose blood contained the gametocytes, and then to examine the insects carefully for the parasites which by hypothesis the gametocytes were expected to develop into. This required not only familiarity with the histology of gnats, but a laborious search for a minute organism throughout the whole tissues of each individual insect examined—a work of at least two or three hours for each gnat. But the actual labour involved was the smallest part of the difficulty. Both the form and appearance of the object which I was in search of, and the species of the gnat in which I might expect to find it, were absolutely unknown quantities. We could make no attempt to predict the appearance which the parasite would assume in the gnat; while owing to the general distribution of malarial fever in India, the species of insect concerned in the propagation of the disease could scarcely be

determined by a comparison of the prevalence of different kinds of gnat at different spots with the prevalence of fever at those spots. In short, I was forced to rely simply on the careful examination of hundreds of gnats, first of one species and then of another, all fed on patients suffering from malarial fever—in the hope of one day finding the clue I was in search of. Needless to say, nothing but the most convincing theory, such as Manson's theory was, would have supported or justified so difficult an enterprise.

As a matter of fact, for nearly two and a half years, my results were almost entirely negative. I could not obtain the correct scientific names of the various species of gnats employed by me in these researches, and consequently used names of my own. Gnats of the genus *Culex* (which abound almost everywhere in India) I called "grey" and "brindled" mosquitoes; and it was these insects which I studied during the period I refer to. At last, the persistently nugatory results which had been obtained with gnats of this genus determined me to try other methods. I went to a very malarious locality, called the Sigur Ghat, near Ootacamund, and examined the mosquitoes there in the hope of finding within them parasites like those of malaria in man. The results were practically worthless (except that I observed a new kind of mosquito with spotted wings); and I saw that I must return to the exact method laid down by Manson. The experiments with the two commonest kinds of Culex were once more repeated—only to prove once more negative. The insects, fed mostly on cases containing the crescentic gametocytes of *Hæmomenas præcox*, were examined cell by cell—not even their excrement being neglected. Although they were known to have swallowed living Hæmamœbidæ, no living parasites like these could be detected in their tissues—the ingested Hæmamœbidæ had in fact perished in the stomach cavity of the insects. I began to ask whether after all there was not some flaw in Manson's induction; but no—I still felt his conclusion to be an inevitable one. And it was at this very moment that good fortune gave me what I was in search of.

In a collecting bottle full of larvæ brought by a native from an unknown source, I found a number of newly-hatched mosquitoes like those first observed by me in the Sigur Ghat—namely, mosquitoes with *spotted wings* and *boat-shaped eggs*. Eight of these were fed on a patient whose blood contained crescentic gametocytes. Unfortunately I dissected six of them either prematurely or otherwise unsatisfactorily. The seventh was examined, on August 20, 1897, cell by cell; the tissues of the stomach (which was now empty owing to the meal of malarial blood taken by the insect four days previously being digested) were reserved to the last. On turning to this organ I was struck by observing, scattered on its outer surface, certain oval or round cells of about two to three times the diameter of a red blood-corpuscle—cells which I had never before seen in any of the hundreds of mosquitoes examined by me. My surprise was complete when I

next detected within each of these cells *a few granules of the characteristic coal-black melanin of malarial fever*—a substance quite unlike anything usually found in mosquitoes. Next day the last of the remaining spotted-winged mosquitoes was dissected. It contained precisely similar cells, each of which possessed the same melanin; only the cells in the second mosquito *were somewhat larger than those in the first.*

These fortunate observations practically solved the malaria problem. As a matter of fact, the cells were the *zygotes of the parasite of remittent fever growing in the tissues of the gnat;* and the gnat with spotted wings and boat-shaped eggs in which I had found them belonged (as I subsequently ascertained) to the genus *Anopheles.* Of course it was impossible absolutely to prove at the time, on the strength of these two observations alone, that the cells found by me in the gnats were indeed derived from Hæmamœbidæ sucked up by the insects in the blood of the patients on whom they had been fed—this proof was obtained by subsequent investigations of mine; but, guided by the presence of the typical and almost unique melanin in the cells, and by numerous other circumstances, I myself had no doubt of the fact. The clue was obtained; it was necessary only to follow it up—an easy matter.

The preparations of the stomachs of the two *Anopheles* were sealed, and were afterwards examined by Drs. Smyth, Manson, Thin and Bland-Sutton; and an account of the work, and of the observations of these gentlemen, was published a little later. Unfortunately, my labours now met with a serious interruption; but not before I had succeeded again in finding the zygotes in two other mosquitoes—one, another species of *Anopheles*, also bred from the larva, and also fed on a case containing crescentic gametocytes; the other, a "grey mosquito" (*Culex pipiens* type), which had been caught feeding on a case of tertian fever, and which I now think had become previously infected from a bird with *Hæmamœba relicta.*

Early in 1898, mainly though the influence of Dr. Manson, Sir H. W. Bliss and the United Planters' Association of Southern India, I was placed by the Government of India on special duty in Calcutta to continue my investigations. Unable to work with human malaria—chiefly on account of the plague scare in Calcutta—I turned my attention to the Hæmamœbidæ of birds. Birds have at least two species of Hæmamœbidæ. I subjected a number of birds containing one or the other of these parasites to the bites of various species of mosquitoes. The result was a repetition of that previously obtained with the human parasites. Pigmented cells precisely similar to those seen in the *Anopheles* were found to appear in gnats of the species called *Culex fatigans*, Wiedemann, when these had been fed on sparrows and larks containing *Hæmamœba relicta.* On the other hand, these cells were never found in insects of the same species when fed on healthy birds or on birds containing the other parasite, called *Hæmamœba danilewskii.*

It will be evident that this fact was the crucial test both as regards the parasitic nature of these cells and as regards their development from the hæmocytozoa of the birds; and it was not accepted by me without very close and laborious experiment.

The actual results obtained were as follows: Out of 245 *Culex fatigans* fed on birds containing *H. relicta*, 178, or 72 per cent., contained "pigmented cells." But, out of 41 *Culex fatigans* fed on a man containing crescentic gametocytes, 5 on a man containing immature tertian parasites, 154 on birds containing *H. danilewskii*, 25 on healthy sparrows, and 24 on birds with immature *H. relicta*—or a total of 249 insects, all carefully examined—not one contained a single "pigmented cell."

Another experiment was as follows: Three sparrows, one containing no parasites, another containing a moderate number of *H. relicta*, and the third containing numerous *H. relicta*, were placed in separate cages within three separate mosquito curtains. A number of *culex fatigans*, all bred simultaneously from larvæ in the same breeding bottle, were now liberated on the same evening partly within the first mosquito netting, partly within the second, and partly within the third. Next morning many of these gnats were found to have fed themselves on the birds during the night. Ten of each lot of gnats were dissected after a few days, with the following result: The ten gnats fed on the healthy sparrow contained no "pigmented cells." The ten gnats fed on the sparrow with a moderate number of parasites were found to contain altogether 292 "pigmented cells"; or an average of twenty-nine in each gnat. The ten gnats fed on the sparrow with numerous parasites, contained 1009 "pigmented cells"; or an average of 100 cells in each gnat. These thirty specimens were sent to Manson in England, who made a similar count of the cells.

I may mention one more out of several experiments of the same kind: A stock of *Culex fatigans*, all bred from the larva, were fed on the same night partly on two sparrows containing *H. relicta*, and partly on a crow containing *H. danilewskii* (placed, of course, under separate mosquito-nettings). Out of twenty-three of the former lot, twenty-two were found to have pigmented cells; while out of sixteen of the latter, none had them.

Hence no doubt remained that the "pigmented cells," really constitute a developmental stage in the mosquito of these parasites; and this view was accepted both by Laveran and Manson, to whom specimens had been sent. In June 1898, Manson published an illustrated paper concerning my researches, and showed that the pigmented cells must in fact be the zygotes resulting from the process of fertilisation discovered by MacCallum.

It remained to follow out the life-history of the zygotes. For this purpose it was immaterial whether I worked with the avian or the human parasites, since these are so extremely like each other. I elected to work with the avian species, chiefly because the plague-

scare in Bengal still rendered observations with the human species almost impossible. By feeding *Culex fatigans* on birds with *H. relicta* and then examining the insects one, two, three and more days afterwards, it was easy to trace the gradual growth of the zygotes. Their development briefly is as follows: After the fertilisation of the macrogamete has taken place in the stomach-cavity of the gnat, the fertilised parasite or zygote has the power of working its way through the mass of blood contained in the stomach, of penetrating the wall of the organ, and of affixing itself on, or just under, its *outer coat.* Here it first appears about thirty-six hours after the insect was fed, and is found as a "pigmented cell"—that is, a little oval body, about the size of a large red corpuscle or larger, and containing the granules of melanin possessed by the parent gametocyte from which the macrogamete originally proceeded. In this position it shows no sign of movement, but begins to grow rapidly, to acquire a thickened capsule, and to project from the outer wall of the stomach, to which it is attached, into the *body cavity* of the insect host. At the end of six days, if the temperature of the air be sufficiently high (about 80° F.), the diameter of the zygote has increased to about eight times what it was at first; that is, to about 60 μ. If the stomach of an infected insect be extracted at this stage, it can be seen, by a lower power of the microscope, to be studded with a number of attached spheres, which have something of the appearance of warts on a finger. These are the large zygotes, which have now reached maturity and which project prominently into the mosquito's body-cavity.

All this could be ascertained with facility by the method I have mentioned; and it should be understood that gnats can be kept alive for weeks or even months by feeding them every few days on blood—or, as Bancroft does, on bananas. But a most important point still required study. What happens after the zygotes reach maturity? I found that each zygote as it increases in size divides into *meres*, each of which next becomes a *blastophore*, carrying a number of *blasts* attached to its surface. Finally the blastophore vanishes, leaving the thick capsule of the zygote packed with thousands of the blasts. The capsule now *ruptures*, and allows the blasts to escape into the body fluids of the insect.

These blasts, when mature, are seen to be minute filamentous bodies, about 12–16 μ in length, of extreme delicacy, and somewhat spindle-shaped—that is, tapering at each extremity. Just as the zygotes recall the shape of the Coccidiidæ, so do these blasts recall the "falciform bodies." Prof. Herdman and I have adopted this word "blast" for these bodies after careful consideration—but others prefer other names. They are, of course, *spores*, but spores which have been produced by a previous sexual process—and are, in fact the result of a kind of *polyembryony*. Just as a fertilised ovum gives rise to blasts which produce the cluster of cells constituting a multicellular animal, so, in this case the fertilised ovum or zygote gives rise to blasts, each of which, however, becomes a separate animal.

Prof. Ray Lankester suggests for the blasts of the Hæmamœbidæ the simple term "filiform young."

At this point the investigations took a turn of extreme interest and importance, scarcely second even to what attached to the first study of the zygotes. Since the blasts are evidently the progeny of the zygotes, they must carry on the life-history of the parasites to a further stage. How do they do so? What is their function? Do they escape from the mosquito, and in some manner, direct or indirect, set up infection in healthy men and birds? Or, if not, what other purpose do they subserve? It was evident that our knowledge of the mode of infection in malarial fever—and perhaps even the prevention of the disease—depended on a reply to these questions.

As I have said, the zygotes become ripe and rupture about a week after the insect was first infected—scattering the blasts into the body-cavity of the host. What happens next? It was next seen that by some process, apparently owing to the circulation of the insect's body-fluids (for the blasts themselves appear to be almost without movement), these little bodies find their way into every part of the mosquito—into the juices of its head, thorax, and even legs. Beyond this it was difficult to go. All theory—at least all theory which I felt I could depend upon—had been long left behind, and I could rely only on direct observation. Gnat after gnat was sacrificed in the attempt to follow these bodies. At last, while examining the head and thorax of one insect, I found a large gland consisting of a central duct surrounded by large grape-like cells. My astonishment was great when I found that many of these cells were closely packed with the blasts—(which I may add are not in the least like any normal structures in the mosquito). Now I did not know at that time what this gland is. It was speedily found, however, to be a large racemose gland consisting of six lobes, three lying in each side of the insect's neck. The ducts of the lobes finally unite in a common channel which runs along the under surface of the head and *enters the middle stylet, or lancet, of the insect's proboscis.*

It was impossible to avoid the obvious conclusion. Observation after observation always showed that the blasts collect within the cells of this gland. It is the *salivary* or *poison* gland of the insect, similar to the salivary gland found in many insects, the function of which, in the gnat, had already been discovered—although I was not aware of the fact. That function is to *secrete the fluid which is injected by the insect when it punctures the skin*—the fluid which causes the well-known irritation of the puncture, and which is probably meant to prevent either the contraction of the torn capillaries or the coagulation of the ingested blood. The position of the blasts in the cells of this gland could have only one interpretation—wonderful as that interpretation is. The blasts must evidently pass down the ducts of the salivary gland into the wound made by the proboscis of the insect, and *thus causes infection in a fresh vertebrate host.*

That this actually happens could, fortunately, be proved without any difficulty. As I had now been studying the parasites of birds for some months, I possessed a number of birds of different species, the blood of which I had examined from time to time (by pricking the toes with a fine needle). Some of them were infected, and some, of course, were not. Out of 111 wild sparrows examined by me in Calcutta, I found *H. relicta*—the parasite which I had just cultivated in *Culex fatigans*—in 15, or 13·5 per cent. As a rule, non-infected birds were released; but I generally kept a few to use for the control experiments mentioned above, and the blood of these birds had consequently been examined on several occasions, and had always been found free from parasites. At the end of June I possessed five of these healthy control birds—four sparrows and one weaver-bird. All of them were now carefully examined again and found to be healthy. They were placed in their cages within mosquito-nets, and at the same time a large stock of old infected mosquitoes were released within the same nets. By "old infected mosquitoes" I mean mosquitoes which had been previously fed repeatedly on infected birds, and many of which on dissection had been shown to have very large numbers of blasts in their salivary glands. Next morning, numbers of these infected gnats were found gorged with blood, proving that they had indeed bitten the healthy birds during the night. The operation was repeated on several succeeding nights, until each bird had probably been bitten by at least a dozen of the mosquitoes. On July 9, the blood of the birds was examined again. I scarcely expected any result so complete and decisive. Every one of the five birds was now found to contain parasites—and not merely to contain them, but to possess such immense numbers of them as I had never before seen in any bird (with *H. relicta*) in India. While wild sparrows in Calcutta seldom contain more than one parasite in every field of the microscope, those which I had just succeeded in infecting contained, ten, fifteen, twenty and even more in each field—a fact due probably to the infecting gnats having been previously fed over and over again on infected birds, a thing which can rarely happen in nature.

The experiment was repeated many times—generally on two or three healthy birds put together. But I now improved on the original experiment by also employing controls in the following manner. A stock of wild sparrows would be examined, and the infected birds eliminated. The remainder would then be kept apart, and at night would be carefully secluded from the bites of gnats by being placed within mosquito nets. These constituted my stock of healthy birds. From time to time two or three of these would be separated, examined again to ensure their being absolutely free from parasites, and then subjected to the bites of "old infected mosquitoes," and, of course, kept apart afterwards for daily study. Thus my stock of healthy birds was also my stock of control birds. Until they were bitten by gnats, I found that they never became infected (except in a

single case in which I think I had overlooked the parasites on the first occasion), although large numbers of healthy birds were kept in this manner. The result in the case of the sparrows which were subjected to the bites of the infected gnats was different indeed. Out of 28 of these, dealt with from time to time, no less than 22, or 79 per cent., became infected in from five to eight days. And, as in the first experiment, all the infected birds finally contained very numerous parasites.

It was most interesting to watch the gradual development of the parasitic invasion in these birds; and this development presented such constant characters that, apart from other reasons, it was quite impossible to doubt that the infection was really caused by the mosquitoes. The course of events was always as follows: The blood would remain entirely free from parasites for four, five, six or even seven days. Next day one or perhaps two parasites would be found in a whole specimen. The following day it was invariably observed that the number of the organisms had largely increased; and this increase continued until in a few days immense numbers were present —so that, finally, I often observed as many as seven distinct parasites contained within a single corpuscle! Later on, many of the birds died; and their organs were then found to be loaded with the characteristic melanin of malarial fever.

I also succeeded in infecting on a second trial one of the six sparrows which had escaped the first experiment; and also a crow and four weaver-birds; and lastly, gave a new and more copious infection to four sparrows which had previously contained only a few parasites.

These experiments completed the original and fundamental observations on the life-history of the Hæmamœbidæ in mosquitoes. The parasites had been carried from the vertebrate host into the gnat; had been followed in their development in the gnat; and had finally been carried back from the gnat to the vertebrate host. The theories of King, Laveran, Koch and Bignami, and the great induction of Manson, were justified by the event; and I have given a detailed historical and critical account of these theories, and of my own difficulties and experiences, in the hope of bringing conviction to those who might, perhaps, otherwise think the story to be too wonderful for credence.

But work of great importance remained to be done. I had intended, immediately after making this study of one of the parasites of birds, to extend the investigation more fully to those of man—a work which now presented no difficulty, since both the kind of mosquito hospitable to them (*Anopheles*) and the form of the parasites in the mosquito were well known to me. Unfortunately I was obliged to attend to other and less important duties, which kept me fully occupied for several months—an interruption which practically put an end to my own study of the mosquito-theory at a very interesting point. No time, however, was really lost. In December

1898, Dr. Daniels of the Malaria Commission of the Royal Society and the Colonial Office, arrived in Calcutta to examine and report upon my results. After carefully repeating the various experiments he fully confirmed the statements made by me.* At the same moment, the work was taken up with great brilliance and success by Dr. Koch and by Prof. Grassi and Drs. Bignami and Bastianelli, in Italy. I must now describe the investigations of these observers—though I have scarcely space to do so at the length they deserve.

Ever since the discoveries of Laveran and Golgi, the Italian observers of the Roman school have done much important work on malaria, facilitated by the well-known prevalence of the disease near Rome; work, if not of much originality, yet full of careful detail. More recently, however, this work had been practically arrested by their theory—wholly gratuitous, but which they accepted as a dogma—that the motile filaments are forms of disintegration *in vitro*. When Manson propounded his theory, Bignami, for instance, rejected it on this ground. But at the same time he evolved a gnat-theory of his own—a theory that malarial fever is inoculated by gnats which carry the parasite from marshy areas. The arguments he used were the epidemiological ones already advanced by King, and which can scarcely be said to amount to more than a plausible hypothesis: the only solid basis for the theory—that of Manson—was opposed by him. Later, however, the work of Simond, Schaudinn, Siedlecki, MacCallum and myself, explained by Manson, rendered the Italian position concerning the motile filaments quite untenable; and Bastianelli, Bignami and Grassi now undertook a study of the mosquito-theory on sound principles. My own results, with descriptions of the technique employed and with illustrations of the zygotes, had been published from time to time; a summary of them had been given by Manson in June 1898, and another, including the infection of healthy birds, before the British Medical Association, early in August; and there could therefore be no difficulty in following up the observations therein recorded. In September, Grassi published a paper in which he described certain investigations made in Italy with a view to ascertaining the species of gnats which are associated with the prevalence of malaria in that country. Such investigations are not, I think, trustworthy; and as a matter of fact two out of the three species of gnat then selected by Grassi as being malaria-bearing ones, have now been rejected by him. The third species was an *Anopheles*, namely *A. claviger*, Fabr.

At the same time Bignami resumed his study of the subject. Some years previously, following his theory, he had endeavoured to infect healthy persons by the bites of gnats brought from malarious places. He had failed and abandoned his efforts—and I believe that his method would of itself never had led to a solution of the problem. In the autumn of 1898, however, he renewed his efforts; but was

* Nature, August 3, 1899.

again unsuccessful until he used a number of *Anopheles claviger*, brought from a house containing infected persons. The result was successful, the subject of the experiment becoming infected after some time. This important experiment gave the first confirmation with human malaria of my previous inoculation experiments with the malaria of birds; but since other species of gnats as well as *A. claviger* had been employed, it failed to fix suspicion entirely on the latter. In order to obtain this result, these observers were finally obliged to resort to the correct method of Manson and myself—namely that of direct cultivation of the parasites in the gnat. Success was now immediate. The zygotes and blasts of the parasites were found, exactly as previously described by me, in the tissues of *A. claviger*; and lastly, healthy persons were infected by the bites of these insects. Pushing forward with admirable rapidity, the Italian observers next found that all three species of the human Hæmamœbidæ are cultivable in *A. claviger*; and not only in this, but in other Italian species of *Anopheles*, while, like me, they failed in cultivating the parasites in *Culex*.

Almost simultaneously Koch repeated and confirmed with the weight of his authority most of the results which had been obtained as regards both the human and avian parasites. In August 1899 the malarial expedition sent to Sierra Leone by the Liverpool School of Tropical Medicine (of which expedition I was a member), found the human parasites in two species of *Anopheles* in that colony, namely *A. costalis*, Loew, and *A. funestus*, Giles. I hear also that the same result has been obtained with *Anopheles* in two other parts of the world, so that it would appear that something like nine species of *Anopheles* have now been inculpated—while as yet every species of Culex which has been tried has failed to give positive results.

From this point it becomes impossible to follow in detail the researches carried out in connection with the mosquito theory in various parts of the world. The facts already collected would fill a small volume; and every month witnesses additional publications on the subject. I shall therefore, in conclusion, content myself with a brief reference to three points of leading importance.

I shall first try to indicate how completely the recent discoveries explain the well-known laws regarding the diffusion of malaria. As mentioned at the beginning of this lecture, malarial fever has long been known to be connected with the presence of stagnant water. That is to say, we generally, though not invariably, find that the disease is associated with low-lying flat areas, where water tends to collect to a considerable extent. It was indeed the general appreciation of this law which led to the old miasma-theory of the disease—the theory on which the word "malaria" was based. We assumed that the poison is one which rises from marshy areas in the form of a mist, and which thence infects all living within a given distance. Later, when the pathogenetic parasite was discovered in the blood of febricitants, many observers, still clinging to this conception, thought

that the parasite is an organism which in its free state dwells in such places, and diffuses itself in such mists. It is interesting to note how near to the truth this almost instinctive conception took us. It is right in idea, wrong in fact. It is not the parasite itself which springs from the marshy ground, *but the carrier of the parasite.*

This was one of the many interesting points made by King in his mosquito-theory of seventeen years ago. But King fell into an error which could have been used as a powerful argument against his hypothesis. He seemed to have assumed that all mosquitoes rise from marshes. Hence, he said, malaria exists in the presence of marshes; hence it is a disease of the country, rather than of towns, and so on. As a matter of fact, mosquitoes as a rule do not rise from marshes at all; they do not all even rise from pools of water on the ground; the commonest species, at least of those which habitually annoy human beings, spring from tubs and pots of water in the vicinity of houses, and are indeed more common in cities than in country places, at any rate in the tropics. Now it is not the least interesting feature of recent researches that they have shown where the error lay. As soon as I have succeeded in cultivating the human parasites in my "dappled-winged mosquitoes," which were really gnats of the genus *Anopheles*, I began to study the habits of these insects, and soon ascertained the remarkable fact that while gnats of genus *Culex* generally breed, in India, in vessels of water round houses, gnats of genus *Anopheles*, which I had just connected with malaria, breed in *small pools of water on the ground.* This point was made the subject of a special investigation by the recent expedition to Sierra Leone; and we found that the law holds good there as in India. While *Culex* larvæ were to be seen in almost every vessel of water, or empty gourd or flower-pot in which a little rain-water collected, in only one case did we find *Anopheles* larvæ in such. On the other hand, *Anopheles* larvæ occurred in about a hundred small puddles scattered through the city of Freetown—puddles mostly of a fairly permanent description, kept filled by the rain, and not liable to scouring out during heavy showers. What was almost equally significant, the larvæ seemed to live chiefly on green water-weed. Hence it follows that while *Culex*, the apparently innocuous genus of gnats, are essentially, or at least often, domestic insects, *Anopheles*, the malaria-bearing genus, are essentially gnats which spring from stagnant water on the ground. And numerous other facts in the history of malaria can be explained by the same discovery. It is supposed, for instance, that malaria originates from freshly-turned earth; now we actually noted examples where railway embankments and the like had produced *Anopheles* pools; and it is easy to see that disturbance of the soil may often produce depressions in the ground capable of holding a little rain-water suitable for the larvæ of these insects. Again malarial fever often appears on board vessels which have touched at malarious ports; as an explanation of this we ascertained that *Anopheles* visit ships from the shore. In short, on study-

ing the matter from every point of view, I must confess to being ignorant of any well-established fact about malarial fever which is not explained by the mosquito-theory.

This brings me to the subject of *objections* to the mosquito-theory. In view of the exact and copious microscopical and experimental evidence which has now been collected in proof of the theory, it is no longer permissible to doubt the main facts; and the objections which one still finds, both in the lay and the medical press, are generally based on a complete ignorance of these facts, and need not be discussed here. But there is one objection—frequently made, in spite of corrections as frequent, by persons who reside in malarious places—which deserves comment. This is, that malaria exists where there are no mosquitoes, and that so-and-so has had fever without being bitten by gnats at all. Generally speaking, we must always remember that malarial fever is a disease in which relapses occur perhaps for years after the first infection, and that it is this first infection and not the relapses which are due to the bite of *Anopheles.* It is thus possible to suffer from any number of attacks of fever without being bitten by *Anopheles* (except on one occasion), and without invalidating the theory—a fact of which those who argue in this manner are generally ignorant. Again, it is well known that one may be bitten without perceiving it; that some persons are singularly callous to the punctures of these insects; and, lastly, that many others have very limited powers of observation. I may say at once that, personally, I cannot accept any statement to the effect that gnats are absent in any locality in the tropics, until such a statement is made by a competent observer after direct search; because I have never been in any place in the tropics—and I have been in a large number—where there were no gnats. On the other hand, I have often found numerous gnats in localities where I was previously told there were none. I was once actually informed that there are no mosquitoes in Sierra Leone! The fact is that those who will trust the statements of the general public on such matters must be very credulous.

I turn lastly to the all-important subject of *prevention*, but can do no more than touch upon it here. Two methods suggested themselves at once. I need not refer to that of guarding against the bites of these insects by the use of mosquito-nets and so on—an obvious and, I believe, an exceedingly useful measure, which may reduce the chances of infection to a small fraction. Unfortunately such methods will never be employed on a large scale in the majority of malarious localities; and we must resort to the *destruction of malaria-bearing species of gnats.* Early in 1892 I reported to the Government of India that it may be possible to exterminate *Anopheles* in *some* localities—especially some towns, cantonments and plantations—owing to the habit the insects have (in some places) of breeding only in selected pools. Since then, a considerable literature has already grown up round the subject. Reviewing this literature, it seems probable that we may be able to exterminate *Anopheles* or at least

largely reduce their numbers, in towns where, owing to the conformation of the ground, the low level of the subsoil water or the small rainfall, surface pools suitable for the insects are comparatively few. The methods which can be adopted against the larvæ are numerous —such as brushing out the pools with a broom, draining them away, filling them up, or treating them with various *culicides*, such as paraffin and numerous other substances (recently investigated by Celli and Casagrandi). On the whole the most promising method which suggests itself is the employment of some cheap solid material or powder which dissolves slowly, which kills the larvæ without injuring higher animals, and which renders small pools uninhabitable for the larvæ for some months. If, for instance, a cartload of such a material would suffice to extirpate the larvæ for a square mile of a malarious town, the result would be a large gain to its healthiness. Dr. Fielding-Ould has lately reported favourably on *tar*. Grillet recently reports a case in France where a large district was rendered free of malaria by the extensive use of *lime* for agricultural purposes. *Gas-lime*, or even common *salt*, may be suggested. In short, though the question of the possibility of attacking these insects with success is still entirely in the experimental stage, we may reasonably hope that the mosquito-theory of malaria may some day prove to be as useful to humanity as it certainly has proved interesting to the student of science.

In conclusion, however, I should add that this result is not likely to be attained unless we, as a nation, determine to pay more attention to scientific discoveries in the field of tropical medicine than hitherto we have done. During the last fifty years discovery after discovery in this field has been made without finding any adequate reflex in medical and sanitary practice in our tropical possessions. The discoveries, for instance, of Lösch, Davaine, Dubini, Bilharz, Bancroft, Koch, Laveran, Manson, Carter and Giles, though nearly concerned with the lives of thousands of human beings, have been generally treated either with scepticism or neglect—have been neither sufficiently followed in the laboratory nor sufficiently acted upon in the region of practical sanitation.

[R. R.]

GENERAL MONTHLY MEETING,

Monday, March 5, 1900.

His Grace The Duke of Northumberland, K.G., President, in the Chair.

Mrs. Francis Ernest Colenso,
Miss Annie C. Colthurst,
Joseph David Everett, Esq. M.A. D.C.L. F.R.S.
Professor Percy Faraday Frankland, D.L. B.Sc. F.R.S.
Alexander C. Ionides, Esq.
Mrs. Alfred B. Kempe,
Alexander G. Low, Esq.
Colonel William Thomas Makins,
Colonel Fairfax Rhodes,
William Mudd Still, Esq.
Major Frederick Richard Trench-Gascoigne,
Samuel West, M.D. F.R.C.P.
Mrs. West,
Miss Hilda Meadows White,

were elected Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Meteorological Office—Diurnal Range of Rain, 1871–90. 8vo. 1899.
Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. 1° Semestre, Vol. IX. Fasc. 1–3. 8vo. 1900.
American Academy of Arts and Sciences—Proceedings, Vol. XXXV. Nos. 4–7. 8vo. 1899.
American Geographical Society—Bulletin, Vol. XXXI. No. 5. 8vo. 1899.
Astronomical Society, Royal—Monthly Notices, Vol. LX. No. 3. 8vo. 1900.
Bankers, Institute of—Journal, Vol. XXI. Part 2. 8vo. 1900.
Bannerman, W. Bruce, Esq. M.R.I. (the Editor)—The Visitations of the County of Surrey, 1530–1623. (Harleian Society, Vol. XLIII.) 8vo. 1899.
Bickerton, Professor A. W. (the Author)—A New Story of the Stars. 8vo.
Some Recent Evidence in favour of Impact. 8vo. 1894.
Boyce, Professor—Instructions for the Prevention of Malarial Fever. 2nd edition. (Liverpool School of Tropical Medicine, Memoir I.) 8vo. 1900.
British Architects, Royal Institute of—Journal, Third Series, Vol. VII. Nos. 7, 8. 4to. 1900.
Camera Club—Journal for Feb. 1900. 8vo.
Chemical Industry, Society of—Journal, Vol. XIX. No. 1. 8vo. 1900.
Chemical Society—Journal for Feb. 1900. 8vo.

Editors—American Journal of Science for Feb. 1900. 8vo.
Analyst for Feb. 1900. 8vo.
Anthony's Photographic Bulletin for Feb. 1900. 8vo.
Astrophysical Journal for Feb. 1900.
Athenæum for Feb. 1900. 4to.
Author for Feb. 1900. 8vo.
Bimetallist for Feb. 1900. 8vo.
Brewers' Journal for Feb. 1900. 8vo.
Chemical News for Feb. 1900. 4to.
Chemist and Druggist for Feb. 1900. 8vo.
Education for Feb. 1900.
Electrical Engineer for Feb. 1900. fol.
Electrical Review for Feb. 1900. 8vo.
Electricity for Feb. 1900. 8vo.
Engineer for Feb. 1900. fol.
Engineering for Feb. 1900. fol.
Homœopathic Review for Feb. 1900. 8vo.
Horological Journal for Feb. 1900. 8vo.
Industries and Iron for Feb. 1900. fol.
Invention for Feb. 1900.
Journal of State Medicine for Feb. 1900. 8vo.
Law Journal for Feb. 1900. 8vo.
Life-Boat Journal for Feb. 1900. 8vo.
Lightning for Feb. 1900. 8vo.
Machinery Market for Feb. 1900. 8vo.
Motor Car Journal for Feb. 1900. 8vo.
Nature for Feb. 1900. 4to.
New Church Magazine for Jan. 1900. 8vo.
Nuovo Cimento for Feb. 1900. 8vo.
Photographic News for Feb. 1900. 8vo.
Physical Review for Feb. 1900. 8vo.
Popular Science Monthly for Feb. 1900. 8vo.
Public Health Engineer for Feb. 1900. 8vo.
Science Abstracts, Vol. III. Part 2. 8vo. 1900.
Science Siftings for Feb. 1900.
Telephone Magazine for Feb. 1900. 8vo.
Travel for Feb. 1900. 8vo.
Tropical Agriculturist for Feb. 1900.
Zoophilist for Feb. 1900. 4to.

Franklin Institute—Journal for Feb. 1900. 8vo.

Geographical Society, Royal—Geographical Journal for Feb. 1900. 8vo.

Grey, Henry, Esq. (the Compiler)—The Plots of some of the most famous old English Plays. 8vo. 1900.

Huggins, Sir William and Lady—An Atlas of Representative Stellar Spectra from 4870 to 3300. (Publications of Sir William Huggins' Observatory, Vol. I.)

Imperial Institute—Imperial Institute Journal for Feb. 1900.

Johns Hopkins University—American Chemical Journal for Feb. 1900. 8vo.

Jordan, William Leighton, Esq. M.R.I. (the Author)—Astral Gravitation and the first Law of Motion. 4to. 1899.

Mensbrugghe, Professor G. Van der, Hon. Mem. R.I. (the Author)—Le Centenaire de l'Institution Royale. 8vo. 1900.
Sur les effets mécaniques produits par l'élasticité de l'eau. 8vo. 1899.

Navy League—Navy League Journal for Feb. 1900. 8vo.

Numismatic Society—Chronicle and Journal, 1899, Part 4. 8vo.

Pharmaceutical Society of Great Britain—Journal for Feb. 1900. 8vo.

Phené, Dr. F.S.A. M.R.I. (the Author)—The Rise, Progress and Decay of the Art of Painting in Greece. 8vo. 1899.
Old London in Pre-Roman Times. 8vo. 1896.
On some Early Settlers near Conway. 8vo. 1899.

Photographic Society, Royal—Photographic Journal for Jan. 1900. 8vo.
Royal Society of London—Proceedings, No. 424. 8vo. 1900.
Selborne Society—Nature Notes for Feb. 1900. 8vo.
Stoddart, F. W. Esq. (the Author)—An Improved Sewage Filter. 8vo. 1900.
United Service Institution, Royal—Journal for Feb 1900. 8vo.
United States Department of Agriculture—Monthly Weather Review for Oct. 1899. 8vo.
United States Patent Office—Official Gazette, Vol. XC. Nos. 5-7. 8vo. 1899.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1900. Heft 1. 8vo.
Washington, Philosophical Society of—Bulletin, Vols. XI. XII. 8vo. 1892-95.

WEEKLY EVENING MEETING,

Friday, March 9, 1900.

Sir William Crookes, F.R.S., Vice-President, in the Chair.

Professor Frank Clowes, D.Sc. F.C.S. *M.R.I.*

Bacteria and Sewage.

The discovery made by Schwann, in 1839, that a putrefying liquid swarmed with microscopic living organisms, gave occasion to a long series of remarkable investigations as to the general nature and the life-history of these organisms, and the chemical changes which they produced.

Prominent amongst the names of those who prosecuted these investigations stands that of Pasteur, who, in 1857, drew attention to the nature and causes of fermentative changes produced upon sugar solution, of the putrefactive changes in liquids containing animal substances, and of disease changes in the blood of the living animal, which were produced in the presence of various minute living organisms. He showed that, if these liquids were sterilised by heat, and were then duly protected against receiving solid particles from the air, or from other sources, these changes did not occur; and that contact with air which had passed through a red-hot tube, or had been filtered through a cotton-wool plug, was incompetent to introduce the organisms and to start the above changes.

These researches drew attention to the important part played by the air as a vehicle of the organisms or of their spores, and were supplemented by the researches of Tyndall (1876), who proved that air, which had been allowed to remain at rest until its motes had subsided, was incompetent to produce putrefaction. Tyndall also proved that boiled sterilised broth, when opened in Alpine air, did not usually putrefy, and that the air near the earth's surface in different localities, and even in the same locality at different times, possessed infective power varying from nil to something considerable. The inference is that the distribution of these organisms and of their spores varies very considerably in any horizontal plane near the earth's surface.

Percy Frankland (1886) determined the number of these living organisms which could be developed from equal volumes of air collected at varying heights from the earth's surface. He made use of hills and cathedral towers for the purpose of collecting his samples, and noted a regular decrease in the number of the organisms which were in the air at greater and greater distances from the earth's surface.

These typical researches render it evident that the organisms and their spores, which are produced at or near the earth's surface, are wafted by natural atmospheric movements to some height, but are constantly tending to subside, and to sow the organisms broadcast as they descend.

It has been shown by more recent bacteriological investigations that many of these minute organisms are normally present in the living organism, and make their appearance in large numbers in the dejecta. It is therefore not remarkable that sewage, which contains the dejecta of men and animals, as well as the washings of considerable road and other surfaces, should contain micro-organisms and their spores in large number.

The fact that animal dejecta and sewage are inoffensively and gradually resolved into simple chemical compounds by contact with different kinds of soil has long been known, but this resolution has, until recently, been attributed to the purifying action of the earth itself, or of the organisms which it may contain. It is now abundantly proved that the resolving or purifying agents are, in the main, the micro-organisms which were originally present in the dejecta themselves, although undoubtedly organisms derived from the air, and those already present in the soil, contribute to the change when they are present.

The experimental purification of sewage by letting it stand in tanks filled with flints, gravel, coal, coke or other mineral substances, proves that there is no special virtue in soil. These experiments, originally commenced by the State Board of Health, Massachusetts, in 1887, have been repeated by many public sanitary authorities, and the results have been abundantly verified; and in various localities broken stone, broken slate, broken clay vessels, "ballast," or burnt clay have been successfully employed in the tanks in place of the materials which were originally used.

For the successful and inoffensive treatment of sewage by this means, a preliminary "priming" of the material is necessary. This is effected by allowing it to remain immersed in sewage for several hours daily for a few weeks. Sewage, which is then introduced and allowed to remain for a few hours in the tank containing the "primed" coke or other material, has the amount of its putrescible dissolved matters considerably and rapidly reduced, while its solid, finely-divided fæcal matter is brought into solution, and caused to undergo, in large measure, resolution into simple inoffensive compounds.

In order that these changes may be completed inoffensively, it is necessary that the "primed" coke surfaces shall be frequently placed in contact with air, and the process is therefore an intermittent one. The coke-bed is first filled with sewage, which is then allowed to flow out from the bottom and to draw air into the interstices of the coke. After the coke surfaces have been for several hours in contact with the air, the cycle of processes is repeated. The treatment of

fresh quantities of sewage in the same coke-bed may apparently be continued indefinitely.

The effluent from one coke-bed undergoes a considerable further purification if it is made to undergo similar treatment in a second coke-bed; and if this second contact with the coke surfaces is followed by ordinary sand filtration, such as is usually applied to river-water which is to be used for drinking purposes, an effluent of extraordinary purity is obtained.

The original method introduced by the Massachusetts experiments, and known as the intermittent aërobic treatment, is sometimes preceded by a preliminary anaërobic treatment. This consists in allowing the sewage to remain quiescent in, or to flow very slowly through, a large tank or channel. A thick, tough scum soon forms upon its surface, and protects the liquid from the air. Under these conditions many of the solid suspended particles of an organic nature pass into solution, and are thus rendered rapidly resolvable by subsequent aërobic intermittent treatment.

The above general description of the bacterial treatment of sewage has been subjected to modification as to details to suit the conditions of particular localities. Thus the sewage is in some places subdivided by suitable mechanical arrangements into drops, and allowed to fall continuously like rain upon the surface of the coke-bed. The bed never becomes full of liquid, since when the sewage has trickled through the coke, and has been exposed to the coke surfaces and to the interstitial air, it is at once allowed to flow away from the bottom of the bed.

That these methods of purifying sewage are correctly described as bacterial has been placed beyond doubt. Any conditions which are unfavourable to bacterial life at once retard the purification, while any treatment of the sewage which sterilises it arrests the purification entirely.

The bacteria in the sewage are considered to be the active agents, and to produce the changes either directly, or indirectly through their products or enzymes. Bacteria and their spores are found to be present in great numbers in sewage. London sewage has been shown by Dr. Houston and others to contain very large numbers of bacteria, varying from about three to six million per cubic centimetre. It seems probable that many of these bacteria form films, or "swarming islands," on the coke surfaces, similar to those which are produced by their growth upon the surface of a gelatine film (Fig. 1); the period of formation of these films may be assumed to be the period of "priming" already referred to. Probably the coke-bed aids bacterial action largely by furnishing surfaces of attachment to the bacteria, upon which they may alternately be exposed to air and to the sewage. The useful effect of solid surfaces in promoting bacterial action in the case of other similar changes is well known, and it may be connected with the effect which the surfaces exert in preventing the settling of the bacteria to the bottom of the liquid.

Sewage contains many different species of bacteria, some of which have been described and figured by Dr. Houston.* As is seen in Figs. 2, 3, 4, some of these bacteria possess motile tail-like flagella, and by the movement of these the minute organisms maintain a rapid progress through the liquid. Bacteria which are devoid of flagella and which cannot traverse free paths in the liquid are shown in Figs. 5, 6, 7, 8. In Fig. 5 the spores of these minute vegetable organisms are seen interspersed among the organisms themselves. The organisms have two methods of multiplying, by fission and by producing spores; the spores have the power of retaining vitality under conditions which are fatal to the organisms themselves. It is found that none of these are selectively retained by a coarse coke-bed during the treatment, but that all the species make their appearance in only slightly diminished numbers in the purified effluent from the coke-bed. The average reduction in the number of the bacteria in the sewage by one treatment in a coarse coke-bed amounted to only 27·7 per cent. It would therefore appear that the different species of bacteria assist one another in the purifying action, and by producing either contemporaneous or consecutive effects upon the sewage secure its purification: in bacteriological language, their action is either symbiotic or metabiotic, or possibly of both kinds. The organisms s em to establish and maintain a condition of equilibrium amongst themselves in the coke-bed, since attempts to artificially increase the number of certain species have thus far failed.

It appears that in the above processes there is no separation of the bacterial action which takes place in the presence of air from that which occurs only in the absence of air, and both processes probably proceed side by side in the open coke-bed. The anaërobic, or so-called "septic" treatment, during which cellulose is slowly resolved with separation of hydrogen and methane, is, however, sometimes made to precede the more truly aërobic treatment.

One result of the anaërobic treatment is the liberation of large volumes of combustible gas, and this gas has been employed at some works for illuminating purposes on the incandescent principle.

The general products from both processes of bacterial action are carbon dioxide, water, ammonia, nitrogen, hydrogen and methane; and in the aërobic changes the ammonia is subsequently oxidised into nitrite and nitrate.

The experience obtained from several years' experimental bacterial treatment of the sewage from several of our largest cities has recently been published.

In 1893 the London County Council constructed an acre coke-bed about three feet in depth at the Barking Outfall of the North

* The illustrative figures in this article have been selected from Reports on 'The Bacteriology of London Crude Sewage' and on 'The Bacterial Treatment of Crude Sewage,' by Dr. Clowes and Dr. Houston, issued by the London County Council (F. S. King and Son), and were originally produced from microphotographs taken by Dr. Norman from Dr. Houston's cultivations.

FIG. 1.—*Proteus vulgaris.* Impression preparation from "swarming islands" on gelatine; 20 hours' growth at 20° C. × 3000. (Houston.)

FIG. 2.—"*Sewage Proteus.*" Microscopic preparation stained by V. Ermengem's method, showing one flagellum at the end of each rod; from a 24 hours' growth agar culture at 20° C. × 1000.

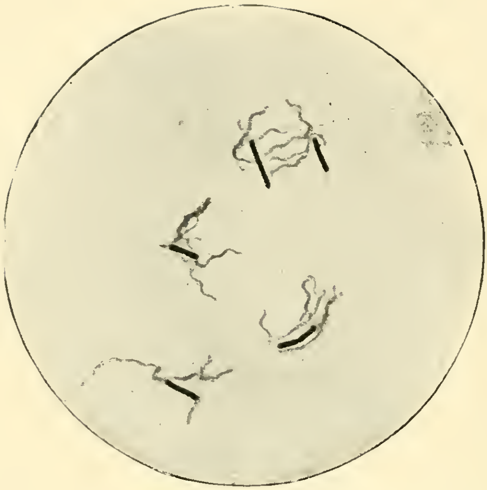


Fig. 3.—*B. mesentericus.* Sewage variety E. Microscopic preparation stained by V. Ermengem's method, showing numerous flagella, from a 20 hours' agar culture at 20° C. × 1000.

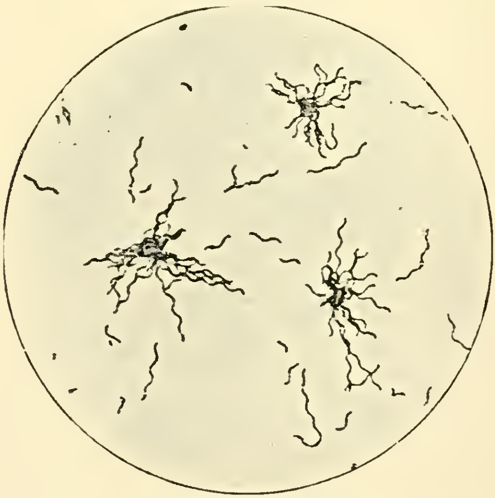


Fig. 4.—*B. mesentericus.* Sewage variety I. Microscopic preparation stained by V. Ermengem's method, showing numerous flagella; from a 20 hours' agar culture at 20° C. × 1000.

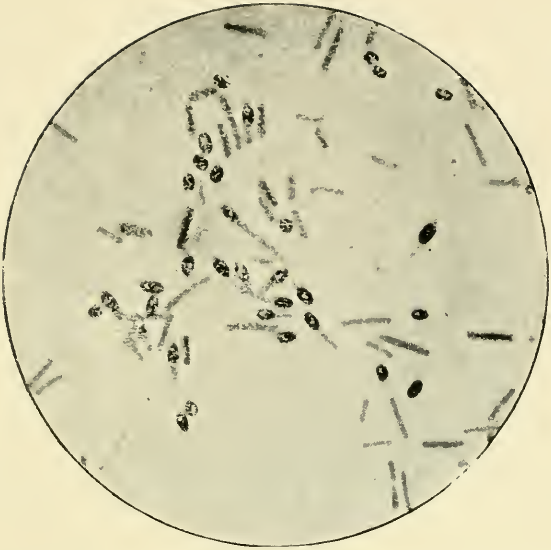


Fig. 5.—*B. enteritidis sporogenes* (Klein). Microscopic double-stained preparation from a serum culture, showing spores × 2000.

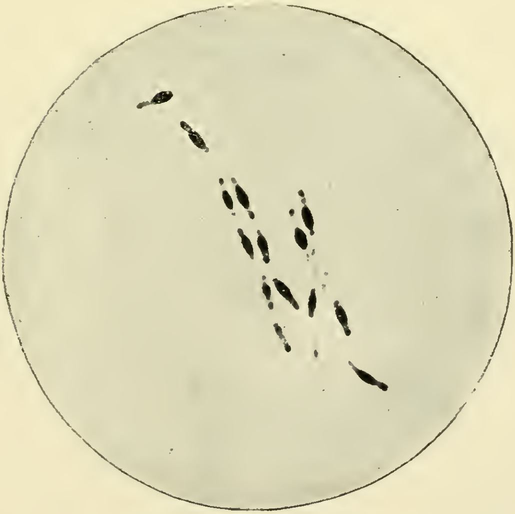


Fig. 6.—*Bacillus subtilis*. × 1500.

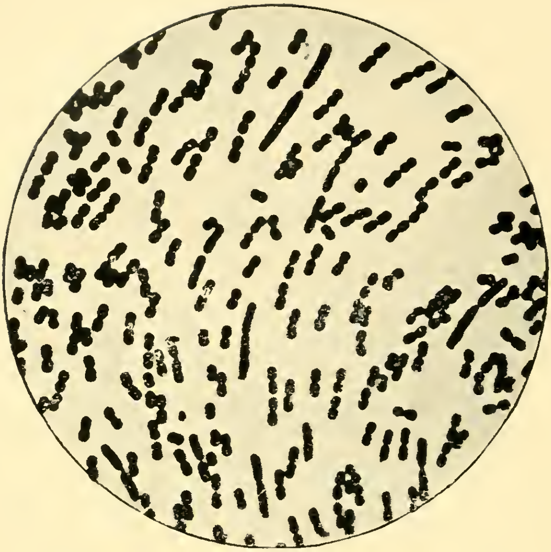


Fig. 7.—*B. subtilissimus.* Impression preparation from a gelatine plate culture × 1000.

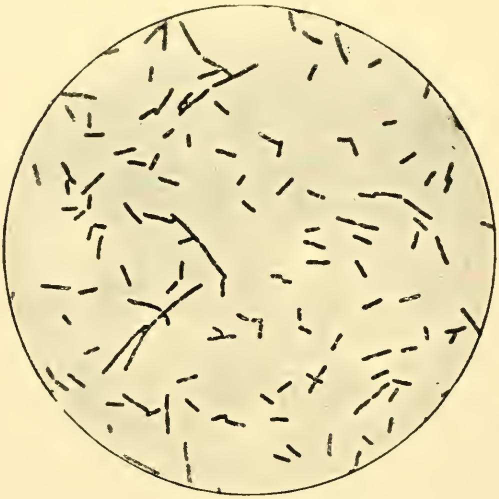


Fig. 8.—*B. mesentericus.* Sewage variety E. Microscopic preparation from a 20 hours' agar culture at 20° C. × 1000.

London Sewage. This bed has been receiving screened and sedimented sewage up to the present time, the process of sedimentation having been assisted by the addition of a small proportion of solutions of lime and of ferrous sulphate. Two years ago the bed was deepened to about six feet. Its purifying action, as measured by the amount of oxidisable matter present in the raw sewage and in the clear effluent, amounts to 92 per cent. and if the purification is calculated from the clear sewage and effluent, it amounts to 84 per cent. More recent experiments have proved that the treatment of raw roughly-screened sewage in such coke-beds is satisfactory, but that the capacity of the bed becomes continuously reduced by

Fig. 9.—*Proteus vulgaris.* × 1000.

the deposition upon the coke of mineral matter from road detritus, of particles of straw, chaff and woody matter from the horse-traffic and from the wood pavements. It was, therefore, evident that these matters must be deposited by sedimentation before the sewage was brought into the coke-beds. A comparatively rapid process of sedimentation suffices to remove these matters, since even the cellulose matters arrive in the sewage in a heavy and waterlogged condition.

It was found advantageous to use coke in comparatively large fragments, about the size of walnuts, since this facilitated the rapid draining of the liquid from the coke, and at the same time increased the sewage capacity of the bed and promoted its efficient aëration. The depth of the beds has been augmented from 4 to 13 feet, and the increase of depth seems to be attended with increase of efficiency.

The 13-foot bed has for long periods given a purification from dissolved oxidisable matter of over 60 per cent. It has maintained a most satisfactory state of aëration, since the air drawn from the bottom has contained, on an average, 17 per cent. of oxygen.

About 60 per cent. of the matter which settles from the sewage under ordinary conditions is combustible, and could, therefore, very well be dealt with by a destructor.

The tendency of the coke bacteria beds is undoubtedly to improve in their purifying power with age, provided they are not overworked. A bed which had given for some time a 50 per cent. purification, gradually increased in efficiency until its purifying effect reached nearly 70 per cent. The effluent from this bed underwent an additional purification of 20 per cent. by treatment in a second similar bed.

The effluent from a single coke-bed worked on the intermittent principle was clear and odourless, and remained in this condition when it was kept in open or closed bottles in a warm laboratory. It maintained the life of gold-fish, roach, dace, and pike indefinitely: it was therefore not only well aërated, but was able to maintain its aërated condition. This proves that it was free from any rapidly oxidisable matter. It was undoubtedly, however, undergoing a gradually further purification by the action of the bacteria which it contained, and with the assistance of dissolved oxygen. Such an effluent would be quite suitable for introduction into the tidal part of the river, where the water is too salt and muddy to be used for drinking purposes.

Bacteria are present in large numbers in the river-water itself, and undoubtedly exert a most useful purifying effect upon the water during its flow. The relation between the number present in the sewage and in the water of the River Thames, below and above locks, is shown by the following estimations made by Dr. Houston. The number of liquefying bacteria included in the total number of bacteria present in one cubic centimetre, and the number of spores of bacteria, are also stated :—

	Bacteria	Liquefying Bacteria	Spores
Raw sewage from North London, Feb. to April 1898 .	3,899,259	430,750	332
Raw sewage from South London, Feb. to April 1898 .	3,526,667	400,000	365
May to Aug. 1898	6,140,000	860,000	407
Effluent from coke-bed, South London, May to Aug. 1898	4,437,500	762,500	252
[Percentage reduction by passing through coke-bed	[27·7]	[11·4]	[38]
Lower Thames water, Greenhithe, half ebb tide, Oct. 1898	10,000	—	63
Lower Thames water, Barking, low tide, Nov. 1898 .	34,400	—	89
Upper Thames water, between Sunbury and Hampton, Nov. 1898	5,100	—	56
Upper Thames water, Twickenham. Nov. 1898 . .	3,000	—	18

The results obtained by the experimental bacterial treatment of sewage at Manchester during the last two or three years bear out generally those which have been obtained in London. The treatment has differed in some details from that adopted in London. The particles of coke constituting the coke-beds have been smaller. The coke-beds have been subjected to a larger number of intermittent fillings per day; and the preliminary treatment in an open anaërobic tank has been carried out with advantageous results. The scientific experts who have suggested and watched the experiments state their conviction that bacterial treatment is the treatment which is most suitable for Manchester sewage, but that in order to secure the most effective purification, the coke-beds must have sufficiently frequent and prolonged periods of rest, and must be fed with sewage as free as possible from suspended matter, and as uniform in quality as may be. Preliminary anaërobic treatment is referred to as the best means of securing uniformity in quality of the sewage, and of adapting it to rapid subsequent aërobic purification. Four fillings in twenty-four hours have been found suitable, if one day's rest in seven is given to each coke-bed; the number of fillings, however, may exceed this without detriment to the bed or to the character of the effluent.

Town sewage is found to arrive at the outfalls at an almost constant temperature throughout the year. It rarely falls below 13° C. And this temperature not only prevents the possibility of the coke-beds being stopped by the freezing of the sewage, but also secures to the bacteria one condition favourable to their action. When a bed is too freely aërated by the passage of frosty air constantly through the interstices of the coke this favourable condition is, however, seriously interfered with, and the bed may even become stopped by the freezing of the sewage.

In the more recent experiments carried out in America by the State Board of Health, Massachusetts, the tendency has been to use fine coke, and to allow the effluent from the coke to pass through sand. The passage of the liquid has either been allowed to take place with the outflow widely opened, so that the bed never fills, or the sewage has been allowed to fill the bed and to remain quiescent in contact with the coke for a time, as in the English experiments. The conclusions arrived at seem to be that the degree of purification obtained by the use of fine coke and sand is very satisfactory, but that the volume of sewage dealt with in a given time is smaller than when larger coke fragments are used, and the tendency seems to be to adopt the larger coke in order to expedite the more rapid drainage away of the effluent.

It will be seen from what has already been said that it is well not to speak of this system of treatment as one of filtration. Filtration ordinarily implies a process of mechanical separation of material suspended in a liquid. The fact that the coke-beds only commence their purifying action after they have been "primed" by repeated contact with sewage, and that this purifying action keeps increasing

as the bed "matures," is sufficient to show that the action is by no means of a mechanical nature. It would be well, therefore, to speak of it as a process of bacterial treatment, and thus to indicate that the purifying agents are bacteria, which are acting under control, and are placed under conditions favourable to the development of their full activity.

It would be rash to say that the methods of bacterial treatment have as yet reached their most effective state; but it is significant that these methods have secured converts wherever they have received careful and fair trial, and that those are their warmest advocates who have had the widest experience of their working. It is even probable that further improvements will be made in the means of treating sewage bacterially; but it is quite certain that the processes at present in use are able to secure the economical and satisfactory purification of ordinary town sewage.

[F. C.]

WEEKLY EVENING MEETING,

Friday, March 16, 1900.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Sir Benjamin Stone, M.P.

Pictorial Historic Records.

In historical documents pictorial delineation of events is probably the most valuable means of preserving the truest record of passing and changing scenes. Many ancient drawings and engravings of the crudest character have been invaluable to the historian, as conveying more accurately to the mind the subjects of illustration than any amount of literary description could do, and we are indebted to artists of former times for presenting to us scenes which we could not possibly have re-created from merely word description.

It is true that imagination and artistic instincts have frequently transmitted to us exaggerated and distorted representations of what was actually seen, but, at the same time, it has not infrequently been the case that the most patient efforts have been concentrated on minute details, which are most usefully instructive. In estimating the value of such work, it only provokes the regret that drawings of even this unfinished character are not more generally available.

In recent years the science of photography has come to the aid of the historian, and this ready means of record is now in evidence in all directions. Since the discovery of permanent processes the value of photographic pictures has been increasingly apparent, and the need of well-directed efforts for securing reliable and trustworthy presentments of objects and events becomes more and more apparent, especially when one sees around such a wasteful expenditure of effort and realises the need of systematic collection and preservation.

But a few years since, labour and money devoted to the collection of photographs for historical purposes—owing to their perishable qualities—had a most disappointing return; this is evidenced in many minor collections made for special reasons, in which the pictures are seen to be fading although possibly in careful keeping.

It has been only recently that the reproduction of photographs in carbon, in platinotype and by mechanical printing processes, has justified the hope and expectation of comparative permanency (that is, as permanent as the nature of the paper upon which the pictures are printed will permit), and which would encourage expenditure in forming an important collection. That public opinion is now in favour of making such collections is clearly shown from the interest

expressed on the subject in so many directions. The public press generally has accorded it support; local associations, archæological and photographic, have exerted themselves to form collections of local records; and during the past few years thousands of pictorial records of the greatest interest have been deposited in the care of local authorities.

One danger seems to threaten the value of such pictures, particularly in the field of work known as "process reproduction," where, to suit the supposed demand of the public for faithful records of events which photography is considered to supply, there is a lamentable custom resorted to of tampering with photographs either in the negative or in the process reproduction, so as to insert artificial effects or to correct partial failures in the original negative, and oftentimes embodying distinctly fraudulent effects.

This degrading custom cannot be too much regretted, as faithful historical records would become impossible, and the best pictures would be valueless, if it were permitted for one moment to take liberties of this character.

It is bad enough for such historical pictures to have suffered from lens-distortion, which gives an exaggerated appearance and fails to correctly record that which is actually seen by the eye. The best lens makers have used their efforts to produce lenses which shall render scenes in the most faithful manner possible, but hundreds of views are daily taken and reproduced, in the pictorial newspapers of the day, which are grotesquely absurd in their aspects and general proportions, in consequence of this primary fault. It will be apparent, therefore, that difficulties have to be dealt with in keeping up a high standard of excellence needed for making a perfectly satisfactory record of current history.

The efforts made to formulate the nucleus of a national collection for preservation in the British Museum, have certainly been in sympathetic accord with public opinion, for not only has there been a consensus of approval in all directions, but there has been a generous response to appeals for suitable pictures to add to the collection.

It would be impossible to refer in detail to the many contributions which have been made, and I can only indifferently satisfy myself by making a brief reference to some of them, which, as far as they go, represent the idea that pictorial records are the most satisfying means for placing upon record the best representation of current history.

It may be assumed that such pictures should have explanatory notes to make them intelligible, for, however good the pictures may be in themselves, they will be more valuable if used in conjunction with literary matter, condensed as much as possible, and of a perfectly reliable character.

This will be more clearly understood if it is remembered that photographic pictures, or engravings, or indeed any illustrations—whether artistic productions in oil and water-colours, lithographs, or other similar productions—have tenfold more interest when the objects or scenes portrayed are recognised and known, inasmuch as the whole subject is at once illuminated by the recollections of those

who inspect them. So by comparison does an ordinary pictorial illustration without a name, title or description, suffer in interest in the minds of those who look upon it for the first time and who do not possess a clue to its history or associations. Or, to give another illustration, how easy it is to create an interest in a most uninviting subject if an appeal is made to the imagination to clothe it with historical interest, such for instance as views of the dungeons of the Tower of London, which cannot have picturesqueness or beauty, but whose inscriptions on the walls, connected as they are with tragic episodes of English history, at once give to them a pathetic and engrossing interest.

It is true that only a very small number of such pictorial records, in comparison with the vast numbers of suitable subjects available, has yet been contributed to the collection, but it is satisfactory to know that a good beginning has been made, and that the work is extending in a marked manner. When one considers the possibilities of extension, it is almost bewildering to speculate upon what might be accomplished. To begin with, there are possible workers everywhere; few middle class houses or families are now-a-days without a camera, and the smallest contributions may possibly rank among the most valuable gifts. Opportunities and subjects abound in all directions; it often happens that familiar objects near at hand are despised by the photographic worker, imagining that scenes novel to him are better and more desirable subjects for selection.

To those who live in towns, street scenes and the domestic life of the inhabitants offer abundant opportunities, for they disclose the social conditions of the people of to-day. How invaluable would a comparison be, if similar pictures were available of similar scenes in former days!

We are proud of our progress and the well-to-do habitations of our middle classes; would that we could compare them with the picturesque streets, the overhanging houses, the half-timbered dwellings, to say nothing of the quaint costumes and artistic surroundings of Tudor times; for, with all our pride of modern self-importance, I think we might possibly learn something from the days when Shakespeare, Lord Bacon, old John Stowe, and others, were living actors on the scene.

To dwellers in the country there are limitless possibilities. There is country life in all its phases, the old manor house, the way-side inn, perhaps a ruined castle, or the crumbling remains of an abbey or monastery. The parish church alone may serve to engross the best efforts: Norman work in the tower, mediæval additions, the decay and restoration, all mark events in ecclesiastical or political history.

Then there are village costumes, many of them relics of the remote past, in which again possibly the parish church plays a part; and lastly the very tombstones in the churchyard offer fleeting records worth noting, and which are irrecoverably perishing before our eyes. The evolution of such monuments is a delightful study in itself, from the early monolith and cromlech of pre-historic days to

the box tomb of our grandfathers' time, there is a progressive story which is clearly discernible in these churchyard memorials.

If remarkable or special objects are sought for, our cathedrals and the mansions of our old nobility provide endless material. A single cathedral may wholly occupy the attention of a devoted worker for years; its architectural details, picturesque vistas, monumental effigies, tombs and inscriptions, old and modern alike, are worth notice; whilst such noble sanctuaries as St. Paul's or Westminster Abbey are of surpassing interest and possess inexhaustible associations.

In addition to these varied objects of interest, there are scattered throughout the country a great number of historical documents, in the shape of transfer deeds, charters, manuscript letters, and other records of national or local value. As all such precious evidence is liable to destruction by fire or other accidents, it is of the greatest value to duplicate them by photographs. Such desirable work offers a large field to those who have patience to undertake such labour.

There are innumerable ancient State and Ecclesiastical records of historical interest stowed away in the deed chests of private families which have never been transcribed or copied; to say nothing of stores possessed by cathedral chapters, by church authorities, and various corporations, the contents of which are absolutely unknown. These, by duplication, by photography, and circulation amongst those who are interested in such matters, would permit of their being carefully studied pending publication, and would throw much additional light on passages of past history.

These observations would not be complete without reference to the excellent photographic record work already done in several parts of the United Kingdom. The efforts of the Warwickshire Photographic Survey Council have resulted in making a fine collection of upwards of 2000 pictures of that county, which are now deposited in the Birmingham Reference Library, and the names of Mr. Jerome Harrison, F.G.S., Mr. J. H. Pickhard, Mr. E. C. Middleton, Mr. James Simkins, Mr. C. J. Fowler, Mr. Harold Baker, and others, are honourably associated with this first distinct effort to collect records.

The example of Warwickshire has been followed in many parts of the country, and local collections are now in progress in the hundred of Wirrall (Cheshire), in Yorkshire, in Worcestershire, in the Borough of Scarborough, and indeed in many places.

In conclusion, the National Record Association itself has received help and encouragement from the learned societies and many associations having historical or literary objects in view. It would be invidious to mention the names of those who have so far contributed to make the collection, but the services rendered by Mr. George Scamel, the Honorary Secretary, deserve distinct recognition.

It is sufficient to say that a most promising commencement has been made towards forming a National Collection, and it is not too much to expect or hope that at some future time this will be one of the most valued possessions of the Nation.

[B. S.]

WEEKLY EVENING MEETING,

Friday, March 23, 1900.

His Grace the Duke of Northumberland, K.G. F.S.A., President, in the Chair.

Sir Andrew Noble, K.C.B. F.R.S. M. Inst. C.E. *M.R.I.*

Some Modern Explosives.

Nearly thirty years ago, in the Royal Institution, I had the honour of describing the great advances which had then recently been made both in our knowledge of the phenomena which attend the decomposition of gunpowder, and in its practical application to the purposes of artillery.

I described the uncertainty which up to that date had existed as to the tension developed by its explosion; the estimates varying enormously from the 101,000 atmospheres (about 662 tons on the square inch) of Count Rumford to the 1000 atmospheres (6·6 tons per square inch) of Robins, or, taking more modern estimates, from the 24,000 atmospheres (158 tons per square inch) of Piobèrt and Cavalli to the 4300 atmospheres (about 29 tons per square inch) of Bunsen and Schischkoff.

These uncertainties were, I think I may say, set to rest by certain experiments carried out both in guns and close vessels at Elswick, by the labours of the Explosive Committee appointed by the War Office, and by researches conducted by Sir F. Abel and myself. These researches were conducted on a large scale, with the view of reproducing as nearly as possible in experiment the conditions that exist in the bore of a gun. You may judge of the magnitude of the experiments when I tell you that I have fired and completely retained in one of my cylinders a charge of no less than 28 lbs. of ordinary powder.

The result of the discussion of the whole series of experiments led to the following conclusions:

1. That the tension of the products of combustion at the moment of explosion when the powder practically filled the space in which it is fired—that is, when the density is about unity—is a little over 40 tons on the square inch, or about 6400 atmospheres.

2. Although changes in the chemical composition of powder, and even changes in the mode of ignition, cause a very considerable change in the metamorphosis experienced in explosion—as evidenced by the proportions of the products, the quantity of heat generated, and the quantity of permanent gases produced, being materially altered—it is somewhat remarkable that the tension of the products in relation

to the gravimetric density is not nearly so much affected as might be expected from the considerable alteration in the above factors.

3. The work that gunpowder is capable of performing in expanding in the bore of a gun was determined both by actual measurement and by calculation, and the results were found to accord very closely.

4. The total potential energy of exploded gunpowder supposed to be fired at the density of unity was found to be about 332,000 gramme units per gramme, or 486 foot-tons per lb. of powder.

I must confess that when I gave the lecture I have referred to, seeing the many centuries during which gunpowder had held its own as practically the sole propelling agent for artillery purposes, seeing also that gunpowder differs in certain important points from the explosives to which I shall presently call your attention, I had serious doubts as to whether it would be possible so far to modify these latter as to permit of their being used in large charges and under the varied conditions required in the Naval and Military Services.

Gunpowder is not like guncotton, cordite, nitro-glycerine, lyddite, and other similar explosives, a definite chemical combination in a state of unstable equilibrium, but is merely an intimate mixture of nitre, sulphur and charcoal, in proportions which can be varied to a very considerable extent without striking differences in results. These constituents do not, during the manufacture of the powder, suffer any chemical change, and being a mixture it cannot be said under any condition truly to detonate. It deflagrates or burns with great rapidity, varying very largely with the pressure and other circumstances under which the explosion is taking place; a train like that to which I set fire taking as you see an appreciable time to burn, while in the bore of the gun a similar length of charge would be consumed in less than the hundredth part of a second.

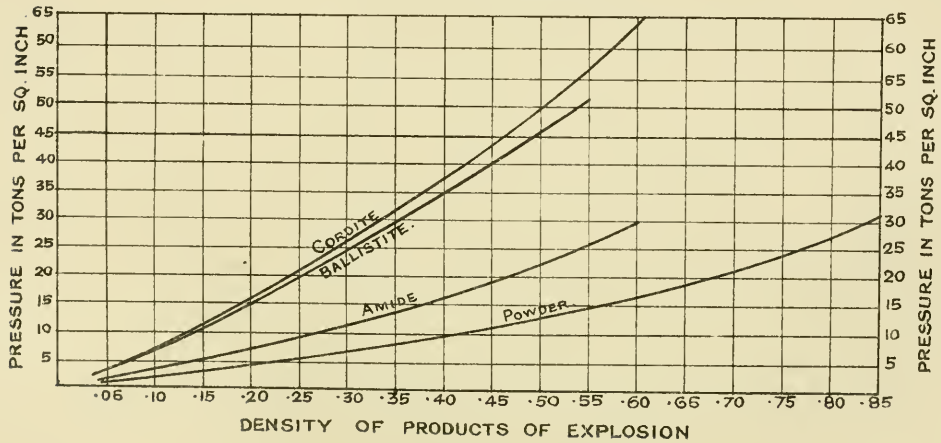
You will further have observed the heavy cloud of smoke which has attended the deflagration you have seen. Nearly six-tenths of the weight of the powder after explosion remains as a finely divided solid, giving rise to the so-called smoke familiar to many of you, and of which a good illustration is shown in this instantaneous photograph. By way of comparison I burn similar lengths of guncotton in the form (1) of cotton, (2) of strand, (3) of rope; and you will observe the different rates at which these varied forms of the same material are consumed, the rate depending in this case upon the greater aggregation and higher density, consequently higher pressure, of the successive samples.

Although the names of cordite and ballistite are probably familiar to all of you, the appearance may not be so familiar, and I have here on the table samples of the somewhat Protean forms which these explosives, or explosives of the same nature, are made to assume.

Here, for instance, are forms of cordite, the explosive of the Service, for which we are indebted to the labours of Sir F. Abel and Prof. Dewar. This, which is in the form of fine threads, is used in

Fig 1.

PRESSURES OBSERVED IN CLOSED VESSELS WITH VARIOUS EXPLOSIVES.



small arms; and here are successive sizes, adapted to successive larger calibres, until we reach this size, which is that employed for the charge of the 12-inch 50-ton guns.

A couple of the smaller cords I burn, both for purposes of comparison and to draw your attention to the entire absence of smoke.

The smoke of the gunpowder you see still floating near the ceiling; but little or no trace of smoke can be seen from such explosives as guncotton, cordite or ballistite, their products of combustion being entirely gaseous.

You will have observed that in the combustion which you have just seen there is no smoke, but I must explain, and I shall shortly show you, that this combustion is not quite the same as that which takes place, for instance, in the chamber of a gun. Here the carbonic oxide and hydrogen, which are products of explosion, burn in the air, giving rise, with the aid of a little free carbon, to the bright flame you see, and somewhat increasing the rate of combustion. In a gun, however, owing chiefly to pressure, the cordite is consumed in a very small portion of a second.

In order to illustrate the effect of pressure upon the rate of combustion, I venture to show you a very beautiful experiment devised by Sir F. Abel. It has been shown in this room before, but it will bear repetition.

In this globe there is a length of cordite. I pass a current through the platinum wire on which it is resting, and you see the cordite burns. I now exhaust the air and repeat the experiment. The wire is red-hot, but the cordite will not burn. That the failure to burn is not due to the absence of oxygen is shown by plunging lighted cordite into a jar of carbonic acid, where, although a match is instantly put out, the cordite continues to burn—but observe the difference. There is no longer any bright flame, although the cordite is being consumed at about the same rate as when burned in air; and when a sufficient quantity of the CO_2 is displaced, I can make the inflammable gases ignite and burn at the mouth of the jar.

Another illustration is also instructive. I have here a stick of cordite wrapped round with filter paper; I dip it in water and light the end. You may note that at first you see the bright flame; but, as the combustion retreats under the wet filter paper, there appears a space between the flame and the cordite, the flame finally disappears, hot gases with sparks of carbon alone showing.

One other pretty experiment I show. I have here a stick of cordite, which I light. When fairly lighted I plunge it in this beaker of water. The experiment does not always succeed at the first attempt, but you now see the cordite burning under the water much as it did in the jar of carbonic acid. The red fumes you observe are due to the formation of nitric peroxide, caused by the decomposition of the water by the heat.

I have on the table samples of certain other smokeless explosives of the same class. Here is a ballistite used in Italy. Here is some

Norwegian ballistite. Here, again, is ballistite in the tubular form, and in these bottles it is seen in the form of cubes. Here is some gelatinised guncotton in the tubular form, and here are some interesting specimens with which I have experimented, and which up to a certain pressure gave good results, but which exhibited some tendency to violence when that pressure was exceeded. Here also are some samples of the French B.N. powder, consisting of nitrocellulose partially gelatinised and mixed with tannin, and with barium and potassium nitrates. Lastly, I show you here a sample of picric acid, a substance which has been used for many years as a colouring material, but which will be of interest to you, because it is used as the explosive of lyddite shell, concerning which I shall presently have more to say; it differs from all the other explosives in being, in the crystalline form, exceedingly difficult to light. I fuse, however, in this porcelain crucible a small quantity. I pour a little on a slab, and on dropping a fragment into a red-hot test-tube you see with how much violence the fragment explodes. I also burn a small quantity, and you will observe that unlike guncotton, cordite and ballistite, it is not free from smoke, the smoke in this case being simply carbonaceous matter. You will observe also how much more slowly it burns.

The composition of these various explosives (although in the case of both cordite and ballistite I have experimented with samples differing widely in the proportion of their ingredients) may be thus stated.

The guncotton I employed was of Waltham Abbey manufacture, and when dried consisted of 4·4 per cent. of soluble cotton and 95·6 per cent. of insoluble—as used it contained 2·25 per cent. of moisture.

The service cordite consists of 37 per cent. trinitro-cellulose, with a small proportion of soluble guncotton, 58 per cent. of nitroglycerine and 5 per cent. of the hydro-carbon vaseline.

The ballistite I principally used was composed of 50 per cent. dinitro-cellulose (collodion cotton) and 50 per cent. of nitroglycerine. The whole of the cellulose was soluble in ether alcohol, and the ballistite was coated with graphite.

The French B.N. powder consisted of nitro-cellulose partly gelatinised and mixed with tannin, with barium and potassium nitrates. The transformation experienced by some of these explosives is given in Table I., while the pressures in relation to the gravimetric densities of some of the more important are shown in Fig. I.

The decomposition experienced by these high explosives on being fired is of much greater simplicity than that experienced by the old powders, and is, moreover, not subject to the considerable fluctuations in the ultimate products exhibited by them.

The products of explosion of gun-cotton, cordite, ballistite, etc. are at the temperature of explosion entirely gaseous, consisting of carbonic anhydride, carbonic oxide, hydrogen, nitrogen and aqueous vapour, with generally a small quantity of marsh gas.

The water collected, after the explosion vessel was opened, always smelt—occasionally very strongly—of ammonia, and an appreciable amount was determined in the water.

TABLE I.

Constituents.	Cordite.	Ballistite.	B.N.	Lyddite.
	vols.	vols.	vols.	vols.
CO_2	20·5	29·1	21·1	12·8
CO	23·3	21·4	24·2	49·7
H	16·5	15·0	16·4	13·8
N	14·6	10·1	12·6	19·6
H_2O	23·6	24·4	25·0	3·8
CH_4	1·5	trace	0·6	0·3
Quantity of gas in c.c. per gramme	890·5	807	822	960·4
Units of heat	1272	1365	1003	856·3

In examining the gaseous products of the explosion of various samples of gunpowder, it was noted that as the pressure under which the explosion took place increased the quantity of carbonic anhydride also increased, while that of carbonic oxide decreased. The same peculiarity is exhibited by all the explosives with which I have experimented. I show in Table II. the result of a very complete series of a sample of guncotton fired under varying pressures, and it will be noted that the volumes of carbonic oxide and carbonic anhydride are, between the highest and lowest pressures, nearly exactly reversed.

TABLE II.

Constituents.	Under Pressure of Explosion, tons per square inch.						
	2 tons.	8 tons.	12 tons.	18 tons.	20 tons.	45 tons.	50 tons.
vols.							
CO_2	21·44	25·06	26·27	27·21	26·75	28 13	29·27
CO	29·66	26·31	25·08	25·24	24·53	23·19	22·31
H	15·92	15·33	16·03	14·56	14·77	14·14	13·56
N	13·63	13·80	13·22	13·13	13·43	12·99	13·07
H_2O	19·09	19·09	19·09	19·09	19·09	19·09	19·09
CH_4	·26	·41	·31	·77	1·47	2·46	2·70

There are slight changes as regards the other products, but they do not compare in importance with that to which I have referred.

But before drawing your attention to other points of interest, it is desirable to give you an idea of the advances in ballistics which have been made both by improvements in the manufacture of the old powders and by the introduction of the new.

On Fig. II. are placed the results as regards velocity of nine explosives, commencing with the R.L.G_2 powder, which was in use in the latter part of the fifties, and terminating with the cordite of the present day.

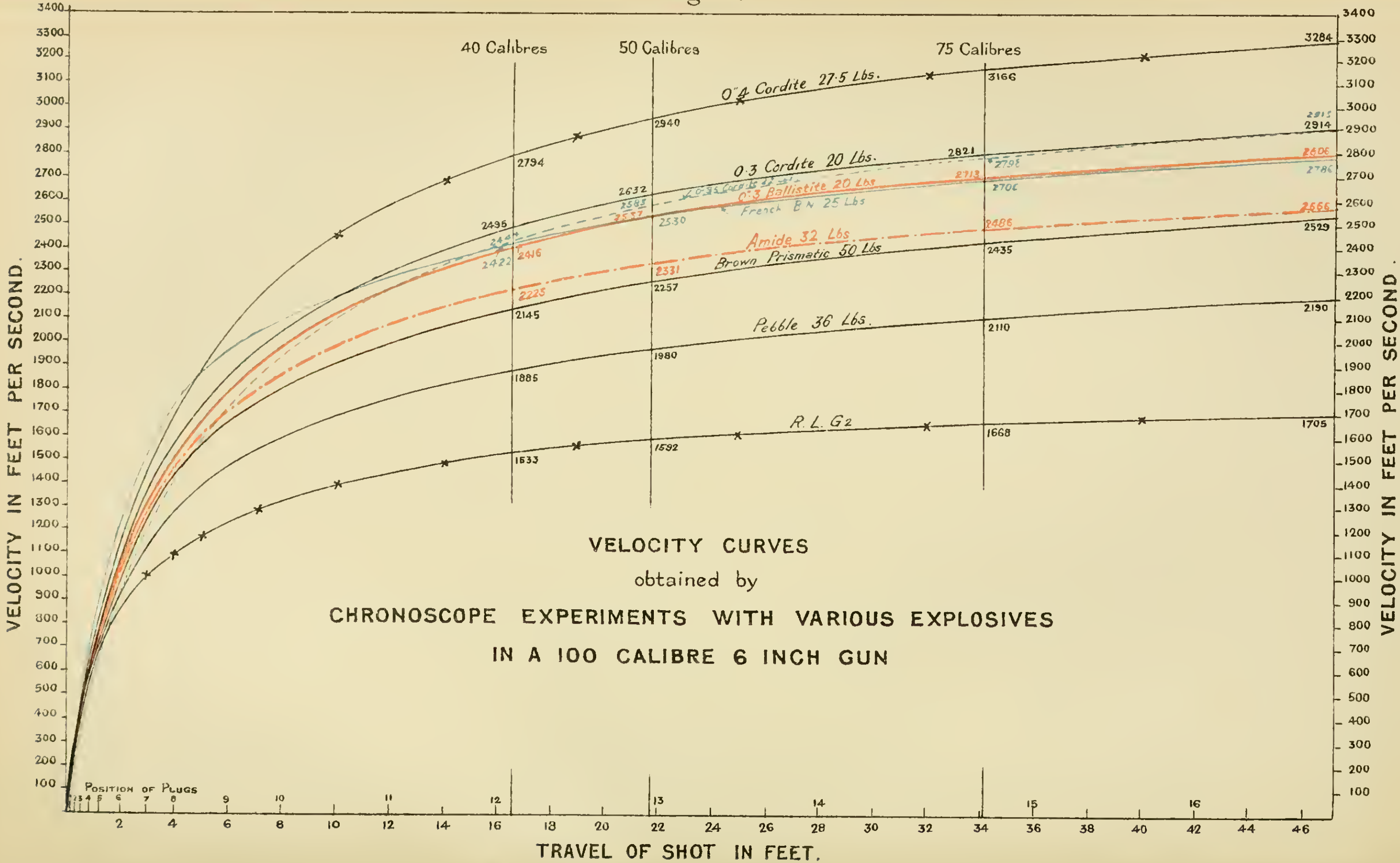
The experiments I am now referring to were made in a gun of 100 calibres in length, and were so arranged that in a single round the velocities could be measured at 16 points of the bore. The chronoscope with which these velocities were taken has been already described, and I will now only say that it is capable of registering time to the millionth of a second with a probable error of between two and three millionths. One curious fact connected with the mode of registration I may mention. In the early experiments with the old powders, where the velocities did not exceed 1500 or 1600 foot-seconds, the arrangement for causing the projectile to record the time of its passing any particular point was effected by the shot knocking down a small steel knife or trigger which projected slightly into the bore; but when the much higher velocities, with which I subsequently experimented, were employed, this plan was found to be unsatisfactory, the steel trigger, instead of being immediately knocked down by the shot, frequently preferred instead to cut a groove in the shot, sometimes nearly its whole length, before it acted. Hence another arrangement for cutting the primary wires had to be adopted.

The diagram I am now showing you is, however, both interesting and instructive. The intention, among other points, was to ascertain for various calibres in length in a 6-inch gun the velocities and energies that could be obtained, the maximum pressures, whether mean or wave, not exceeding about 20 tons on the square inch. The horizontal line or axis of abscissæ represents the travel of the shot in feet, the ordinates or perpendiculars from this line to the curve represents the velocity at that point.

The lowest curve on the diagram gives, under the conditions I have mentioned, the velocities attainable with the powder which was used when rifled guns were first introduced into the service; and you will note that with this powder the velocity attained with 100 calibres was only 1705 foot-seconds, while with 40 calibres it was 1533 foot-seconds. Next on the diagram comes pebble powder, with a velocity of 2190 foot-seconds; next comes brown prismatic, with a velocity of 2529 foot-seconds.

The next powder is one of considerable interest, and one which might have arisen to importance had it not been superseded by explosives of a very different nature. It is called Amide powder, and in it ammonium nitrate is substituted for a large portion (about half) of the potassium nitrate, and there is also an absence of sulphur. You will observe the velocity in the 100 calibre gun is very good—2566 foot-seconds. The pressure also was low, and free from wave action. It is naturally not smokeless, but the smoke is much less dense and disperses much more rapidly than does the smoke of ordinary powder. Its great advantage, however, was, that it eroded

Fig. II.


VELOCITY IN FEET PER SECOND.
40 Calibres
50 Calibres
75 Calibres
0·4 Cordite 27·5 Lbs.
0·3 Cordite 20 Lbs.
0·3 Ballistite 20 Lbs
French B N 25 Lbs
Amide 32 Lbs
Brown Prismatic 50 Lbs
Pebble 36 Lbs.
R. L. G2
VELOCITY CURVES
obtained by
CHRONOSCOPE EXPERIMENTS WITH VARIOUS EXPLOSIVES
IN A 100 CALIBRE 6 INCH GUN
Position of Plugs
TRAVEL OF SHOT IN FEET.

steel very much less than any other powder with which I experimented, while its great disadvantage was due to the deliquescent properties of ammonium nitrate necessitating the keeping of the cartridges in air-tight cases.

Next on the diagram comes B.N. or Blanche Nouvelle powder, an explosive which, while free from wave action, is remarkable, as you will note if you follow the curve, in developing a much higher velocity than the other powders in the first few feet of motion, and less in the later stages of expansion.

Thus, if you compare this curve with the highest curve on the diagram, that of the four-tenths cordite, you will note that the B.N. curve for the first eight feet of motion is the higher, and that at about eight feet the curves cross, the B.N. giving a final velocity of 2786 foot-seconds, or 500 feet below the cordite curve.

Then follows ballistite, which, with much lower initial pressure, gives a velocity of 2806 foot-seconds, or somewhat higher than that of B.N. Then follow three different sizes of cordite, the highest of which gives a muzzle velocity of 3284 foot-seconds, or a velocity nearly double that of the early R.L.G_2.

In the somewhat formidable-looking table (Table III.) I have placed on the wall, are exhibited the velocities and energies realised in a 6-inch gun with the various explosives I have named, and the table, in addition, shows the velocities and energies in guns of the same calibre but of 40, 50, and 75 calibres in length as well as in that of 100 calibres.

TABLE III.

6-INCH GUN, 100 CALIBRES LONG.

Velocities and Energies realised with High Explosives.
Weight of Projectile 100 lbs.

Nature and Weight of Explosive.	Length of Bore, 40 Calibres.		Length of Bore, 50 Calibres.		Length of Bore, 75 Calibres.		Length of Bore, 100 Calibres.	
	Velocity.	Energy.	Velocity.	Energy.	Velocity	Energy.	Velocity.	Energy.
	f. s.	ft. tons.	f. s.	ft. tons.	f. s.	ft. tons.	f. s.	ft. tons.
Cordite, ·4 in. (27·5 lbs.)	2794	5413	2940	5994	3166	6950	3284	7478
Cordite, 0·35 in. (22 lbs.)	2444	4142	2583	4626	2798	5429	2915	5892
Cordite, 0·3 in. (20 lbs.)	2495	4316	2632	4804	2821	5518	2914	5888
Ballistite, 0·3 in. cubs. (20 lbs.)	2416	4047	2537	4463	2713	5104	2806	5460
French B.N. (25 lbs.) ..	2422	4068	2530	4438	2700	5055	2786	5382
Amide prism (32 lbs.) ..	2225	3433	2331	3768	2486	4285	2566	4566
Brown prism (50 lbs.) ..	2145	3190	2257	3532	2435	4111	2529	4485
Pebble powder (36 lbs.)	1885	2464	1980	2718	2110	3087	2190	3326
R.L.G_2 (23 lbs.)	1533	1630	1592	1757	1668	1929	1705	2016

If you compare the results shown in the highest and lowest lines of this table, that is, the results given by the highest and lowest

curves on the diagram, you will see that the velocity of the former is nearly twice as great as that of the latter, while its energy and capacity for penetration is nearly four times as great.

I need hardly remind most of you that in artillery matters it is the energy developed, not the velocity alone, that is of vital importance. I venture to insist upon this point, because so many of those who desire to instruct the authorities write as if velocity were the only point to be considered. In a given gun with a given charge, if the weight of the shot, within reasonable limits, be made to vary, the ballistic advantage is greatly on the side of the heavier shot, and for three principal reasons :

1. More energy is obtained from the explosive.
2. Owing to the lower velocity, the resistance of the air is greatly reduced.
3. The heavier shot has greater capacity for overcoming the reduced resistance.

You will observe that on this velocity diagram, upon which I have kept you so long a time, is shown, not only the travel of the shot in feet, but the position of the plugs which gave the velocities. Further, on the higher and lower curves, the observed velocities are shown where it is possible to do so. Near the origin of motion the points are so close that it is not possible to insert them without confusing the diagram.

At the risk of fatiguing you, I show, in Fig. III., curves showing the pressure existing in the bore at all points, these pressures being deduced from the curves of velocity.

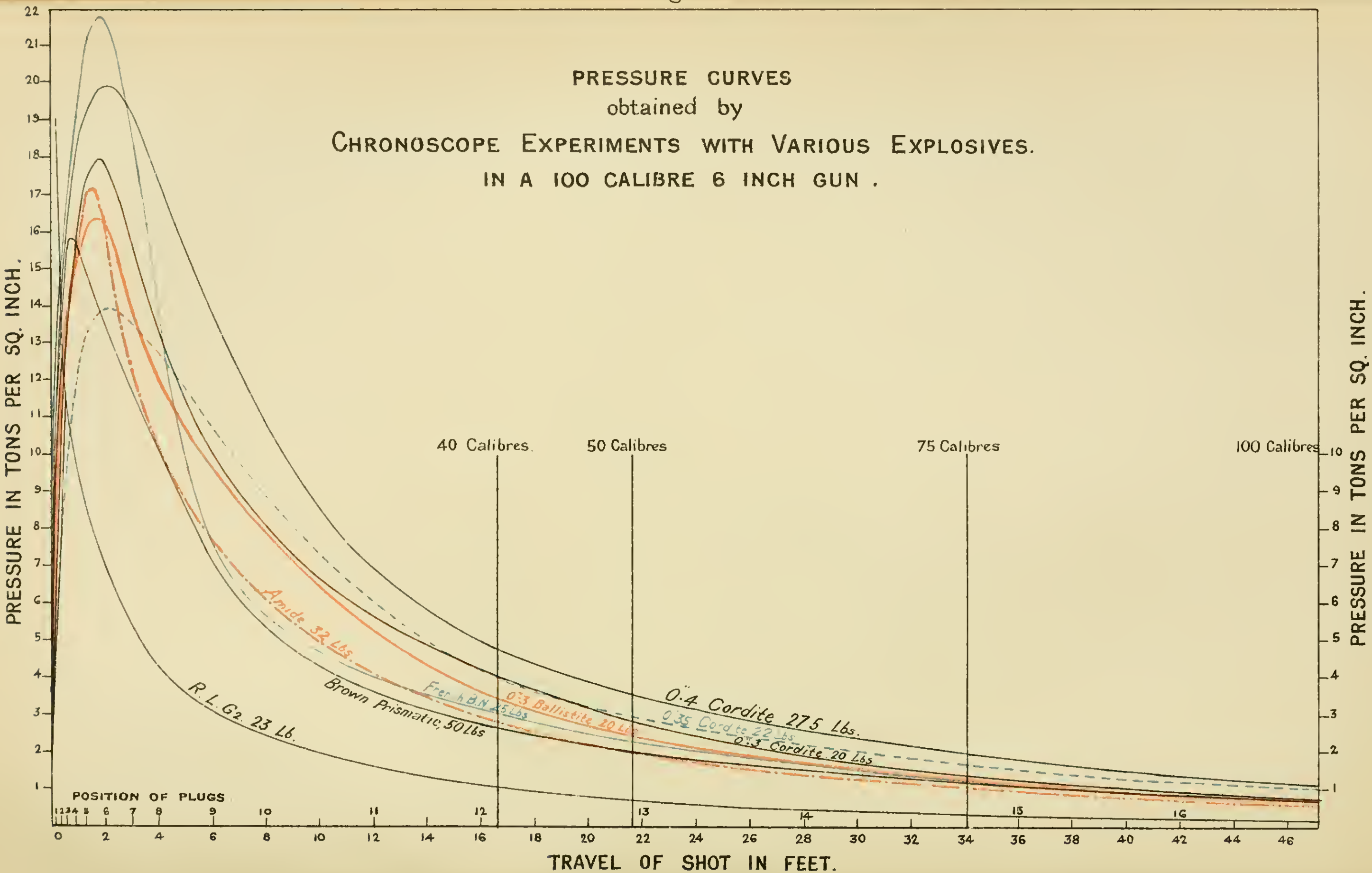
You will note the point to which I drew your attention with regard to the powder called B.N. You will remember, that in the early stages of motion it gave velocity to the shot much more rapidly than did the other powders. You see the effect in the pressure curves, the maximum being considerably higher than any of the other pressures, while the pressure towards the muzzle is, on the other hand, considerably below the average.

I fear you may think I have kept you unnecessarily long with these somewhat dry details, but I have had reasons for so doing.

In the first place, I desire to demonstrate to you the enormous advances which have been made in artillery by the introduction of the new explosives, and which we in a great measure owe to the distinguished chemists and physicists who have occupied themselves with these important questions.

Secondly, I desire to show you that the explosive which has been adopted by this country, and which we chiefly owe to the labours of Sir F. Abel and Prof. Dewar, is in ballistic effect inferior to none of its competitors. I might go further and say that it is decidedly superior.

Lastly, at a time when the efficiency of all our arms, and especially our artillery, is a question which has been deeply agitating the country, I may do some good by pointing out that the authorities are



PRESSURE CURVES
obtained by
CHRONOSCOPE EXPERIMENTS WITH VARIOUS EXPLOSIVES.
IN A 100 CALIBRE 6 INCH GUN .
PRESSURE IN TONS PER SQ. INCH.
TRAVEL OF SHOT IN FEET.
POSITION OF PLUGS
40 Calibres.
50 Calibres
75 Calibres
100 Calibres
R.L.G2. 23 Lb.
Brown Prismatic, 50 Lbs
Amide 32 Lbs.
French B.N. 25 Lbs
0.3 Ballistite 20 Lbs
0.4 Cordite 27.5 Lbs.
0.35 Cordite 22 Lbs
0.3 Cordite 20 Lbs

well aware that any practicable velocity or energy they may desire for their guns is at their disposal.

They have such guns—I mean guns with high velocity and high energy. Whether they have enough of them, and whether they are always in the right place, is another matter, for which perhaps the military authorities are not altogether responsible. But velocity and energy is not the only thing that is required under all circumstances in war, and I ask you to believe that if the War Office authorities have, for their field guns, fixed on a velocity very much below what is possible, they have had sound and sufficient reasons for so doing.

My firm and I, individually, have had much to do with the introduction of the larger high velocity and quick-firing guns into our own and other services, but as an old artillery officer, in no way responsible for our field guns, I may perhaps be allowed to say that, whether as regards *matériel* or *personnel*, our field artillery is inferior to none anywhere, and I venture to add that in the present war it appears to have been handled in a way worthy of the reputation of the corps.

I fear the causes of some of our military failures at the commencement of the war, must be looked for in other directions; and the present unfortunate war will turn out to be a blessing in disguise, if it should awaken the Empire to the necessity of correcting serious defects in our organisation, possibly the natural result of our constitution, and in that case the invaluable lives that have been lost will not have been sacrificed in vain.

I now pass to points which have to be considered when weighing the comparative merits of explosives for their intended ends.

You will easily understand that between explosives which are intended to be used for propelling purposes, and those which are intended to be used, say for bursting shell, a wide difference may exist.

In the former case, facility of detonation would be an insuperable objection. In the latter, the more perfect the detonation the better; certain special cases, to which I have not time to refer, excepted.

There exists, I think, considerable diversity of opinion as to what does, and what does not, constitute true detonation. I find many persons speak of a detonation, when I should merely consider that a very high pressure had been reached. This gun-cotton slab on the table affords me, I think, a fair opportunity of explaining my meaning. Were I to set fire to it, except for the large volume of flame and the great amount of heat generated, we in this room would not suffer; we should probably experience more inconvenience did I fire a similar slab of gunpowder, as detached burning portions would probably be projected to some distance.

But if I fired this same slab with two or three grammes of fulminate of mercury, a detonation of extreme violence would follow. The detonation would be capable of blowing a hole in a tolerably thick iron plate, and would probably put an end to a considerable proportion of the Managers in the front row.

I mentioned to you some time ago the time in which a charge would be consumed in the chamber of a gun—if a charge of 500 lbs. of these slabs were effectively detonated, this charge would be converted into gas in less than the 20,000th part of a second.

No such result would follow were I to try a similar experiment with a slab of compressed gunpowder of the same dimensions. I do not say the experience would be pleasant, but there would be nothing of the instantaneous violent action which marks the decomposition of the guncotton.

To give you an idea of the extraordinary violence which accompanies detonation, I have fired, for the purpose of this lecture, with fulminate of mercury, a charge of lyddite in a cast-iron shell, and those who are sufficiently near, can see for themselves the result. By far the greater part of the cast-iron shell, weighing about 10 lbs., is reduced to dust, some of which is so fine that I assumed it to be deposited carbon until I had tested it with a magnet. I may add that the indentation of the steel vessel by pieces of the iron which were not reduced to powder would appear to indicate velocities of not less than 1200 foot-seconds, and this velocity must have been communicated to the fragments in a space of less than two inches.

For the sake of comparison I place beside it a cast-iron shell burst by gunpowder. You will observe the extraordinary difference. I also have on the table two small steel shells exploded, one by a perfectly detonated the other by a partially detonated charge. I may remark that in the accounts of correspondents from the seat of war, frequent mention is made of the green smoke of lyddite. This appearance is probably due to imperfect detonation—to a mixture in fact of the yellow picric with the black smoke. I do not say however that imperfect detonation is necessarily an evil.

To another experiment I draw your attention.

For certain purposes I caused to be detonated, in the chamber of a 12-pounder, a steel shell charged with lyddite. The detonation was not perfect, but the base of the shell was projected with great violence against the breech screw. You may judge of how great that violence was when I tell you that the base of the shell took a complete impression of the recess for the primer, developing great heat in so doing; but, what was still more remarkable, the central portion of the base also sheared, passing into the central hole through which the striker passes. This piece of shell is upon the table, and open to your inspection.

One other instance, to illustrate the difference between combustion and detonation, I trouble you with. Desiring to ascertain the difference, if any, in the products of explosion between combustion and detonation, I fired a charge of lyddite in such a manner that detonation did not follow. The lyddite merely deflagrated. But a similar charge differently fired shortly afterwards detonated with such extreme violence as to destroy the vessel in which it was exploded. The manner in which the vessel failed I now show you (Fig. IV.);

Fig. IV.

EXPLOSION VESSEL.

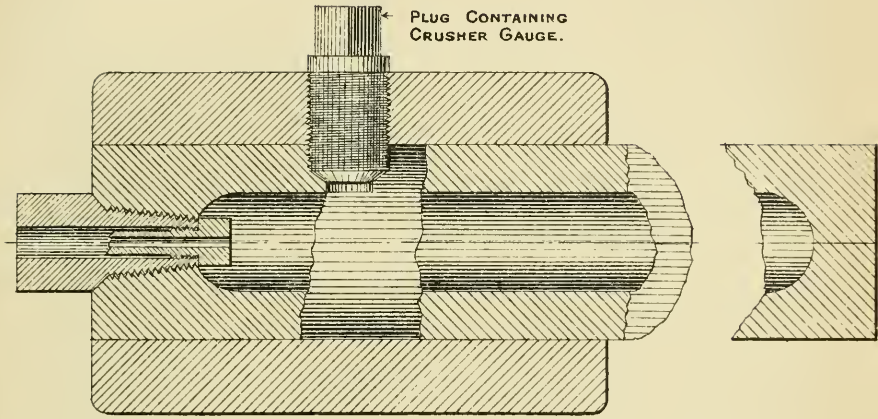
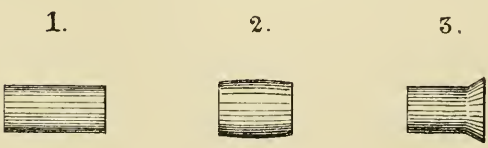


Fig V.

COPPER CYLINDERS.



and I have on the table the internal crusher gauge which was used, and which was also totally destroyed.

The condition of this gauge is very remarkable, and the action on the copper cylinder employed to measure the pressure was one to which I have no parallel in the many thousand experiments I have made with these gauges. The gauge itself is fractured in the most extraordinary way, even in some places to which the gas had no access; and the copper cylinder, which when compressed usually assumes a barrel-like form—that is, with the central diameter larger than that at the ends as shown in the diagram, Fig. V.—in this experiment, and in this only, was bulged close to the piston as you see. It would appear as if the blow was so suddenly given that the laminæ of the metal next the piston endeavoured to escape in the direction of least resistance, that being easier than to overcome the inertia of the laminæ below.

The erosive effect of the new explosives is another point of first rate importance in an artillery point of view. The cordite of the service is not, if the effect be estimated in relation to the energy impressed on the projectiles, more erosive than, for example, brown prismatic, which was itself a very erosive powder; but as we are able to obtain, as you have seen, very much higher energies with cordite than with brown prismatic, the erosion of the former is, for a given number of rounds, materially higher.

There is, however, one striking difference. By the kindness of Colonel Bainbridge, the Chief Superintendent of Ordnance Factories, I am enabled to show you a section of the barrel of a large gun eroded by 137 rounds of gunpowder. Beside it is a barrel of a 4·7-inch quick-firing gun eroded by 1087 rounds of gunpowder and another eroded by 1292 rounds of cordite. You will observe the difference. In the former case the erosion much resembles a ploughed field. In the latter the appearance is more as if the surface were washed away by the flow of the highly heated gases.

But take it in what way you please, the heavy erosion of the guns of the service, if fired with the maximum charges, is a very serious matter, as with the large guns, accuracy, and in a smaller degree energy, are rapidly lost after a comparatively small number of rounds have been fired.

Cordite was first produced for use in small arms only, where, owing to the small charges employed, the question of erosion is not of the same importance as with large guns, but its employment, from the great results obtained with it, was rapidly extended to artillery; and the attention of my friends, Sir F. Abel and Professor Dewar, has for some time been devoted, in conjunction with myself, to investigating whether it is not possible materially to reduce this most objectionable erosion.

With this object I made the following series of experiments.

I had cordite of the same dimensions prepared with varying proportions of nitro-glycerine and gun-cotton. The nitro-glycerine

being successively in the proportions of 60, 50, 40, 30, 20 and 10 per cent., and with each of these cordites I determined the following points:

1. The quantity of permanent gases generated.
2. The amount of aqueous vapour formed.
3. The heat generated by the explosion.
4. The erosive effect of the gases.
5. The ballistic energy developed in a gun and the corresponding maximum pressure.
6. The capacity of the cordite to resist detonation when fired with a strong charge of fulminate of mercury.

The results of these experiments were both interesting and instructive.

To avoid wearying you with a crowd of figures, I have placed on Fig. VI. the results of the first five series of experiments.

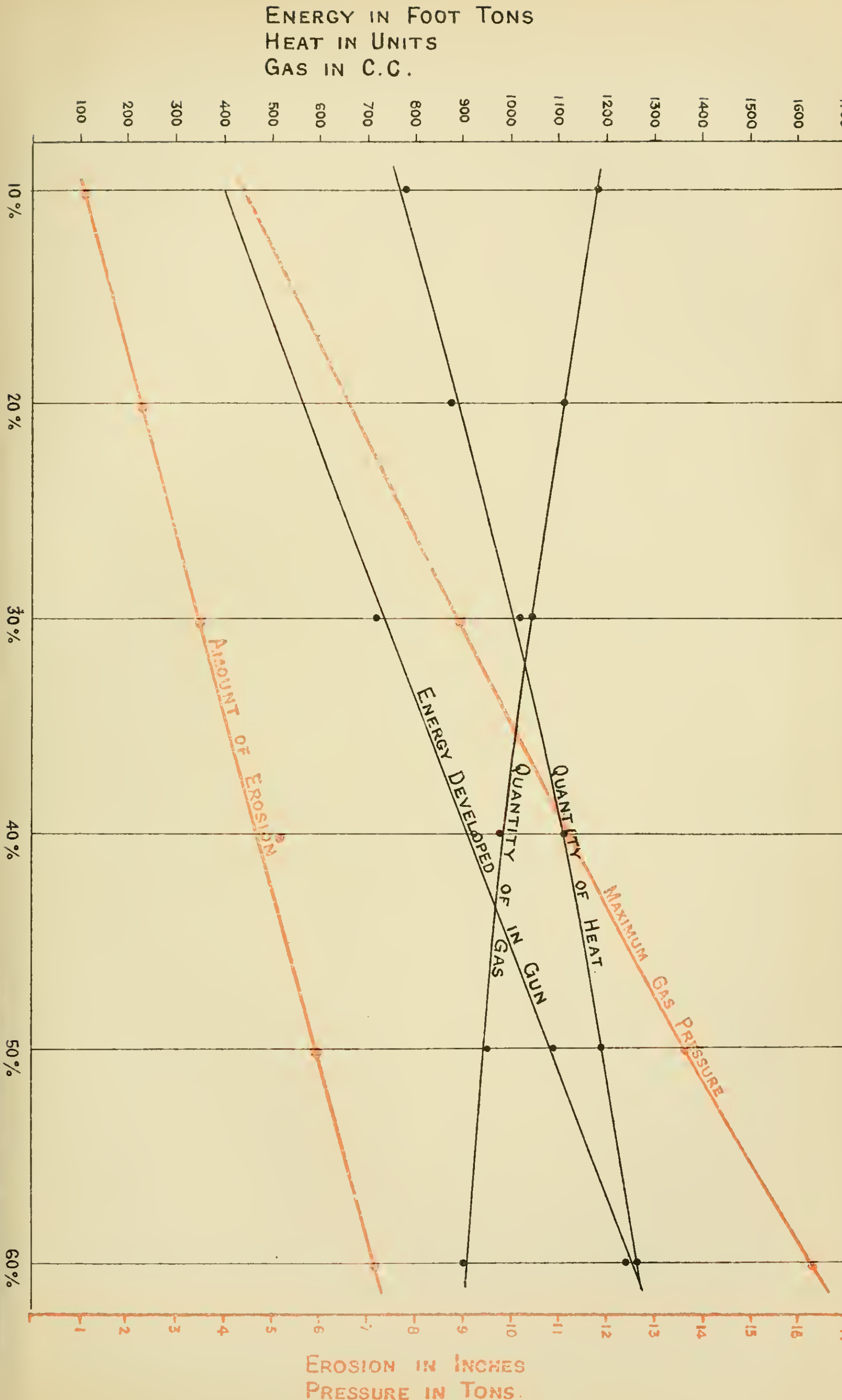
On the axis of abscissæ are placed the percentages of nitro-glycerine, while the ordinates show the quantities of the gases generated, the amount of heat developed, the erosive effect of this explosive, the ballistic energy exhibited in a gun, and the maximum gaseous pressure.

You will note that with the smallest proportion of nitro-glycerine the volume of permanent gases is a maximum, and that the volume steadily decreases with the increase of nitro-glycerine. On the other hand the heat generated as steadily increases with the nitro-glycerine, and if we take the product of the quantity of heat and the quantity of gas, as an approximate measure of the potential energy of the explosive, the higher proportion of nitro-glycerine has an undoubted advantage; but in this case, as in the case of every other explosive with which I have experimented, the potential energies differ less than might be expected from the changes in transformation, as the effect of a large quantity of gas is to a great extent compensated by a great reduction in the quantity of heat generated.

This effect is, of course, easily explained, and was very strikingly exhibited in the much more complicated transformation experienced by gunpowders of different compositions, a long series of which were very fully investigated by Sir F. Abel and myself.

Looking at this diagram you will have observed that the energy developed in the gun is very much smaller with the smaller proportions of nitro-glycerine, but if you will look at the corresponding maximum-pressure curve you will note that the pressures have decreased nearly in like proportion. Hence it is probable that the lower effect is mainly due to a slower combustion of the cordite, and it follows that this effect may be, to a great extent, remedied by increasing the rate of combustion by reducing the diameter of the cordite to correspond with the reduction in the quantity of nitro-glycerine.

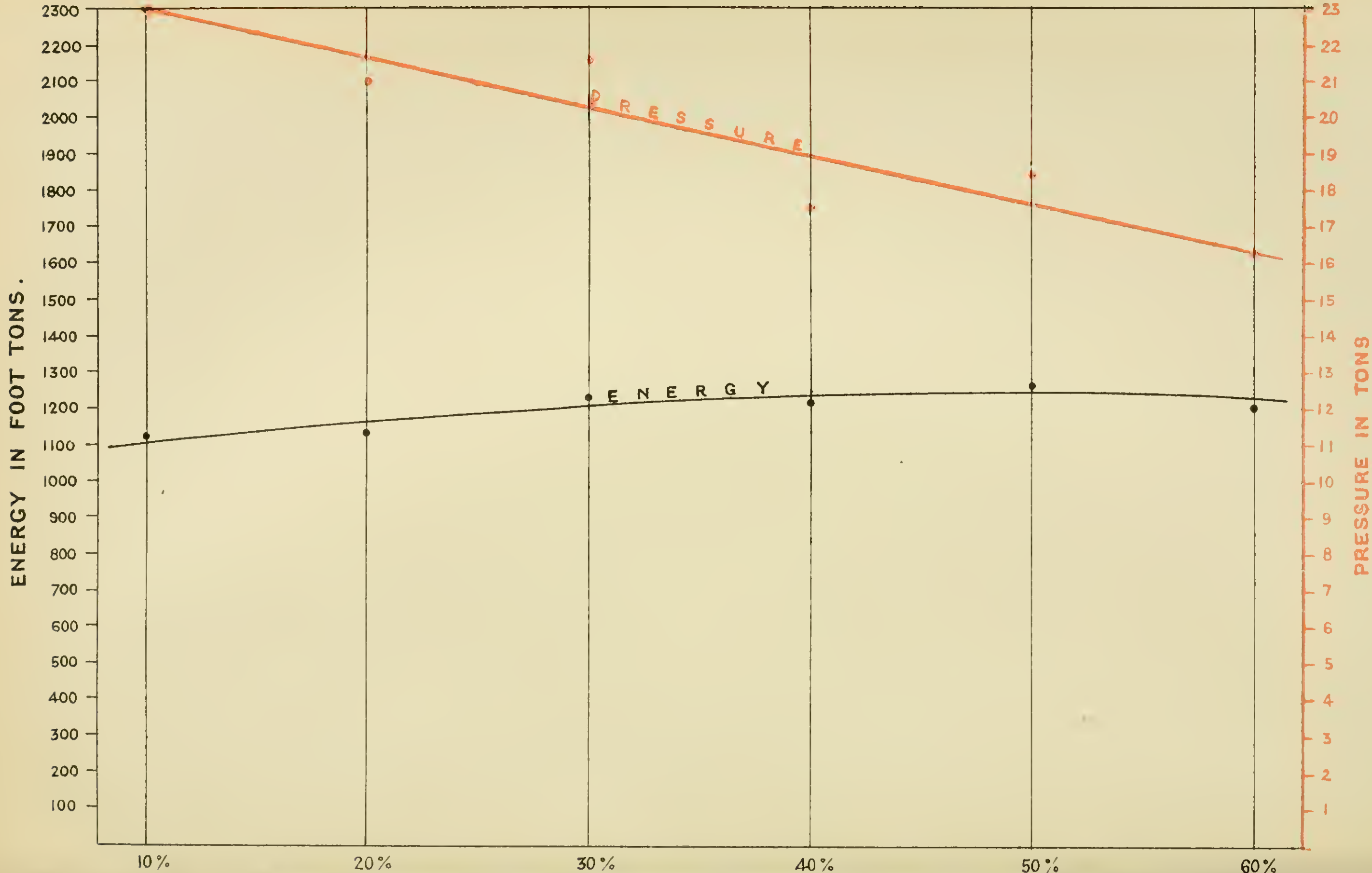
To test this point I caused to be manufactured a second series of cordites of the same composition, but with the diameters successively



Energy in Foot Tons
Heat in Units
Gas in C.C.
100 200 300 400 500 600 700 800 900 1000 1100 1200 1300 1400 1500 1600 1700
10 % 20 % 30 % 40 % 50 % 60 %
Amount of Erosion
Energy Developed in Gun
Quantity of Gas
Quantity of Heat
Maximum Gas Pressure
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17
Erosion in Inches
Pressure in Tons.

Fig. VI.

Fig. VII.


ENERGY IN FOOT TONS.
PRESSURE IN TONS
PRESSURE
ENERGY
100
200
300
400
500
600
700
800
900
1000
1100
1200
1300
1400
1500
1600
1700
1800
1900
2000
2100
2200
2300
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
17
18
19
20
21
22
23
10 %
20 %
30 %
40 %
50 %
60 %

reduced by ·03, as you see with the samples I hold, and this diagram (Fig. VII.) shows at a glance the result. The energies you see are, roughly, practically the same, but if you look at the pressure curve you will observe that I have obtained a curve in which, on the whole, the pressures vary in the contrary direction, that is to say, in this case the pressures increase as the nitro-glycerine diminishes.

Taking the two series into account, they show that by a proper arrangement of amount of charge and diameter of cord, it would be possible to obtain the same ballistics and approximately the same pressure from any of the samples I have exhibited to you.

But I have to draw your attention to another point. From the curve showing the quantities of heat you will note that in passing from 10 per cent. nitro-glycerine to 60 per cent., the heat generated has increased by about 60 per cent. But if you examine the curve indicating the corresponding amount of erosion, you will see that while the quantity of heat is only greater by about 60 per cent., the erosion is greater by nearly 500 per cent.

These experiments entirely confirm the conclusion at which I have previously arrived, viz. that heat is the principal factor in determining the amount of erosion.

In experimenting with a number of alloys of steel, the greatest resistance was shown by an alloy of steel with a small proportion of tungsten, but the difference between the whole of these alloys amounted only to about 16 per cent.

The whole of these cordites were, as I have mentioned, subjected to detonation tests. None of them, so far as my experiments went, exhibited any special tendency in this direction.

I will now endeavour to describe to you a most interesting and important series of experiments which, I regret to say, is still a long way from completion.

The objects of these experiments were (1) to ascertain the time required for the combustion of charges of cordite in which the cordite was of different thicknesses, varying from 0·05 inch to 0·6 of an inch; (2) the rapidity with which the explosives part with their heat to the vessel in which the charge is confined; and (3) to ascertain, if possible, by direct measurement, the temperature of explosion and to determine the relation between the pressure and temperature at pressures approximating to those which exist in the bore of a gun, and which are, of course, greatly above any which have yet been determined.

As regards the first two objects I have named, I have had no serious difficulties to contend with, but as regards the third, I have so far had no satisfactory results, having been unable to use Sir W. Roberts-Austen's beautiful instrument owing to the temperature at the moment of explosion being greatly too high, high enough indeed to melt and volatilise the wires of the thermo junction.

I am, however, endeavouring to make an arrangement by which

I hope to be able to determine these points when the temperature is so far reduced that the wires will no longer be fused.

The apparatus I have used for these experiments is placed on the table. The cylinder in which the explosives were made is too heavy to transport here, but this photograph will sufficiently explain the arrangement. The charge I used is a little more than a kilogramme, and it is fired in this cylinder in the usual manner.

The tension of the gas acting on the piston compresses the spring and indicates the pressure on the scale here shown. But to obtain a permanent record the apparatus I have mentioned is employed.

There is, you see, a drum made to rotate by means of a small motor. Its rate of rotation is given by a chronometer acting on a relay, and marking seconds on the drum, while the magnitude of the pressure is registered by this pencil actuated by the pressure gauge I have just described.

To obtain with sufficient accuracy the maximum pressure, and also the time taken to gasify the explosive, two observations, that is two explosions, are necessary.

If the piston be left free to move the instant of the commencement of pressure, the outside limit of the time of complete explosion will be indicated, but on account of the inertia of the moving parts the pressure indicated will be in excess of the true pressure, and the excess will be, more or less, inversely as the time occupied by the explosion.

If we desire to know the true pressure, it is necessary to compress the gauge beforehand to a point closely approximating to the expected pressure, so that the inertia of the moving parts may be as small as possible—the arrangement by which this is effected is not shown on the diagram, but the gauge is retained at the desired pressure by a wedge-shaped stop, held in its place by the pressure of the spring, and to the stop a heavy weight is attached; when the pressure is relieved by the explosion, the weight falls and leaves the spring free to act.

I have made a large number of experiments with this instrument, both with a variety of explosives and with explosives fired under different conditions. Time will not permit me to do more than to show you on the screen three pairs of experiments to illustrate the effect of exploding cordite of different dimensions, but of precisely the same composition.

I shall commence with rifle cordite. In this diagram (Fig. VIII.) the axis of abscissæ has the time in seconds marked upon it, while the ordinates denote the pressures, and I draw your attention to the great difference, in the initial stage, between the red and the blue curves. You will notice that the red curves show a maximum pressure some $4\frac{1}{2}$ tons higher than that shown by the blue curve, but this pressure is not real. It is due to the inertia of the moving parts. The red and blue curves in a very small fraction of a second come together and remain practically together for the rest of their

Fig. VIII.

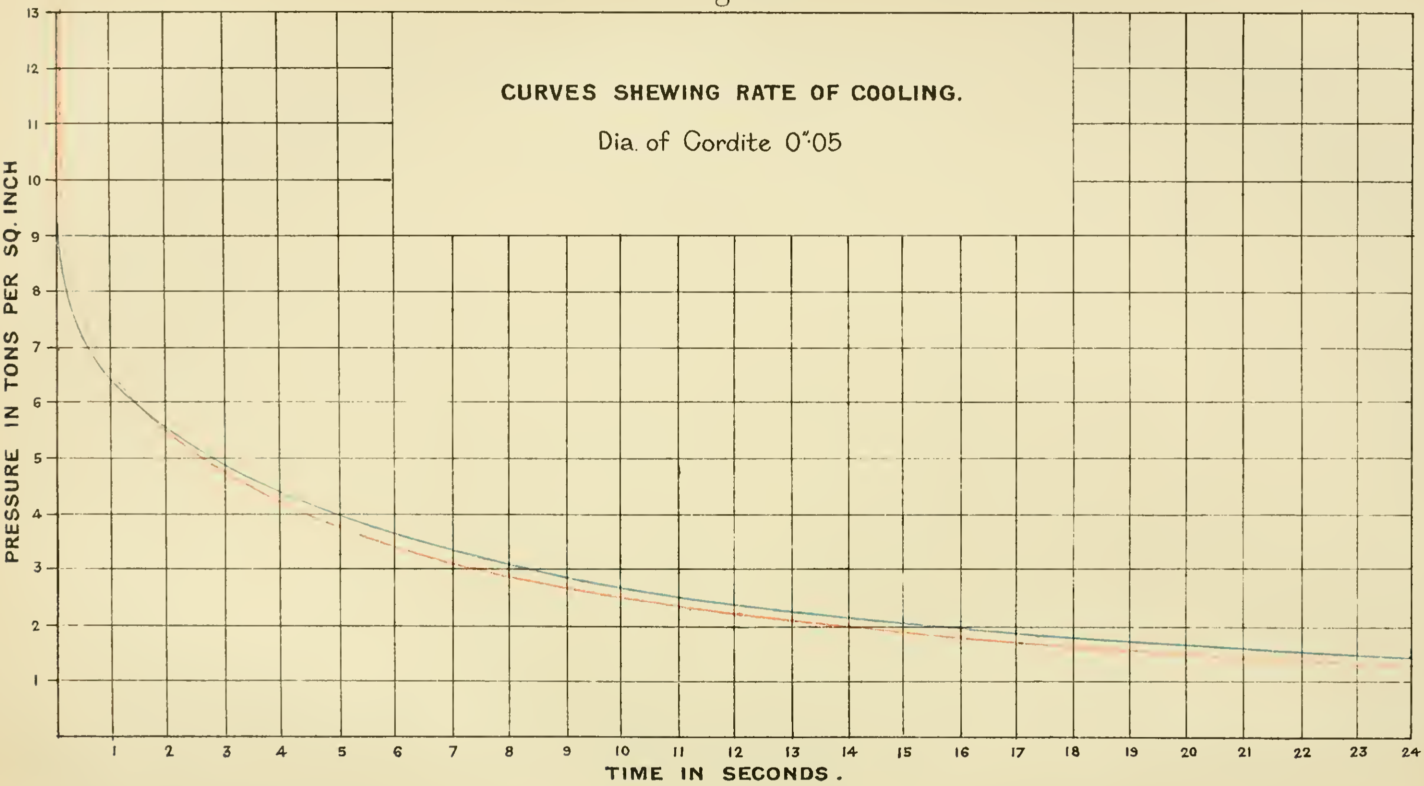
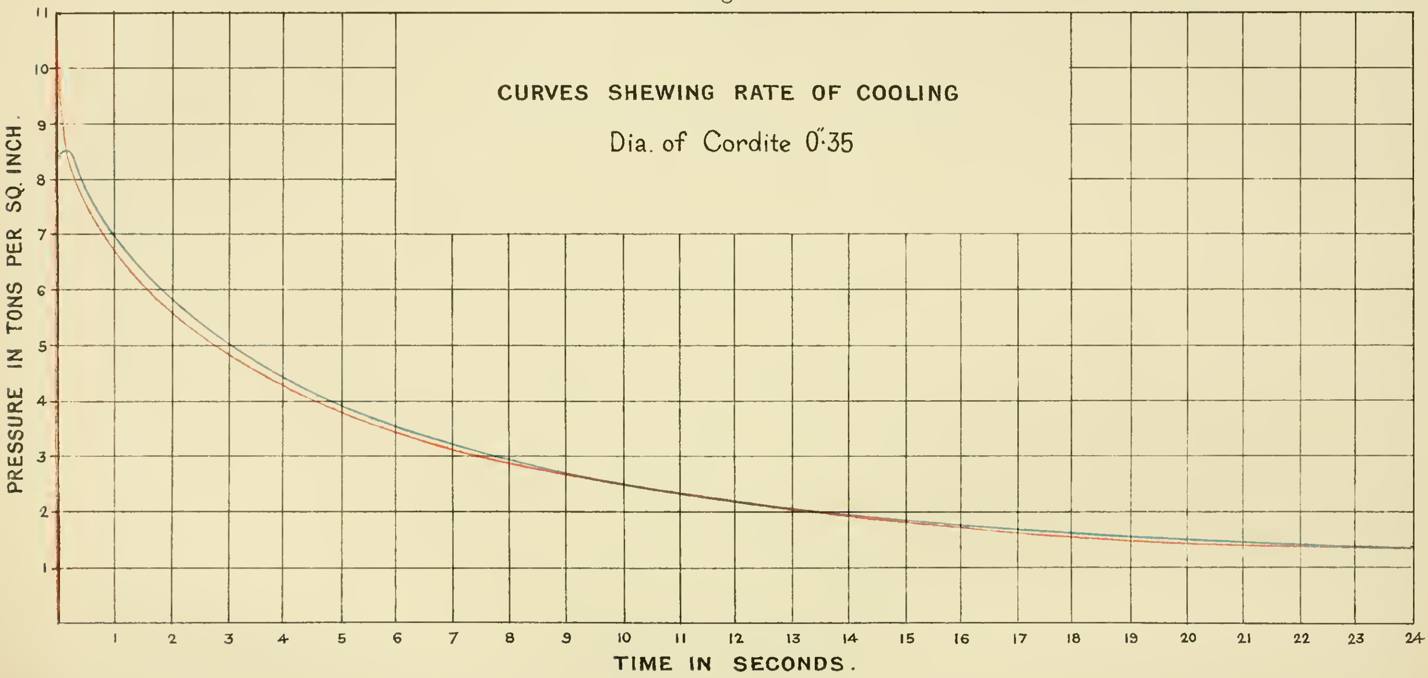

CURVES SHEWING RATE OF COOLING.
Dia. of Cordite 0"·05
PRESSURE IN TONS PER SQ. INCH
1 2 3 4 5 6 7 8 9 10 11 12 13
TIME IN SECONDS.
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24

Fig. IX.



CURVES SHEWING RATE OF COOLING
Dia. of Cordite 0"·35
PRESSURE IN TONS PER SQ. INCH.
TIME IN SECONDS.
1
2
3
4
5
6
7
8
9
10
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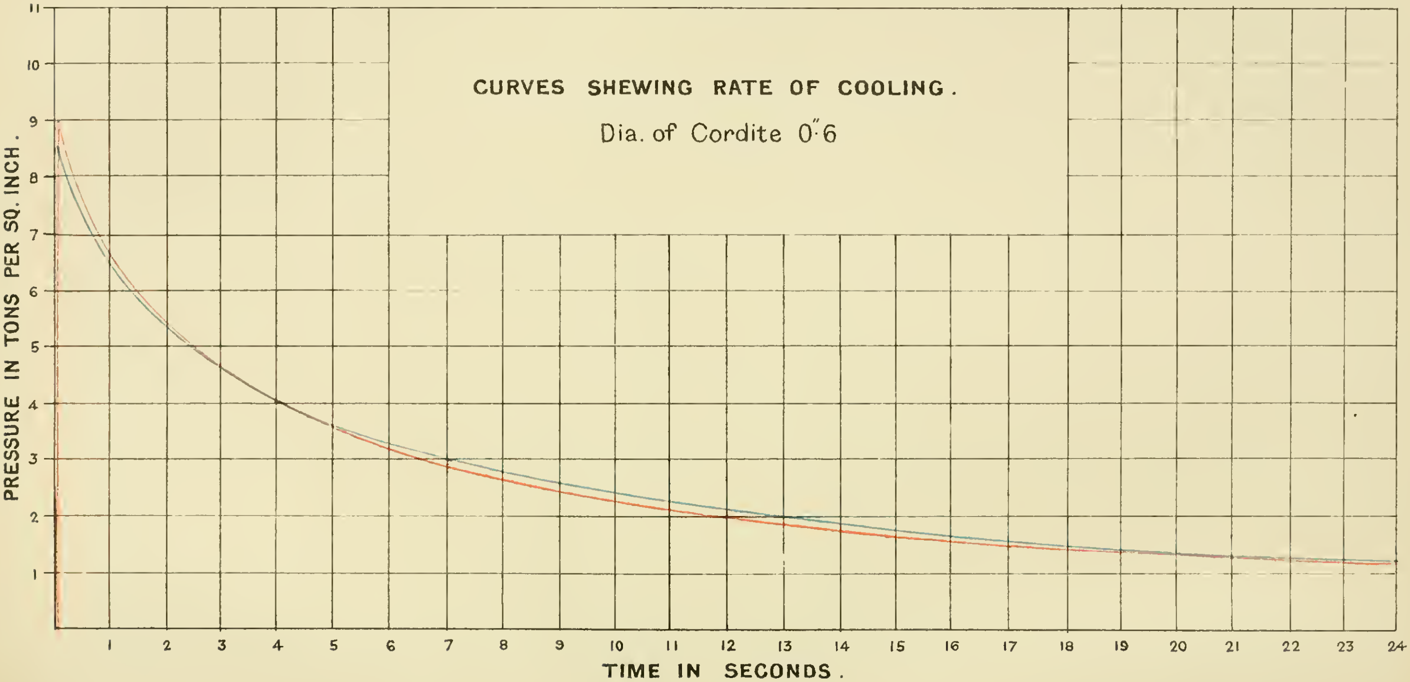
Fig. X


CURVES SHEWING RATE OF COOLING.
Dia. of Cordite 0"·6
PRESSURE IN TONS PER SQ. INCH.
TIME IN SECONDS.

course. The whole of the charge is consumed in something less than fifteen thousandths of a second.

In the case of the blue curve the maximum pressure indicated is obtained in the way I have described, and is approximately correct—about nine tons per square inch. The rapidity with which this considerable charge parts with its heat by communication to the explosion vessel is very striking. In four seconds after the explosion the pressure is reduced to about one-half, and in twelve seconds to about one-quarter.

I now show you (Fig. IX.) similar curves for cordite 0·35 inch in diameter, or about fifty times the rifle cordite section. Here you see that the time taken to consume the charge is longer. The effect of inertia is still very marked, although much reduced. The true maximum pressure is a little over 8·5 tons, but after the first third of a second the two curves run so close together that they are indistinguishable.

Again you see the pressure is reduced by one-half in four seconds, and in a little more than twelve seconds again halved.

The last pair of curves I shall show you (Fig. X.) was obtained with cordite 0·6 inch in diameter, or nearly 150 times the section of the rifle cordite. With this cordite the combustion has been so slow that the effect of inertia almost disappears, it is reduced to about half a ton per square inch; the maximum being nearly the same as in the last set of experiments. The time of combustion indicated I have called slow, but it is about ·06 of a second, and the whole of the experiments show a most remarkable regularity in their rate of cooling, the pressures at the same distance of time from the explosion being in all cases approximately the same, as indeed they ought to be, the density being the same and the explosive the same, the only difference being the time in which the decomposition is completed.

It appears to me that, knowing from the experiments I have described, the volume of gas liberated, its composition, its density, its pressure, the quantity of heat disengaged by the explosion, and knowing all these points with very considerable accuracy, we should be able, from the study of the curves to which I have drawn your attention, and which can be obtained from different densities of gas, to throw considerable light upon the kinetic theory of real, not ideal, gases, at temperatures and pressures far removed from those which have been the subject of such careful and accurate research by many distinguished physicists.

The question, as I have said, involves some very considerable difficulties, nevertheless I am not without hope that the experiments I have been describing may, in some small degree, add to our knowledge of the kinetic theory of gas.

That wonderful theory faintly shadowed forth almost from the commencement of philosophic thought, was first distinctly put forward by Daniel Bernoulli early in the last century. In the latter half of the century now drawing to a close the labours of Joule,

Clausius, Clerk Maxwell, Lord Kelvin, and others have placed the theory in a position analogous and equal to that held by the undulatory theory of light.

The kinetic theory has, however, for us artillerists a special charm, because it indicates that the velocity communicated to a projectile in the bore of a gun is due to the bombardment of that projectile by myriads of small projectiles moving at enormous speeds, and parting with the energy they possess by impact to the projectile.

There are few minds which are not more or less affected by the infinitely great and the infinitely little.

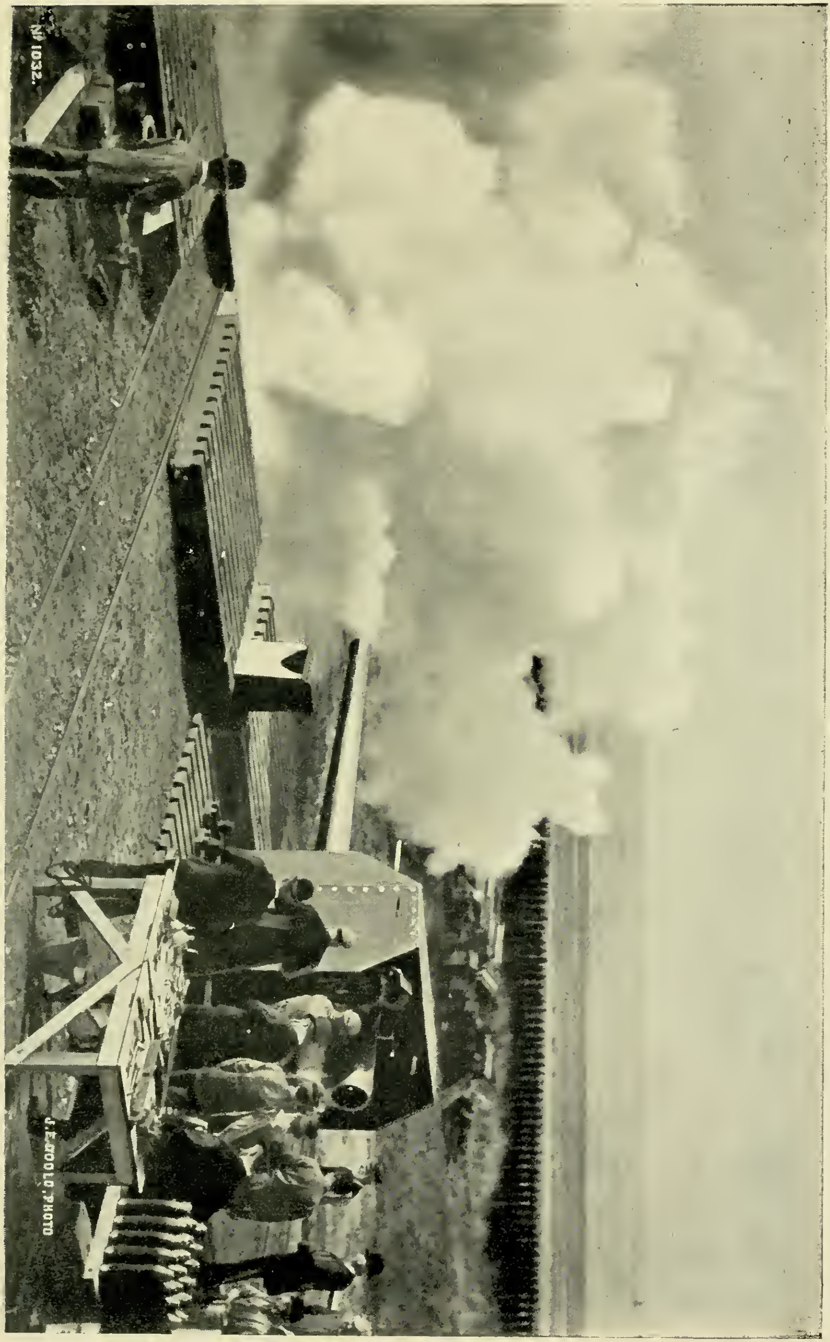
It was said that the telescope which revealed to us infinite space was balanced by the microscope which showed us the infinitely small; but the labours of the men to whom I have referred have introduced us to magnitudes and weights infinitesimally smaller than anything that the microscope can show us, and to numbers which are infinite to our finite comprehension.

Let me draw your attention to this diagram (Fig. II.) showing the velocity impressed upon the projectile, and let me endeavour to describe the nature of the forces which acted upon it to give it its motion. I hold in my hand a cubic centimetre, a cube so small that I daresay it is hardly visible to those at a distance. Well, if this cube were filled with the gases produced by the explosion at 0° C. and atmospheric pressure, there would be something over seven trillions, that is, seven followed by eighteen cyphers, of molecules. Large as these numbers are, they occupy but a very small fraction of the contents of the cubic centimetre, but yet their number is so great that they would, if placed in line touching one another, go round many times the circumference of the earth, a pretty fair illustration of Euclid's definition of a line.

These molecules however are not at rest, but are moving, even at the low temperature I have named, with great velocity, the molecules of the different gases moving with different velocities dependent upon their molecular weight. Thus, the hydrogen molecules, which have the highest velocity, move with about 5500 foot-seconds mean velocity, while the slowest, the carbonic anhydride molecules, have only 1150 foot-seconds mean velocity, or about the speed of sound.

But in the particular gun under discussion, when the charge was exploded there were no less than 20,500 cubic centimetres of gas, and each centimetre at the density of explosion contained 580 times the quantity of gas, that is, 580 times the number of molecules, that I mentioned. Hence the total number of molecules in the exploded charge is $8\frac{1}{3}$ quadrillions, or let us say approximately for the total number eight followed by twenty-four cyphers.

It is difficult for the mind to appreciate what this immense number means, but it may convey a good idea if I tell you that if a man were to count continuously at the rate of three a second, it would take him 265 billions of years to perform the task of counting them.



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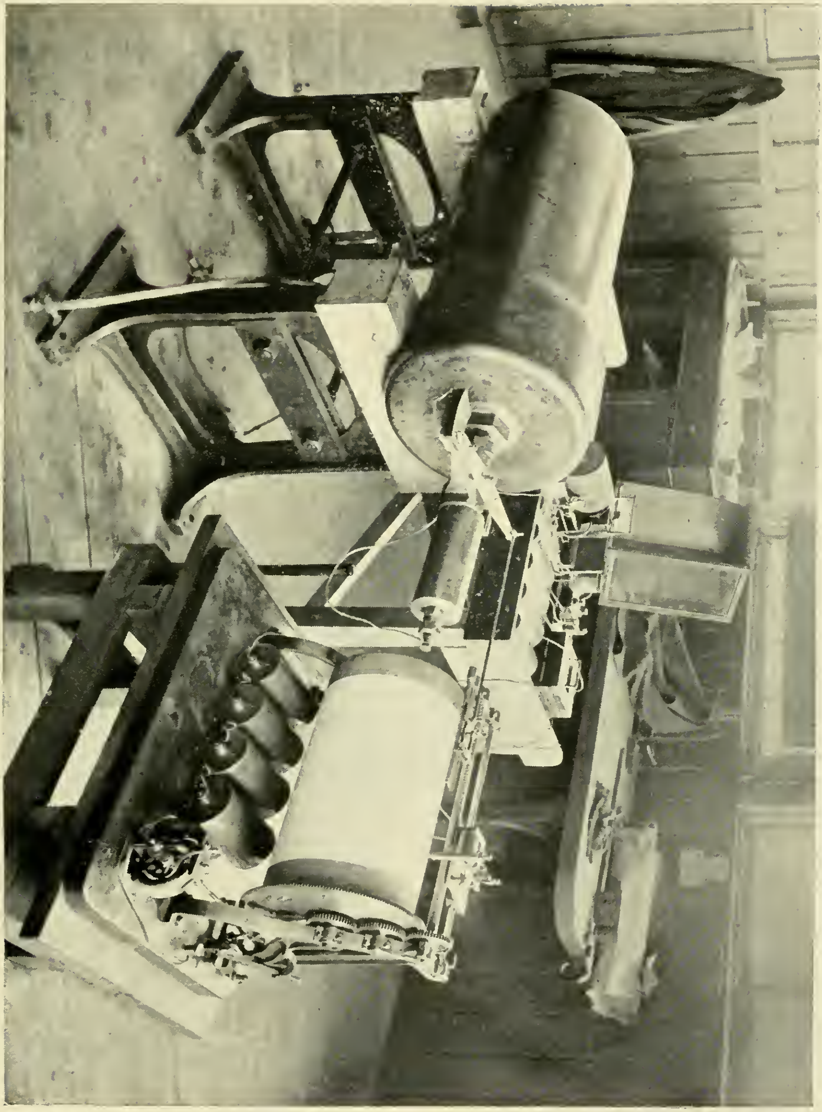
J.E. GOOLD, PHOTO



So much for the numbers; now let me tell you of the velocities with which, at the moment of explosion, the molecules were moving. Taking first the high-velocity gas, the hydrogen, the molecules of the gas would strike the projectile with a mean velocity of about 12,500 foot-seconds. You will observe I say mean velocity, and you must note that the molecules move with very variable velocities. Clerk Maxwell was the first to calculate the probable distribution of the velocities. A little more than one-half will have the mean velocity or less, and about 98 per cent. will have 25,000 foot-seconds or less. A very few, about one in 100 millions, might reach the velocity of 50,000 foot-seconds.

The mean energy of the molecules of different gases at the same temperature being equal, it is easy from the data I have given to calculate the mean velocity of the molecules of the slowest moving gas, carbonic anhydride, which would be about 2600 foot-seconds.

I have detained you, I fear, rather long over these figures, but I have done so because I think they throw some light upon the extraordinary violence that some explosives exhibit when detonated. Take for instance the lyddite shell, exploded by detonation, I showed you earlier in the evening. I calculate that that charge was converted into gas in less than the one 60,000th part of a second, and it is not difficult to conceive the effect that these gases of very high density suddenly generated, the molecules of which are moving with the velocities I have indicated, would have upon the fragments of the shell.

The difference between the explosion of gunpowder fired in a close vessel, and that of gun-cotton or lyddite when detonated, is very striking. The former explosion is noiseless, or nearly so. The latter, even when placed in a bag, gives rise to an exceedingly sharp metallic ring, as if the vessel were struck a sharp blow with a steel hammer.

But I must conclude. I began my lecture by recalling some of the investigations I described in this place a great many years ago. I fear I must conclude in much the same way as I then did, by thanking you for the attention with which you have listened to a somewhat dry subject, and by regretting that the heavy calls made on my time during the last few months have prevented my making the lecture more worthy of my subject and of my audience.

[A. N.]

WEEKLY EVENING MEETING,

Friday, March 30, 1900.

Alfred B. Kempe, Esq., M.A., Treas. R.S., Vice-President, in the Chair.

Professor J. Arthur Thomson, M.A.

Facts of Inheritance.

One of the distinctive features of the nineteenth century has been a reduction in the number of supposed separate powers or entities—the use of William of Occam's razor, in fact. "Caloric" was one of the first to be eliminated, yielding to the modern interpretation of "heat as a mode of motion;" "Light" had to follow, when the undulatory theory of its nature was accepted; a specific "Vital Force" is disowned even by the Neo-vitalists; "Force" itself has become a mere measure of motion; and so on. In view of this progress towards greater precision of phraseology, it cannot be a matter for surprise that a biologist should affirm that to speak of the "Principle of Heredity" in organisms is like speaking of the "Principle of Horologity" in clocks. The sooner we get rid of such verbiage the better for clear thinking, since heredity is certainly no power or force, or principle, but a convenient term for the relation of organic or genetic continuity which binds generation to generation. Ancestors, grandparents, parents are real enough; children and children's children are also very real; heredity is a term for the relation of genetic continuity which binds them together. As for such a question as this, "Is my grandfather's environment my heredity?" it is an offence against Queen's English as much as against scientific phraseology; it should probably read, "Have the structural changes induced by external stimulus on my grandfather's body had any effect on my inheritance?"

Another distinctive feature in scientific progress has been the introduction of precise measurement. It is hardly too much to say that in the development of natural knowledge, science begins where measurement begins. And this is the case in regard to inheritance. As long as we are content to say, "This child takes after his grandfather," "This pigeon shows a throw-back to its rock-dove ancestry," and so on, we may be making interesting remarks, but it is only when we are able to give precise measurements of the amounts of resemblance or difference that we make contributions of real importance to that department of life-lore which deals with inheritance. Or, perhaps, instead of measurement, which may be taken in too

narrow a sense, I should say that precision of observation and record which admits of statistical, mathematical, or some other exact formulation. While nothing can take the place of experiment—which is urgently needed for the further development of our knowledge of heredity—much has been gained by the application of statistical and mathematical methods to biological results—a new contact between different disciplines—which we may particularly associate with the names of Mr. Francis Galton and Mr. Karl Pearson.

I. The Physical Basis of Inheritance.

What was for so long quite hidden from inquiring minds, or but dimly discerned by a few, is now one of the most marvellous of biological commonplaces—that the individual life of the great majority of plants and animals begins in the union of two minute elements—the sperm-cell and the egg-cell. These microscopic individualities unite to form a new individuality, a potential offspring, which will by and by become like to, and yet different from its parents. If we mean by inheritance to include all that the living creature is or has to start with in virtue of its genetic relation to its parents and ancestors, then it is plain that the physical basis of inheritance is in the fertilised ovum. As regards property, there is an obvious distinction between the inheritance and the person who inherits, but there is no such distinction in biology. The fertilised egg-cell *is* the inheritance, and is at the same time the potential inheritor. What might be compared to an inheritance of property as apart from the organism itself is the store of food which may be inside the egg, or round about it.

An organic inheritance means so much, even when we use the magic word potentiality, that although we are quite sure that the germ-cells constitute the physical basis of inheritance, we may consider for a moment the difficulty which rises in the minds of many when they remember that the egg-cell is often microscopic, and the sperm-cell often only $\frac{1}{100000}$th of the ovum's size. Can there be room, so to speak, in these minute elements for the complexity of organisation supposed to be requisite? And the difficulty will be increased if the current opinion be accepted that only the nuclei within the germ-cells are the true bearers of the hereditary qualities. Darwin spoke of the pinhead-like brain of the ant as the most marvellous little piece of matter in the world, but must we not rank as a greater marvel the microscopic germ-cells which contained potentially all the inherited qualities of that ant, or of that man?

Nowhere more than in biology is one made to feel that a little may go a long way. A microbic spore invisible even with a fairly good microscope may kill a man. From one microscopic egg of a sea-urchin cut into three, Delage reared three larvæ. In another case he says that he reared an embryo from a $\frac{1}{37}$th fraction of an egg.

We know of twin animals developed from one egg, but what shall we say of the quadruplets Wilson obtained by shaking apart the four-cell stage in the development of the lancelet, or of the "legion of embryos" which Marchal describes as developing from a single ovum of a peculiar Hymenopterous insect *Encyrtus?* In development, indeed, a half may be as good as a whole.

In reference to the difficulty raised in some minds by the minuteness of the physical basis, it may be recalled that the students of physics, who make theories regarding the sizes of atoms and molecules, which they have invented, tell us that the image of a Great Eastern filled with framework as intricate as that of the daintiest watches does not exaggerate the possibilities of molecular complexity in a spermatozoon, whose actual size may be less than the smallest dot on the watch's face. Secondly, as we learn from embryology that one step conditions the next and that one structure grows out of another, we are not forced to stock the microscopic germ-cells with more than initiatives. Thirdly, we must remember that the development implies an interaction between the growing organism and a complex environment without which the inheritance would remain unexpressed, and that the full-grown organism includes much that was not inherited at all, but has been acquired as the result of nurture or external influence.

The central problem of heredity is to form some conception of what we have called the relation of genetic continuity between successive generations; the central problem of inheritance is to measure the resemblances and differences in the hereditary characters of successive generations, and to arrive, if possible, at some formula, which will sum up the facts. It is inexpedient to lay on the shoulders of the student of heredity the burden of problems, which are not in any special sense his business. It is no doubt interesting to ask how an organisation supposed to be very complex, may be imagined to find physical basis in a microscopic germ-cell, but the same sort of question may be raised in regard to a ganglion-cell. It is not distinctively a problem of heredity. It is interesting to inquire into the orderly and correlated succession of events by which the fertilised egg-cell gives rise to an embryo, but this is the unsolved problem of physiological embryology. It raises questions distinct from those of heredity and inheritance, and apparently much less soluble.

In the preformationist theories, which held sway in the seventeenth and eighteenth centuries—theories which asserted the pre-existence of the organism and all its parts, in miniature, within the germ—there was a kernel of truth well concealed within a thick husk of error. For we may still say, as the preformationists did, that the future organism is implicit in the germ, and that the germ contains not only the rudiment of the adult organism, but the potentiality of successive generations as well. But what baffled the earlier investigators was the question how the germ-cell comes to have this ready-made organisation, this marvellous potentiality.

Discovering no natural way of accounting for this, the majority fell back upon a hypothesis of hyperphysical agencies, that is to say, they abandoned the scientific method and drew cheques upon that bank where credit is unlimited as long as credulity endures.

An attempt to solve the difficulty which confronted the preformationists—the difficulty of accounting for the complex organisation presumed to exist in the germ-cell—is expressed in a theory which seems to have occurred at intervals in the long period between Democritus and Darwin, the theory of pangenesis. On this theory, the cells of the body are supposed to give off characteristic and representative gemmules: these are supposed to find their way to the reproductive elements, which thus come to contain, as it were, concentrated samples of the different components of the body, and are therefore able to develop into an offspring like the parent. The theory involves many hypotheses, and is avowedly unverifiable in direct sense-experience, but the same might be said about many other theories. It is perhaps more to the point to notice that there is another theory of heredity which is, on the whole, simpler, which seems, on the whole, to fit the facts better, especially the fact that our experience does not warrant the conclusion that the modifications or acquired characters of the body of the parent affect in any specific and representative way the inheritance of the offspring.

As is well known, the view which many, if not most biologists now take of the uniqueness of the germ-cells is rather different from that of pangenesis. It is expressed in the phrase "germinal continuity," and has been independently suggested by several thinkers, though Weismann has the credit of working it out into a theory. Let me recall its purport. There is a sense, as Mr. Galton says, in which the child is as old as the parent, for when the parent's body is developing from the fertilised ovum, a residue of unaltered germinal material is kept apart to form the future reproductive cells, one of which may become the starting-point of a child. In many cases, scattered through the animal kingdom, from worms to fishes, the beginning of the lineage of germ-cells is demonstrable in very early stages before the differentiation of the body-cells has more than begun. In the development of the threadworm of the horse, according to Boveri, the very first cleavage divides the fertilised ovum into two cells, one of which is the ancestor of all the body-cells, and the other the ancestor of all the germ-cells. In other cases, particularly among plants, the segregation of germ-cells is not demonstrable until a relatively late stage. Weismann, generalising from cases where it seems to be visibly demonstrable, maintains that in all cases the germinal material which starts an offspring, owes its virtue to being materially continuous with the germinal material from which the parent or parents arose. But it is not on a continuous lineage of recognisable germ-cells that Weismann insists, for this is often unrecognisable, but on the continuity of the germ-plasm—that is of a specific substance of definite chemical and

molecular structure which is the bearer of the hereditary qualities. In development a part of the germ-plasm, "contained in the parent egg-cell, is not used up in the construction of the body of the offspring, but is reserved unchanged for the formation of the germ-cells of the following generation." Thus the parent is rather the trustee of the germ-plasm than the producer of the child. In a new sense, the child is a chip of the old block. Early segregation of the germ-cells is in many cases an observable fact—and doubtless the list of such cases will be added to—the conception of a germ-plasm is hypothetical, just as the conception of a specific living stuff or protoplasm is hypothetical. In the complex microcosm of the cell, we cannot point to any one stuff and say "this is protoplasm"; it may well be that vital activity depends upon several complex stuffs which, like the members of a carefully constituted firm, are characteristically powerful only in their inter-relations. In the same way, it must be clearly understood that we cannot demonstrate the germ-plasm, even if we may assume that it has its physical basis in the stainable nuclear bodies or chromosomes. The theory has to be judged, like all conceptual formulæ, by its adequacy in fitting facts.

Let us suppose that the fertilised ovum has certain qualities, $a, b, c, \ldots x, y, z$; it divides and re-divides, and a body is built up; the cells of this body exhibit division of labour and differentiation, losing their likeness to the ovum and to the first results of its cleavage. In some of the body-cells the qualities a, b find predominant expression, in others the qualities y, z, and so on. But if, meanwhile, there be certain germ-cells which do not differentiate, which retain the qualities $a, b, c, \ldots x, y, z$, unaltered, which keep up, as one may say figuratively, "the protoplasmic tradition," these will be in a position by and by to develop into an organism like that which bears them. Similar material to start with, similar conditions in which to develop: *therefore*, like tends to beget like.

May we think for a moment of a baker who has a very precious kind of leaven, and some less precious material on which this may work; he uses both in baking a large loaf; but he so arranges matters by a clever contrivance that part of the original leaven is always carried on unaltered, carefully preserved for the next baking. Nature is the baker, the loaf is a body, the leaven is the germ-plasm, and each baking is a generation.

II. Dual Nature of Inheritance.

Apart from exceptional cases, the inheritance of a multicellular animal or plant is dual, part of it comes from the mother and part of it from the father; the beginning of the new individuality is a fertilised egg-cell. The exceptions referred to are cases of asexual multiplication by buds or otherwise, as in the freshwater Hydra; cases of parthenogenesis, as in the case of the unfertilised eggs which

develop into green fly in the summer; and cases like liver-flukes, where an animal is both mother and father to its offspring. Apart from these exceptions the inheritance does at the start consist of maternal and paternal contributions in intimate and orderly union.

Prof. E. B. Wilson states the general opinion of experts somewhat as follows:—As the ovum is much the larger, it is believed to furnish the initial capital—including it may be a legacy of food-yolk—for the early development of the embryo. From both parents alike comes the inherited organisation which has its seat (according to many) in the readily stainable (chromatin) rods of the nuclei. From the father comes a little body (the centrosome) which organises the machinery of division by which the egg splits up, and distributes the dual inheritance equally between the daughter-cells.

Recent discoveries have shown that the paternal and maternal contributions which come together in fertilisation, are for several divisions at least exactly divided among the daughter-cells, thus confirming a prophecy which Huxley made in 1878: "It is conceivable, and indeed probable, that every part of the adult contains molecules derived both from the male and from the female parent; and that, regarded as a mass of molecules, the entire organism may be compared to a web of which the warp is derived from the female and the woof from the male." "What has since been gained," Prof. Wilson says, "is the knowledge that this web is to be sought in the chromatic substance of the nuclei, and that the centrosome is the weaver at the loom."

In regard to these conclusions I wish to make three remarks. (*a*) Although inheritance is dual, it is in quite as real a sense multiple, from ancestors through parents, as we shall afterwards see. (*b*) If Loeb is able to induce artificial parthenogenesis in sea-urchins' eggs exposed for a couple of hours to sea-water to which some magnesium chloride has been added; if Delage is able to fertilise and to rear normal larvæ from non-nucleated ovum-fragments of sea-urchin, worm and mollusc, we should be chary of committing ourselves definitely to the conclusion that the nuclei are the exclusive bearers of the hereditary qualities, or that both must be present in all cases. Furthermore, the fact that an ovum without any sperm-nucleus, or an ovum-fragment without any but a sperm-nucleus, can develop into a normal larva points to the conclusion, probable also on other grounds, that each germ-cell, whether ovum or spermatozoon, bears a complete equipment of hereditary qualities. (*c*) It must be carefully observed that our second fact does not imply that the dual nature of inheritance must be patent in the full-grown offspring, for hereditary resemblance is often strangely unilateral, the characters of one parent being "prepotent" as we say, over those of another.

III. Different Degrees of Hereditary Resemblance.

Before the middle of the century considerable attention was paid to what might be called the demonstration of the general fact of inheritance. In a big treatise like that of Prosper Lucas (1847) many hundreds of pages are devoted to proving what we now take for granted—that the present is the child of the past, that our start in life is no haphazard affair, but is rigorously determined by our parents and ancestors, that various peculiarities, normal and abnormal, physical and mental, may re-appear generation after generation, and so on. One step of progress during the Darwinian era has been the recognition of inheritance as a fact of life which requires no further proof.

Yet this aspect of the study of heredity is by no means worked out. Thus there are some characters, e.g. tendencies to certain diseased conditions, which are more frequently transmitted than others, and we ought to have, in each case, precise statistics as to the probabilities of transmission.

Again, there are some subtle qualities whose heritability must not be assumed without evidence. Thus it is of very great importance to students of organic evolution that Prof. Karl Pearson has recently supplied, for certain cases, definite proof of the inheritance of fecundity, fertility and longevity.

The familiar saying, "like begets like," should rather read, "like tends to beget like," since variation is quite as important a fact as complete hereditary resemblance. If it seems to us that in many cases the offspring is practically a facsimile reproduction of the parent, this may be due to absence of variation, or, what comes almost to the same thing, to great completeness of inheritance ; but it is more likely to be due to our ignorance, to our inability to detect the idiosyncrasies.

But it will be granted by all that the completeness with which the characters of race, genus, species and stock are reproduced generation after generation, is one of the large facts of inheritance. It is obvious, however, that this does not sum up our experience, and we must face the task of considering what may be called the different degrees of hereditary resemblance. For these a confused classification and a troublesome terminology have been suggested, to discuss which would be most unprofitable in the limits of a short lecture.

I therefore propose to restrict attention to three familiar cases, which are called blended, exclusive, and particulate inheritance, and then to say a few words in regard to the phenomena known as regression, reversion and atavism.

A preliminary consideration must be attended to. It is a matter of observation that there are great differences in the degree in which offspring resemble their parents ; but it is surely a matter of conjecture that lack of resemblance is necessarily due to incompleteness

in the inheritance. Indeed, the fact that the resemblance so often re-appears in the third generation, makes it probable that the incompleteness is not in the inheritance, but simply in its expression. The characters which seem to be absent, to "skip a generation," as we say, are probably part of the inheritance, as usual. But they remain latent, neutralised, silenced (we can only use metaphors) by other characters, or else unexpressed because of the absence of the appropriate stimulus.

We can imagine the son of a lavish millionaire reacting to plain living; we can imagine the superficial supposition that the money had been lost; and we can imagine the complete contradiction of this inference in the third generation.

(*a*) In *blended* inheritance, the characters of the two parents, e.g. in regard to a particular structure, such as the colour of the hair, may be intimately combined in the offspring. This is particularly well seen in some hybrids, where the offspring seems like the mean of the two parents; it is probably the most frequent mode of inheritance.

(*b*) In *exclusive* inheritance, the expression of maternal or of paternal characters in relation to a given structure, such as eye-colour, is suppressed. Sometimes the unilateral resemblance is very pronounced, and we say that the boy is "the very image of his father," or the daughter "her mother over again"; though even more frequently the resemblance seems "crossed," the son taking after the mother, and the daughter after the father. Our emphasis on the distinction between inheritance and the expression of inheritance is surely warranted by cases on record where the young boy resembled the mother and the girl the father; but when they came of age, the likeness was reversed, i.e. formerly obscure resemblances became dominant.

(*c*) It seems convenient to have a third category for cases where there is neither blending nor exclusiveness, but where in the expression of a given character, part is wholly paternal and part wholly maternal. This is called *particulate* inheritance. Thus, an English sheep-dog may have a paternal eye on one side, and a maternal eye on the other. Suppose the parents of a foal to be markedly light and dark in colour; if the foal is light brown the inheritance in that respect is blended, if light or dark it is exclusive, if piebald it is particulate. In the last case there is in the same character an exclusive inheritance from both parents.

The numerous experiments on hybridisation made by botanists, zoologists, and more practical people, have led us to expect one of three results when a crossing has a successful issue. (1) The hybrid may be intermediate between its parents, sometimes so exactly that we may liken the blending, not merely to warp and woof, but to a mingling of two colours; (2) the hybrid may show an exaggeration of the characters of one parent, often with little apparent realisation of the peculiarities of the other. These correspond to blended and exclusive inheritance in ordinary cases of mating within the same

breed. But (3) the hybrid may also be very different from either parent, showing features which appear to be quite novel, or which on close investigation are seen to be interpretable as the reassertion of the characters of a remoter ancestor. In short, it may show either a new variation or a reversion. The extraordinary fact is that at least two of these different results may be illustrated in one brood or litter of hybrids.

The facts above referred to may be considered in another aspect, in terms of what is called the quality of prepotency, with which breeders have been for a long time familiar. The term refers to the fact that in the development of a character the paternal or the maternal qualities may predominate, as in unequal blending where there is relative prepotency, or in exclusive inheritance where the prepotency in respect to a given character is absolute. It seems doubtful whether we gain much by using the word, since all these general terms are apt to form the dust particles of intellectual fog, but what we have to do with is the fact that in respect to certain characters the paternal inheritance seems more potent than the maternal, or *vice versa*. Thus in man the father is usually prepotent in the matter of stature, and breeders give many instances where certain, even trivial, characters of sire or dam reappear persistently in the offspring irrespective of the nature of the other parent.

It seems that one of the ways in which the quality of prepotency may be developed is by inbreeding, as Prof. Ewart and others have maintained. "Some breeders say that they can produce a horse so prepotent, so fixed by interbreeding (inbreeding) that it will produce its like however mated"; and there is much evidence to show that, of two parents, the more inbred—up to a certain limit of stability—is likely to have the greater influence on the offspring.

As inbreeding may be frequent in nature, especially among gregarious and isolated groups, and as it tends to develop prepotency, we are able to understand better how new variations may have been fixed in the course of evolution. And we can better understand the position maintained by Reibmayr, that the evolution of a human race implies alternating periods of dominant inbreeding, and dominant cross-breeding. The inbreeding gives fixity to character, the cross-breeding averts degeneracy and stimulates new variations which form the raw material of progress. The Jews, especially in isolated colonies, may serve to illustrate persistent inbreeding, which we may contrast with the complex cross-breeding at present conspicuous in America.

Until we have more precise statistical data in regard to blended, exclusive, and particulate inheritance, we cannot hope to simplify the matter with any security. But perhaps a unified view will be found in the theoretical conception of a germinal struggle in the arcana of the fertilised ovum—a struggle in which the maternal and paternal contributions may blend and harmonise, or may neutralise one another, or in which one may conquer the other, or in which both

may persist without combining. We have extended the wide conception of the struggle for existence in many directions; it may be between organisms akin or not akin, between plants and animals, between organisms and their inanimate environment, between the sexes, between the different parts of the body, between the ova, between the spermatozoa, between the ova and the spermatozoa, and Weismann has suggested that it may also be between the constituents of the germ-plasm.

IV. Regression.

We have already referred to the fact which stares us in the face that there is a sensible stability of type from generation to generation. "The large," Mr. Galton says, "do not always beget the large, nor the small the small; but yet the observed proportion between the large and the small, in each degree of size and in every quality, hardly varies from one generation to another." In other words, there is a tendency to keep up a specific average. This may be partly due to the action of natural elimination, weeding out abnormalities, often before they are born. But it is to be primarily accounted for by what Mr. Galton calls the fact of "filial regression." Let me take an instance from Mr. Pearson's 'Grammar of Science.' Take fathers, of stature 72 inches, the mean height of their sons is 70·8, we have a regression towards the mean of the general population. On the other hand, fathers with a mean height of 66 inches give a group of sons of mean height 68·3 inches, again nearer the mean. "The father with a great excess of the character contributes sons with an excess, but a less excess of it; the father with a great defect of the character contributes sons with a defect; but less of it."

As Mr. Galton puts it, society moves as a vast fraternity. The sustaining of the specific average is certainly not due to each individual leaving his like behind him, for we all know that this is not the case. It is due to a regression which tends to bring the offspring of extraordinary parents nearer the average of the stock. In other words, children tend to differ less from mediocrity than their parents.

This big average fact is to be accounted in terms of that genetic continuity which makes an inheritance not dual, but multiple. "A man," says Mr. Pearson, "is not only the product of his father, but of all his past ancestry, and unless very careful selection has taken place, the mean of that ancestry is probably not far from that of the general population. In the tenth generation a man has [theoretically] 1024 tenth great-grandparents. He is eventually the product of a population of this size, and their mean can hardly differ from that of the general population. It is the heavy weight of this mediocre ancestry which causes the son of an exceptional father to regress towards the general population mean; it is the balance of this sturdy

commonplaceness which enables the son of a degenerate father to escape the whole burden of the parental ill."

At this point one should discuss reversion or atavism, but it is exceedingly difficult to get a firm basis of fact. I use the term reversion to include cases where through inheritance there re-appears in an individual some character which was not expressed in his parents, but which did occur in an ancestor. I say advisedly "through inheritance," in order to exclude those cases where the re-appearance of the character can be accounted for in some other way. The term thus defined is a very wide one, and not very fortunate, but it is difficult to get rid of. I use it to refer to abnormal as well as normal characters, even to include the re-appearance of characters, the normal occurrence of which is outside of the limits of the race altogether, i.e. in some phyletically older race. In other words, the character whose re-appearance is called a reversion may be found within the verifiable family, within the breed, within the species, or even in a presumed ancestral species.*

The best illustrations of reversion are furnished by hybrids. Thus in one of Prof. Cossar Ewart's experiments a pure white fantail cock pigeon, of old-established breed, which in colour had proved itself prepotent over a blue pouter, was mated with a cross previously made between an owl and an archangel, which was far more of an owl than an archangel. The result was a couple of fantail-owl-archangel crosses, one resembling the Shetland rock-pigeon, and the other the blue rock of India. Not only in colour, but in shape, attitude and movements there was an almost complete reversion to the form which is believed to be ancestral to all the domestic pigeons. The only marked difference was a slight arching of the tail. Similar results were got with fowls and rabbits.

But great carefulness is necessary in arguing from the results of hybridisation, to those of ordinary mating, and even if some of the phenomena of exclusive inheritance seem to show reversion to a near ancestor we need a broader basis of fact than we have at present before we can formulate any law. It is impossible to read the recorded cases without becoming convinced that many phenomena are labelled reversions on the flimsiest evidence. Thus the occurrence of a Cyclopean human monster with a median eye has been called a reversion to the sea-squirt, and gout has been called a reversion to the reptilian condition of liver and kidneys. Often

* Professor Karl Pearson defines a *reversion* as "the full reappearance in an individual of a character which is recorded to have occurred in a *definite* ancestor of the same race," and *atavism* as "a return of an individual to a character not typical of the race at all, but found in allied races supposed to be related to the evolutionary ancestry of the given race." "In reversion we are considering a variation, normal or abnormal, from the standpoint of *heredity in the individual*; in atavism we are considering an abnormal variation from the standpoint of the *ancestry of the race*." But as the two words seem to be used by some authors in the converse way, or as equivalent, and as it is surely difficult to define the field of abnormal variation, I have adhered to the wider usage.

there is not the slightest attempt to discriminate between true reversion (i.e. the re-expression of latent ancestral characters) and the phenomena of arrested development, or of abnormalities which have been induced from without. Often, too, there has been no scruple in naming or inventing the ancestor to whom the reversion is supposed to occur, although evidence of the pedigree is awanting; and the vicious circle is not unknown of arguing to the supposed ancestor from the supposed reversion, and then justifying the term reversion from its resemblance to the supposed ancestor. Little allowance has been made for coincidence, and the postulate of characters remaining latent for millions of years is made as glibly as if it were just as conceivable as a throw-back to a great-grandfather.

I do not see any way out of the theory that characters may lie latent for a generation or for generations, or in other words that certain potentiabilities or initiatives which form part of the heritage may remain unexpressed for lack of the appropriate liberating stimulus, or for other reasons, or may have their normal expression disguised. The drone bee has a mother, the queen, but no father, for the eggs which develop into drones are not fertilised, yet his structure differs from that of the queen in other points besides those immediately related with sex, and he may in his turn be the father of future queens and workers. At the same time it does not follow that the re-appearance of an ancestral character not seen in the parents is necessarily due to the re-assertion of latent elements in the inheritance. It may be a case of ordinary regression; it may be a case of arrested development; it may be an extreme variation whose resemblance to an ancestral characteristic is a coincidence; it may be an individually acquired modification, reproduced apart from inheritance, by a recurrence of suitable external conditions, and so on. In short, what are called reversions are probably in many cases misinterpretations.

V. Galton's Law.

The most important general conclusion which has yet been reached in regard to inheritance is formulated in Galton's Law. Mr. Galton was led to it by his studies on the inheritance of human qualities, and more particularly by a series of studies on Basset hounds. It is one of those general conclusions which have been reached statistically, and I must refer for the evidence and also for its strictest formulation to the revised edition of Mr. Pearson's 'Grammar of Science.'

As we have seen, it is useful to speak of a heritage as dual, half derived from the father and half from the mother. But the heritable material handed on from each parent was also dual, being derived from the grandparents. And so on, backwards. We thus reach the idea that a heritage is not merely dual, but in a deeper sense multiple.

Though a comparison with the inheritance of property cannot be

exact, we may fancy a youth inheriting an estate, in regard to which it might be said that half of it had belonged to his father, and half of it to his mother, yet one with a full knowledge of the family history and the gradual acquisition of the property, might be able to make the story of the heritage much more interesting by showing how this meadow was due to a grandmother and that forest to a great-grandfather.

To appreciate the possible complexity of our mosaic inheritance we must recall the number of our ancestors. We have two parents, four grandparents, eight great-grandparents, about sixteen great-great-grandparents, and so on. "If," as Prof. Milnes Marshall said, "we allow three generations to a century, there will have been twenty-five since the Norman Invasion, and a man may be descended not merely from one ancestor who came over in 1066, but directly and equally from over sixteen million ancestors who lived at or about that date." But on these theoretical lines the existence of one man to-day would involve the existence of nearly seventy thousand millions of millions of ancestors at the commencement of the Christian era. Which is absurd, for it overlooks the frequent occurrence of close inter-marriage, of cousins for instance.

The problem of reduction in the number of ancestors has been very carefully discussed by genealogists like Prof. Lorenz and Dr. F. T. Richter, but we should soon lose ourselves in the discussion. We must be content to take one example. Theoretically, Kaiser Wilhelm II. might have had in the direct line the number of ancestors indicated in the upper row of the following scheme; the second row indicates the number actually known, on to the twelfth generation; the third row gives the number of those possible ancestors of whose existence there is deficient record; and the fourth row gives the probable total.

Generations.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.	XII.
(1) Theoretical Number	2	4	8	16	32	64	128	256	512	1024	2048	4096
(2) Actual number known	2	4	8	14	24	44	74	111	162	206	225	275
(3) Inadequately known	.	.	.	.	.	.	.	5	15	50	117	258
(4) Probable total	.	.	.	.	.	.	.	116	177	256	342	533

According to Galton's law, "the two parents between them contribute *on the average* one-half of each inherited faculty, each of them contributing one-quarter of it. The four grandparents contribute between them one-quarter, or each of them one-sixteenth; and so on, the sum of the series, $\frac{1}{2} + \frac{1}{4} + \frac{1}{8} + \frac{1}{16} +$ etc., being equal to 1, as it should be. It is a property of this infinite series that each term is equal to the sum of all those that follow: thus $\frac{1}{2} = \frac{1}{4} + \frac{1}{8} + \frac{1}{16} +$ etc.; $\frac{1}{4} = \frac{1}{8} + \frac{1}{16} +$ etc., and so on. The prepotencies or subpotencies of particular ancestors, in any given pedigree, are eliminated by a law that deals only with *average* contributions, and

the varying prepotencies of sex in respect to different qualities are also presumably eliminated."

This law of ancestral inheritance, which states that each parent contributes on an average one-quarter, each grandparent one-sixteenth and so on, rests on researches on human stature, etc., and on colour in Basset hounds, but Prof. Karl Pearson trusts it even more because of its success in predicting results. He is very enthusiastic on the subject, and finishes a paper on Galton's law with the following words: "It is highly probable that it is the simple descriptive statement which brings into a simple focus all the complex lines of hereditary influence. If Darwinian evolution be natural selection combined with *heredity*, then the single statement which embraces the whole field of heredity must prove almost as epoch-making to the biologist as the law of gravitation to the astronomer." *

The aim of this lecture has been to present in brief compass a statement of the leading facts of inheritance, which should be clear in the minds of all. I have said nothing in regard to the transmissibility of acquired characters, for this cannot be ranked at present as an established fact, and I have left some other doubtful points unmentioned. Allow me in conclusion to make this simple remark. The study of inheritance leaves a fatalistic—almost paralysing—impression on many minds, especially perhaps if it be believed that the acquired results of experience and education—of "nurture," in short, cannot be entailed upon the offspring. To some extent this fatalistic impression is justified, but it is well that it should rest upon a sound basis of fact and not on exaggerations. In a sense we can never get away from our inheritance. As Heine said half bitterly, half laughingly, "A man should be very careful in the selection of his parents." On the other hand, although the human organism changes slowly in its heritable organisation, it is very modifiable individually, and "nature" can be bettered by "nurture." If there is little scientific warrant for our being other than sceptical at present as to the inheritance of acquired characters, this scepticism lends greater importance than ever, on the one hand, to a good "nature" to secure which for offspring is part of the problem of careful mating; and, on the other hand, to a good "nurture" to secure which for our children and children's children is one of the most obvious of duties, the hopefulness of the task resting upon the fact that, unlike the beasts that perish, man has a lasting external heritage, capable of endless modification for the better.

[J. A. T.]

* Reference should, however, be made to Mr. Pearson's recent paper (P. Roy. Soc. lxvi., 1900, pp. 140–164) on The Law of Reversion.

GENERAL MONTHLY MEETING,

Monday, April 2, 1900.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Richard Tetley Glazebrook, Esq. F.R.S.
Edward J. Humphrey, Esq.
Hiram Stevens Maxim, Esq.
Saxton W. Armstrong Noble, Esq.
William F. Snell, Esq. B.A.
William John Tennant,

were elected Members of the Royal Institution.

The Chairman announced that the Managers had this day awarded the Actonian Prize of One Hundred Guineas to Sir William Huggins, K.C.B. D.C.L. F.R.S. and Lady Huggins for their work, 'An Atlas of Representative Stellar Spectra.'

The Special Thanks of the Members were returned to Mrs. West and Mrs. F. Colenso, for their present of a portrait of their father. the late Sir Edward Frankland, K.C.B. D.C.L. F.R.S., Professor of Chemistry at the Royal Institution from 1863 to 1868.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Lords of the Admiralty—The Nautical Almanac for 1903. 8vo.
The Meteorological Office—Hourly Means for 1896. 4to. 1899.
Meteorological Observations at Stations of the Second Order for 1896. 4to. 1899.
Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. 1° Semestre, Vol. IX. Fasc. 4, 5. Classe di Scienze Morali, Storiche, etc. Vol. VIII. Fasc. 11, 12. 8vo. 1899–1900.
American Academy of Arts and Sciences—Proceedings, Vol. XXXV. Nos. 8, 9. 8vo. 1899.
American Geographical Society—Bulletin, Vol. XXXII. No. 1. 8vo. 1900.
Asiatic Society of Bengal—Proceedings, 1899, Nos. 8–11; 1900, No. 1. 8vo. Journal, Vol. LXVIII. Part 2, Nos. 2, 3. 8vo. 1899-1900.
Association for the Advancement of Medicine by Research—Experiments on Animals. By S. Paget. 8vo. 1900.
Astronomical Society, Royal—Monthly Notices, Vol. LX. Nos. 4, 5. 8vo. 1899.
Bankers, Institute of—Journal, Vol. XXI. Part 3. 8vo. 1900.
Boston Public Library—Monthly Bulletin, Vol. V. No. 3. 8vo. 1900.
Boston Society of Medical Sciences—Journal, Vol. IV. No. 6. 8vo. 1899.

British Architects, Royal Institute of—Journal, 3rd Series, Vol. VII. No. 9. 4to. 1900.
British Astronomical Association—Journal, Vol. X. No. 5. 8vo. 1900.
California, University of—The International Competition for the Phœbe Hearst Architectural Plan for the University of California. 4to. 1899.
Camera Club—Journal for March, 1900. 8vo.
Canada, Geological Survey of—Contributions to Canadian Palæontology, Vol. IV. Part 1. 8vo. 1899.
Chemical Industry, Society of—Journal, Vol. XIX. No. 2. 8vo. 1900.
Chemical Society—Proceedings, Nos. 220, 221. 8vo. 1900.
Journal for March, 1900. 8vo.
Cracovie, l'Académie des Sciences—Bulletin International, 1900, No. 1. 8vo.
Duckett, Sir G. F. Bart.—Conscription and Voluntary Enlistment, 1900. 8vo.
Editors—American Journal of Science for March, 1900. 8vo.
Analyst for March, 1900. 8vo.
Anthony's Photographic Bulletin for March, 1900. 8vo.
Appointments Gazette for March, 1900. 8vo.
Athenæum for March, 1900. 4to.
Author for March, 1900. 8vo.
Bimetallist for March, 1900. 8vo.
Brewers' Journal for March, 1900. 8vo.
Chemical News for March, 1900. 4to.
Chemist and Druggist for March, 1900. 8vo.
Electrical Engineer for March, 1900. fol.
Electrical Review for March, 1900. 8vo.
Electricity for March, 1900. 8vo.
Engineer for March, 1900. fol.
Engineering for March, 1900. fol.
Homœopathic Review for March, 1900. 8vo.
Horological Journal for March, 1900. 8vo.
Industries and Iron for March, 1900. fol.
Invention for March, 1900.
Journal of the British Dental Association for March, 1900. 8vo.
Journal of State Medicine for March, 1900. 8vo.
Law Journal for March, 1900. 8vo.
Lightning for March, 1900. 8vo.
London Technical Education Gazette for March, 1900.
Machinery Market for March, 1900. 8vo.
Nature for March, 1900. 4to.
New Church Magazine for March, 1900. 8vo.
Photographic News for March, 1900. 8vo.
Physical Review for March, 1900. 8vo.
Popular Science Monthly for March, 1900.
Public Health Engineer for March, 1900. 8vo.
Science Abstracts, Vol. III. Part 3. 8vo. 1900.
Science Siftings for March, 1900. 8vo.
Telephone Magazine for Feb. 1900.
Travel for March, 1900. 8vo.
Tropical Agriculturist for Feb. and March 1900. 8vo.
Zoophilist for March, 1900. 4to.
Electrical Engineers, Institution of—Journal, Vol. XXIX. No. 143. 8vo. 1900.
Florence, Biblioteca Nazionale Centrale—Bollettino, No. 341. 8vo. 1900.
Franklin Institute—Journal for March, 1900. 8vo.
Geographical Society, Royal—Geographical Journal for March, 1900. 8vo.
Imperial Institute—Imperial Institute Journal for March, 1900.
Johns Hopkins University—American Journal of Philology, Vol. XX. Part 4. 8vo. 1899.
American Chemical Journal for March, 1900. 8vo.
Leighton, John, Esq. M.R.I.—Journal of the Ex-Libris Society for March, 1900. 4to.

Mechanical Engineers, Institution of—Proceedings, 1899, No. 4. 8vo.
Navy League—Navy League Journal for March, 1900. 8vo.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XLVIII. Parts 5, 6; Vol. XLIX. Parts 1, 2. 8vo. 1899.
Annual Report. 8vo. 1899.
Odontological Society—Transactions, Vol. XXXII. Nos. 4, 5. 8vo. 1900.
Paris, Société Française de Physique—Séances, 1899, Fasc. 3. 8vo.
Bulletin, No. 144. 8vo. 1900.
Pharmaceutical Society of Great Britain—Journal for March, 1900. 8vo.
Phené, Dr. J. S. M.R.I. (the Author)—Old London in pre-Roman Times. 8vo. 1896.
On some Early Settlers near Conway. 8vo. 1897.
Photographic Society, Royal—Photographic Journal for Feb. 1900. 8vo.
Queensland, The Agent-General for—The Garden of Queensland. By G. E. Evans. 8vo. 1899.
Pamphlets relating to Queensland.
Rome, Ministry of Public Works—Giornale del Genio Civile, 1899, Fasc. 12. 8vo. 1899.
Royal College of Physicians—List. 8vo. 1900.
Royal Society of London—Year Book, 1900. 8vo.
Philosophical Transactions, Vol. CXCIV. A, Nos. 253, 254. 4to. 1899.
Proceedings, Nos. 425, 426. 8vo. 1900.
Saxon Society of Sciences, Royal—
Mathematisch-Physische Classe—
Abhandlungen, Band XXVI. No. 1. 8vo. 1900.
Scottish Microscopical Society—Proceedings, Vol. II. No. 4. 8vo. 1898–99.
Selborne Society—Nature Notes for March, 1900. 8vo.
Smithsonian Institution—Report of U.S. National Museum, Part 1. 8vo. 1899.
Society of Arts—Journal for March, 1900. 8vo.
Tacchini, Prof. P. Hon. Mem. R.I. (the Author)—Memorie della Società degli Spettroscopisti Italiani, Vol. XXVIII. Disp. 12ª. 4to. 1899.
Thurston, R. H. Esq. (the Author)—Evolution of Technical Education. 8vo. 1900.
Tommasina, M. T. Esq. (the Author)—Sur la cristallisation métallique par transport électrique de certains métaux dans l'eau distillée. 8vo. 1899.
United Service Institution, Royal—Journal for March, 1900. 8vo.
United States Department of Agriculture—Experiment Station Record, Vol. XI. Nos. 5–7. 8vo. 1899–1900.
Monthly Weather Review for Dec. 1899. 4to.
United States Geological Survey—Nineteenth Annual Report, Part 2. 4to. 1899.
Twentieth Annual Report, Part 6. 2 vols. 4to. 1899.
United States Patent Office—Official Gazette, Vol. XC. Nos. 8–12. 8vo. 1900.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1900, Heft 2. 8vo.
Vienna, Geological Institute, Imperial—Verhandlungen, 1900, Nos. 1, 2. 8vo.
Washington Academy of Sciences—Proceedings, Vol. II. pp. 1–30. 8vo. 1900.
Washington National Academy of Sciences—Memoirs, Vol. VIII. No. 4. 4to. 1899.
Wright, Messrs. J. & Co. (the Publishers)—The Medical Annual for 1900. 8vo.
Yorkshire Philosophical Society—Annual Report for 1899. 8vo.

WEEKLY EVENING MEETING,

Friday, April 27, 1900.

Sir Frederick Bramwell, Bart., D.C.L. LL.D. F.R.S., Vice-President, in the Chair.

The Right Hon. Lord Kelvin, G.C.V.O. D.C.L. LL.D. F.R.S. *M.R.I.*

Nineteenth Century Clouds over the Dynamical Theory of Heat and Light.

[In the present article, the substance of the lecture is reproduced—with large additions, in which work commenced at the beginning of last year and continued after the lecture, during thirteen months up to the present time, is described—with results confirming the conclusions and largely extending the illustrations which were given in the lecture. I desire to take this opportunity of expressing my obligations to Mr. William Anderson, my secretary and assistant, for the mathematical tact and skill, the accuracy of geometrical drawing, and the unfailingly faithful perseverance in the long-continued and varied series of drawings and algebraic and arithmetical calculations, explained in the following pages. The whole of this work, involving the determination of results due to more than five thousand individual impacts, has been performed by Mr. Anderson.—K., Feb. 2, 1901.]

§ 1. The beauty and clearness of the dynamical theory, which asserts heat and light to be modes of motion, is at present obscured by two clouds. I. The first came into existence with the undulatory theory of light, and was dealt with by Fresnel and Dr. Thomas Young; it involved the question, How could the earth move through an elastic solid, such as essentially is the luminiferous ether? II. The second is the Maxwell-Boltzmann doctrine regarding the partition of energy.

§ 2. Cloud I.—Relative Motion of Ether and Ponderable Bodies; such as movable bodies at the earth's surface, stones, metals, liquids, gases; the atmosphere surrounding the earth; the earth itself as a whole; meteorites, the moon, the sun, and other celestial bodies. We might imagine the question satisfactorily answered, by supposing ether to have practically perfect elasticity for the exceedingly rapid vibrations, with exceedingly small extent of distortion, which constitute light; while it behaves almost like a fluid of very small viscosity, and yields with exceedingly small resistance, practically no resistance, to bodies moving through it as slowly as even the most rapid of the heavenly bodies. There are, however, many very serious objections

to this supposition; among them one which has been most noticed, though perhaps not really the most serious, that it seems incompatible with the known phenomena of the aberration of light. Referring to it, Fresnel, in his celebrated letter * to Arago, wrote as follows:

"Mais il paraît impossible d'expliquer l'aberration des étoiles "dans cette hypothèse; je n'ai pu jusqu'à présent du moins concevoir "nettement ce phénomène qu'en supposant que l'éther passe librement "au travers du globe, et que la vitesse communiquée à ce fluide subtil "n'est qu'une petite partie de celle de la terre; n'en excède pas le "centième, par exemple.

"Quelque extraordinaire que paraisse cette hypothèse au premier "abord, elle n'est point en contradiction, ce me semble, avec l'idée "que les plus grands physiciens se sont faite de l'extrème porosité des "corps."

The same hypothesis was given by Thomas Young, in *his* celebrated statement that ether passes through among the molecules or atoms of material bodies like wind blowing through a grove of trees. It is clear that neither Fresnel nor Young had the idea that the ether of their undulatory theory of light, with its transverse vibrations, is essentially an elastic solid, that is to say, matter which resists change of shape with permanent or sub-permanent force. If they had grasped this idea, they must have noticed the enormous difficulty presented by the laceration which the ether must experience if it moves through pores or interstices among the atoms of matter.

§ 3. It has occurred to me that, without contravening anything we know from observation of nature, we may simply deny the scholastic axiom that two portions of matter cannot jointly occupy the same space, and may assert, as an admissible hypothesis, that ether does occupy the same space as ponderable matter, and that ether is not displaced by ponderable bodies moving through space occupied by ether. But how then could matter act on ether, and ether act on matter, to produce the known phenomena of light (or radiant heat), generated by the action of ponderable bodies on ether, and acting on ponderable bodies to produce its visual, chemical, phosphorescent, thermal, and photographic effects? There is no difficulty in answering this question if, as it probably is, ether is a compressible and dilatable † solid. We have only to suppose that the atom exerts force on the ether, by which condensation or rarefaction is produced within the space occupied by the atom. At present ‡ I confine myself,

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* 'Annales de Chimie,' 1818; quoted in full by Larmor in his recent book, 'Æther and Matter,' pp. 320–322.

† To deny this property is to attribute to ether infinitely great resistance against forces tending to condense it or to dilate it—which seems, in truth, an infinitely difficult assumption.

‡ Further developments of the suggested idea have been contributed to the Royal Society of Edinburgh, and to the Congrès International de Physique, held in Paris in August. (Proc. R.S.E. July 1900; vol. of reports, in French, of the Cong. Inter.; and Phil. Mag., Aug., Sept., 1900.)

for the sake of simplicity, to the suggestion of a spherical atom producing condensation and rarefaction, with concentric spherical surfaces of equal density, but the same total quantity of ether within its boundary as the quantity in an equal volume of free undisturbed ether.

§ 4. Consider now such an atom given at rest anywhere in space occupied by ether. Let force be applied to it to cause it to move in any direction, first with gradually increasing speed, and after that with uniform speed. If this speed is anything less than the velocity of light, the force may be mathematically proved to become zero at some short time after the instant when the velocity of the atom becomes uniform, and to remain zero for ever thereafter. What takes place is this:

§ 5. During all the time in which the velocity of the atom is being augmented from zero, two sets of non-periodic waves, one of them equi-voluminal, the other irrotational (which is therefore condensational-rarefactional), are being sent out in all directions through the surrounding ether. The rears of the last of these waves leave the atom, at some time after its acceleration ceases. This time, if the motion of the ether outside the atom, close beside it, is infinitesimal, is equal to the time taken by the slower wave (which is the equi-voluminal) to travel the diameter of the atom, and is the short time referred to in § 4. When the rears of both waves have got clear of the atom, the ether within it and in the space around it, left clear by both rears, has come to a steady state of motion relatively to the atom. This steady motion approximates more and more nearly to uniform motion in parallel lines, at greater and greater distances from the atom. At a distance of twenty diameters it differs exceedingly little from uniformity.

§ 6. But it is only when the velocity of the atom is very small in comparison with the velocity of light, that the disturbance of the ether in the space close round the atom is infinitesimal. The propositions asserted in § 4 and the first sentence of § 5 are true, however little the final velocity of the atom falls short of the velocity of light. If this uniform final velocity of the atom exceeds the velocity of light, by ever so little, a non-periodic conical wave of equi-voluminal motion is produced, according to the same principle as that illustrated for sound by Mach's beautiful photographs of illumination by electric spark, showing, by changed refractivity, the condensational-rarefactional disturbance produced in air by the motion through it of a rifle bullet. The semi-vertical angle of the cone, whether in air or ether, is equal to the angle whose sine is the ratio of the wave velocity to the velocity of the moving body.*

* On the same principle we see that a body moving steadily (and, with little error, we may say also that a fish or water-fowl propelling itself by fins or web-feet) through calm water, either floating on the surface or wholly submerged at some moderate distance below the surface, produces no wave disturbance if its

§ 7. If, for a moment, we imagine the steady motion of the atom to be at a higher speed than the wave velocity of the condensational-rarefactional wave, two conical waves, of angles corresponding to the two wave velocities, will be steadily produced ; but we need not occupy ourselves at present with this case, because the velocity of the condensational-rarefactional wave in ether is, we are compelled to believe, enormously great in comparison with the velocity of light.

§ 8. Let now a periodic force be applied to the atom so as to cause it to move to and fro continually, with simple harmonic motion. By the first sentence of § 5 we see that two sets of periodic waves, one equi-voluminal, the other irrotational, are continually produced. Without mathematical investigation we see that if, as in ether, the condensational-rarefactional wave velocity is very great in comparison with the equi-voluminal wave velocity, the energy taken by the condensational-rarefactional wave is exceedingly small in comparison with that taken by the equi-voluminal wave ; how small we can find easily enough by regular mathematical investigation. Thus we see how it is that the hypothesis of § 3 suffices for the answer suggested in that section to the question, How could matter act on ether so as to produce light ?

§ 9. But this, though of primary importance, is only a small part

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velocity is less than the minimum wave velocity due to gravity and surface tension (being about 23 cms. per second, or ·44 of a nautical mile per hour, whether for sea water or fresh water) ; and if its velocity exceeds the minimum wave velocity, it produces a wave disturbance bounded by two lines inclined on each side of its wake at angles each equal to the angle whose sine is the minimum wave velocity divided by the velocity of the moving body. It is easy for anyone to observe this by dipping vertically a pencil or a walking stick into still water in a pond (or even in a good-sized hand basin), and moving it horizontally, first with exceeding small speed, and afterwards faster and faster. I first noticed it nineteen years ago, and described observations for an experimental determination of the minimum velocity of waves, in a letter to William Froude, published in 'Nature' for October 26, and in the Phil. Mag. for November 1871, from which the following is extracted. "[Recently, in the schooner yacht *Lalla Rookh*], "being becalmed in the Sound of Mull, I had an excellent opportunity, with the "assistance of Professor Helmholtz, and my brother from Belfast [the late "Professor James Thomson], of determining by observation the minimum wave-"velocity with some approach to accuracy. The fishing-line was hung at a distance "of two or three feet from the vessel's side, so as to cut the water at a point not "sensibly disturbed by the motion of the vessel. The speed was determined by "throwing into the sea pieces of paper previously wetted, and observing their "times of transit across parallel planes, at a distance of 912 centimetres asunder, "fixed relatively to the vessel by marks on the deck and gunwale. By watching "carefully the pattern of ripples and waves which connected the ripples in "front with the waves in rear, I had seen that it included a set of parallel waves "slanting off obliquely on each side and presenting appearances which proved "them to be waves of the critical length and corresponding minimum speed of "propagation." When the speed of the yacht fell to but little above the critical velocity, the front of the ripples was very nearly perpendicular to the line of motion, and when it just fell below the critical velocity the ripples disappeared altogether, and there was no perceptible disturbance on the surface of the water. The sea was "glassy"; though there was wind enough to propel the schooner at speed varying between ½ mile and 1 mile per hour.

of the very general question pointed out in § 3 as needing answer. Another part, fundamental in the undulatory theory of optics, is, How is it that the velocity of light is smaller in transparent ponderable matter than in pure ether? Attention was called to this particular question in my address to the Royal Institution, of last April; and a slight explanation of my proposal for answering it was given, and illustrated by a diagram. The validity of this proposal is confirmed by a somewhat elaborate discussion and mathematical investigation of the subject worked out since that time and communicated under the title, 'On the Motion produced in an Infinite Elastic Solid by the Motion through the Space occupied by it of a Body acting on it only by Attraction or Repulsion,' to the Royal Society of Edinburgh on July 17, and to the Congrès International de Physique for its meeting at Paris in the beginning of August.

§ 10. The other phenomena referred to in § 3 come naturally under the general dynamics of the undulatory theory of light, and the full explanation of them all is brought much nearer if we have a satisfactory fundamental relation between ether and matter, instead of the old intractable idea that atoms of matter displace ether from the space before them, when they are in motion relatively to the ether around them. May we then suppose that the hypothesis which I have suggested clears away the first of our two clouds? It certainly would explain the "aberration of light" connected with the earth's motion through ether in a thoroughly satisfactory manner. It would allow the earth to move with perfect freedom through space occupied by ether without displacing it. In passing through the earth the ether, an elastic solid, would not be lacerated as it would be according to Fresnel's idea of porosity and ether moving through the pores as if it were a fluid. Ether would move relatively to ponderables with the perfect freedom wanted for what we know of aberration, instead of the imperfect freedom of air moving through a grove of trees suggested by Thomas Young. According to it, and for simplicity neglecting the comparatively very small component due to the earth's rotation (only ·46 of a kilometre per second at the equator where it is a maximum), and neglecting the imperfectly known motion of the solar system through space towards the constellation Hercules, discovered by Herschel,* there would be at all points of the earth's surface a flow

* The splendid spectroscopic method originated by Huggins thirty-three years ago, for measuring the component in the line of vision of the relative motion of the earth, and any visible star, has been carried on since that time with admirable perseverance and skill by other observers, who have from their results made estimates of the velocity and direction of the motion through space of the centre of inertia of the solar system. My Glasgow colleague, Professor Becker, has kindly given me the following information on the subject of these researches:

"The early (1888) Potsdam photographs of the spectra of 51 stars brighter than 2½ magnitude have been employed for the determination of the apex and velocity of the solar system. Kempf (Astronomische Nachrichten, vol. 132) finds for the apex: right ascension, 206° ± 12°; declination, 46° ± 9°; velocity, 19 kilometres per second; and Risteen (Astronomical Journal, 1893) finds practically the same

of ether at the rate of 30 kilometres per second in lines all parallel to the tangent to the earth's orbit round the sun. There is nothing inconsistent with this in all we know of the ordinary phenomena of terrestrial optics; but, alas! there is inconsistency with a conclusion that ether in the earth's atmosphere is motionless relatively to the earth, seemingly proved by an admirable experiment designed by Michelson, and carried out, with most searching care to secure a trustworthy result, by himself and Morley.* I cannot see any flaw either in the idea or in the execution of this experiment. But a possibility of escaping from the conclusion which it seemed to prove, may be found in a brilliant suggestion made independently by Fitzgerald † and by Lorentz ‡ of Leyden, to the effect that the motion of ether through matter may slightly alter its linear dimensions, according to which if the stone slab constituting the sole plate of Michelson and Morley's apparatus has, in virtue of its motion through space occupied by ether, its lineal dimensions shortened one one-hundred-millionth ‖ in the direction of motion, the result of the experiment would not disprove the free motion of ether through space occupied by the earth.

§ 11. I am afraid we must still regard Cloud No. I. as very dense.

§ 12. Cloud II.—Waterston (in a communication to the Royal Society, now famous, which, after lying forty-five years buried and almost forgotten in the archives, was rescued from oblivion by Lord Rayleigh and published, with an introductory notice of great interest and importance, in the Transactions of the Royal Society for 1892) enunciated the following proposition: "In mixed media the mean "square molecular velocity is inversely proportional to the specific "weight of the molecule. This is the law of the equilibrium of vis viva." Of this proposition Lord Rayleigh in a footnote ¶ says, "This is the first "statement of a very important theorem (see also Brit. Assoc. Rep., "1851). The demonstration, however, of § 10 can hardly be defended. "It bears some resemblance to an argument indicated and exposed "by Professor Tait (Edinburgh Trans., vol. 33, p. 79, 1886). There "is reason to think that this law is intimately connected with the "Maxwellian distribution of velocities of which Waterston had no "knowledge."

§ 13. In Waterston's statement, the "specific weight of a mole-

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quantities. The proper motions of the fixed stars assign to the apex a position which may be anywhere in a narrow zone parallel to the Milky-way, and extending 20° on both sides of a point of Right Ascension 275° and Declination + 30°. The authentic mean of 13 values determined by the methods of Argelander or Airy gives 274° and + 35° (André, 'Traité d Astronomie Stellaire')."

* Phil. Mag., December 1887.

† Public Lectures in Trinity College, Dublin.

‡ Versuch einer Theorie der electrischen und optischen Erscheinungen in bewegten Körpern.

‖ This being the square of the ratio of the earth's velocity round the sun (30 kilometres per sec.) to the velocity of light (300,000 kilometres per sec.).

¶ Phil. Trans. A, 1892, p. 16.

cule" means what we now call simply the mass of a molecule; and "molecular velocity" means the translational velocity of a molecule. Writing on the theory of sound in the Phil. Mag. for 1858, and referring to the theory developed in his buried paper,* Waterston said, "The theory . . . . assumes . . . . that if the impacts produce "rotatory motion the vis viva thus invested bears a constant ratio to "the rectilineal vis viva." This agrees with the very important principle or truism given independently about the same time by Clausius to the effect that the mean energy, kinetic and potential, due to the relative motion of all the parts of any molecule of a gas, bears a constant ratio to the mean energy of the motion of its centre of inertia when the density and pressure are constant.

§ 14. Without any knowledge of what was to be found in Waterston's buried paper, Maxwell, at the meeting of the British Association at Aberdeen, in 1859 † gave the following proposition regarding the motion and collisions of perfectly elastic spheres: "Two systems of "particles move in the same vessel; to prove that the mean vis viva "of each particle will become the same in the two systems." This is precisely Waterston's proposition regarding the law of partition of energy, quoted in § 12 above; but Maxwell's 1860 proof was certainly not more successful than Waterston's. Maxwell's 1860 proof has always seemed to me quite inconclusive, and many times I urged my colleague, Professor Tait, to enter on the subject. This he did, and in 1886 he communicated to the Royal Society of Edinburgh a paper ‡ on the foundations of the kinetic theory of gases, which contained a critical examination of Maxwell's 1860 paper, highly appreciative of the great originality and splendid value, for the kinetic theory of gases, of the ideas and principles set forth in it; but showing that the demonstration of the theorem of the partition of energy in a mixed assemblage of particles of different masses was inconclusive, and successfully substituting for it a conclusive demonstration.

§ 15. Waterston, Maxwell, and Tait, all assume that the particles of the two systems are thoroughly mixed (Tait, § 18), and their theorem is of fundamental importance in respect to the specific heats of mixed gases. But they do not, in any of the papers already referred to, give any indication of a proof of the corresponding theorem, regarding the partition of energy between two sets of equal particles separated by a membrane impermeable to the molecules, while permitting forces to act across it between the molecules on its two sides, ‖

* 'On the Physics of Media that are Composed of Force and Perfectly Elastic Molecules in a State of Motion.' Phil. Trans. A, 1892, p. 13.

† 'Illustrations of the Dynamical Theory of Gases.' Phil. Mag., January and July 1860, and collected works, vol. i. p. 378.

‡ Phil. Trans. R.S.E., 'On the Foundations of the Kinetic Theory of Gases,' May 14 and December 6, 1886, and January 7, 1887.

‖ A very interesting statement is given by Maxwell regarding this subject in his latest paper regarding the Boltzmann-Maxwell doctrine. 'On Boltzmann's Theorem on the Average Distribution of Energy in a System of Material Points,' Camb. Phil. Trans., May 6, 1878; Collected Works, vol. ii. pp. 713–741.

which is the simplest illustration of the molecular dynamics of Avogadro's law. It seems to me, however, that Tait's demonstration of the Waterston-Maxwell law may possibly be shown to virtually include, not only this vitally important subject, but also the very interesting, though comparatively unimportant, case of an assemblage of particles of equal masses with a single particle of different mass moving about among them.

§ 16. In §§ 12, 14, 15, "particle" has been taken to mean what is commonly, not correctly, called an elastic sphere, but what is in reality a Boscovich atom acting on other atoms in lines exactly through its centre of inertia (so that no rotation is in any case produced by collisions), with, as law of action between two atoms, *no force at distance greater than the sum of their radii, infinite force at exactly this distance.* None of the demonstrations, unsuccessful or successful, to which I have referred would be essentially altered if, instead of this last condition, we substitute a repulsion increasing with diminishing distance, according to any law for distances less than the sum of the radii, subject only to the condition that it would be infinite before the distance became zero. In fact the impact, oblique or direct, between two Boscovich atoms thus defined, has the same result after the collision is completed (that is to say, when their spheres of action get outside one another) as collision between two conventional elastic spheres, imagined to have radii dependent on the lines and velocities of approach before collision (the greater the relative velocity the smaller the effective radii); and the only assumption essentially involved in those demonstrations is, that the radius of each sphere is very small in comparison with the average length of free path.

§ 17. But if the particles are Boscovich atoms, having centre of inertia not coinciding with centre of force; or quasi Boscovich atoms, of non-spherical figure; or (a more acceptable supposition) if each particle is a cluster of two or more Boscovich atoms; rotations and changes of rotation would result from collisions. Waterston's and Clausius' leading principle, quoted in § 13 above, must now be taken into account, and Tait's demonstration is no longer applicable. Waterston and Clausius, in respect to rotation, both wisely abstained from saying more than that the average kinetic energy of rotation bears a constant ratio to the average kinetic energy of translation. With magnificent boldness Boltzmann and Maxwell declared that the ratio is equality; Boltzmann having found what seemed to him a demonstration of this remarkable proposition, and Maxwell having accepted the supposed demonstration as valid.

§ 18. Boltzmann went further* and extended the theorem of equality of mean kinetic energies to any system of a finite number of material points (Boscovich atoms) acting on one another, according to any law of force, and moving freely among one another; and finally,

* 'Studien über das Gleichgewicht der lebendigen Kraft zwischen bewegten materiellen Punkten.' Sitzb. K. Akad. Wien., October 8, 1868.

Maxwell * gave a demonstration extending it to the generalised Lagrangian co-ordinates of any system whatever, with a finite or infinitely great number of degrees of freedom. The words in which he enunciated his supposed theorem are as follows:

"The only assumption which is necessary for the direct proof is "that the system, if left to itself in its actual state of motion, will, "sooner or later, pass [infinitely nearly †] through every phase which is "consistent with the equation of energy" (p. 714) and, again (p. 716).

"It appears from the theorem, that in the ultimate state of the "system the average ‡ kinetic energy of two portions of the system must "be in the ratio of the number of degrees of freedom of those portions.

"This, therefore, must be the condition of the equality of tem-"perature of the two portions of the system."

I have never seen validity in the demonstration ‖ on which Maxwell founds this statement, and it has always seemed to me exceedingly improbable that it can be true. If true, it would be very wonderful, and most interesting in pure mathematical dynamics. Having been published by Boltzmann and Maxwell it would be worthy of most serious attention, even without consideration of its bearing on thermo-dynamics. But, when we consider its bearing on thermo-dynamics, and in its first and most obvious application we find it destructive of the kinetic theory of gases, of which Maxwell was one of the chief founders, we cannot see it otherwise than as a cloud on the dynamical theory of heat and light.

§ 19. For the kinetic theory of gases, let each molecule be a cluster of Boscovich atoms. This includes every possibility ("dynamical," or "electrical," or "physical," or "chemical") regarding the nature and qualities of a molecule and of all its parts. The mutual forces between the constituent atoms must be such that the cluster is in stable equilibrium if given at rest; which means, that if started from

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* 'On Boltzmann's Theorem on the Average Distribution of Energy in a System of Material Points.' Maxwell's Collected Papers, vol. ii. pp. 713–741, and Camb. Phil. Trans., May 6, 1878.

† I have inserted these two words as certainly belonging to Maxwell's meaning.—K.

‡ The average here meant is a time-average through a sufficiently long time.

‖ The mode of proof followed by Maxwell, and its connection with antecedent considerations of his own and of Boltzmann, imply, as included in the general theorem, that the average kinetic energy of any one of three rectangular components of the motion of the centre of inertia of an isolated system, acted upon only by mutual forces between its parts is equal to the average kinetic energy of each generalised component of motion relatively to the centre of inertia. Consider, for example, as "parts of the system" two particles of masses m and m' free to move only in a fixed straight line, and connected to one another by a massless spring. The Boltzmann-Maxwell doctrine asserts that the average kinetic energy of the motion of the inertial centre is equal to the average kinetic energy of the motion relative to the inertial centre. This is included in the wording of Maxwell's statement in the text if, but not unless, $m = m'$. See footnote on § 7 of my paper 'On some Test-Cases for the Boltzmann-Maxwell Doctrine regarding Distribution of Energy.' Proc. Roy. Soc., June 11, 1891.

equilibrium with its constituents in any state of relative motion, no atom will fly away from it, provided the total kinetic energy of the given initial motion does not exceed some definite limit. A gas is a vast assemblage of molecules thus defined, each moving freely through space, except when in collision with another cluster, and each retaining all its own constituents unaltered, or only altered by interchange of similar atoms between two clusters in collision.

§ 20. For simplicity we may suppose that each atom, A, has a definite radius of activity, a, and that atoms of different kinds, A, A′, have different radii of activity, a, a'; such that A exercises no force on any other atom, A′, A″, when the distance between their centres is greater than $a + a'$ or $a + a''$. We need not perplex our minds with the inconceivable idea of "virtue," whether for force or for inertia, residing in a mathematical point * the centre of the atom; and without mental strain we can distinctly believe that the substance (the "substratum" of qualities) resides, not in a point, nor vaguely through all space, but definitely in the spherical volume of space bounded by the spherical surface whose radius is the radius of activity of the atom, and whose centre is the centre of the atom. In our intermolecular forces thus defined, we have no violation of the old scholastic law, "Matter cannot act where it is not," but we explicitly violate the other scholastic law, "Two portions of matter cannot simultaneously occupy the same space." We leave to gravitation, and possibly to electricity (probably not to magnetism), the at present very unpopular idea of action at a distance.

§ 21. We need not now (as in § 16, when we wished to keep as near as we could to the old idea of colliding elastic globes) suppose the mutual force to become infinite repulsion before the centres of two atoms, approaching one another, meet. Following Boscovich, we may assume the force to vary according to any law of alternate attraction and repulsion, but without supposing any infinitely great force, whether of repulsion or attraction, at any particular distance; but we must assume the force to be zero when the centres are coincident. We may even admit the idea of the centres being absolutely coincident, in at all events some cases of a chemical combination of two or more atoms; although we might consider it more probable that in most cases the chemical combination is a cluster, in which the volumes of the constituent atoms overlap without any two centres absolutely coinciding.

§ 22. The word "collision" used without definition in § 19 may now, in virtue of §§ 20, 21, be unambiguously defined thus: Two atoms are said to be in collision during all the time their volumes overlap after coming into contact. They necessarily in virtue of inertia separate again, unless some third body intervenes with action which causes them to remain overlapping; that is to say,

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* See Math. and Phys. Papers, vol. iii. art. xcvii. 'Molecular Constitution of Matter,' § 14.

causes combination to result from collision. Two clusters of atoms are said to be in collision when, after being separate, some atom or atoms of one cluster come to overlap some atom or atoms of the other. In virtue of inertia the collision must be followed either by the two clusters separating, as described in the last sentence of § 19, or by some atom or atoms of one or both systems being sent flying away. This last supposition is a matter-of-fact statement belonging to the magnificent theory of dissociation, discovered and worked out by Sainte-Clair Deville without any guidance from the kinetic theory of gases. In gases approximately fulfilling the gaseous laws (Boyle's and Charles'), two clusters must in general fly asunder after collision. Two clusters could not possibly remain permanently in combination without at least one atom being sent flying away after collision between two clusters with no third body intervening.*

§ 23. Now for the application of the Boltzman-Maxwell doctrine to the kinetic theory of gases: consider first a homogeneous single gas, that is, a vast assemblage of similar clusters of atoms moving and colliding as described in the last sentence of § 19; the assemblage being so sparse that the time during which each cluster is in collision is very short in comparison with the time during which it is unacted on by other clusters, and its centre of inertia, therefore, moves uniformly in a straight line. If there are i atoms in each cluster, it has $3i$ freedoms to move, that is to say, freedoms in three rectangular directions for each atom. The Boltzman-Maxwell doctrine asserts that the mean kinetic energies of these $3i$ motions are all equal, whatever be the mutual forces between the atoms. From this, when the durations of the collisions are not included in the time-averages, it is easy to prove algebraically (with exceptions noted below) that the time-average of the kinetic energy of the component translational velocity of the inertial centre,† in any direction, is equal to any one of the $3i$ mean kinetic energies asserted to be equal to one another in the preceding statement. There are exceptions to the algebraic proof corresponding to the particular exception referred to in the last footnote to § 18 above; but, nevertheless, the general Boltzmann-Maxwell doctrine includes the proposition, even in those cases in which it is not deducible algebraically from the equality of the $3i$ energies. Thus, without exception, the average kinetic energy of any component of the motion of the inertial centre is, according to the Boltzmann-Maxwell doctrine, equal to $\frac{1}{3i}$ of the whole average kinetic energy of the system. This makes the total average energy, potential and kinetic, of the whole motion of the system, translational and relative, to be $3i\,(1 + P)$ times the mean

* See Kelvin's Math. and Phys. Papers, vol. iii. art. XCVII. § 33. In this reference, for "scarcely" substitute "not."

† This expression I use for brevity to signify the kinetic energy of the whole mass ideally collected at the centre of inertia.

kinetic energy of one component of the motion of the inertial centre, where P denotes the ratio of the mean potential energy of the relative displacements of the parts to the mean kinetic energy of the whole system. Now, according to Clausius' splendid and easily proved theorem regarding the partition of energy in the kinetic theory of gases, the ratio of the difference of the two thermal capacities to the constant-volume thermal capacity is equal to the ratio of twice a single component of the translational energy to the total energy. Hence, if according to our usual notation we denote the ratio of the thermal capacity pressure-constant to the thermal capacity volume-constant by k, we have,

$$k - 1 = \frac{2}{3i(1 + P)}.$$

§ 24. *Example* 1.—For first and simplest example, consider a monatomic gas. We have $i = 1$, and according to our supposition (the supposition generally, perhaps universally, made) regarding atoms, we have $P = 0$. Hence, $k - 1 = \frac{2}{3}$.

This is merely a fundamental theorem in the kinetic theory of gases for the case of no rotational or vibrational energy of the molecule; in which there is no scope either for Clausius' theorem or for the Boltzmann-Maxwell doctrine. It is beautifully illustrated by mercury vapour, a monatomic gas according to chemists, for which many years ago Kundt, in an admirably designed experiment, found $k - 1$ to be very approximately $\frac{2}{3}$; and by the newly discovered gases argon, helium, and krypton, for which also $k - 1$ has been found to have approximately the same value, by Rayleigh and Ramsay. But each of these four gases has a large number of spectrum lines, and therefore a large number of vibrational freedoms, and therefore, if the Boltzmann-Maxwell doctrine were true, $k - 1$ would have some exceedingly small value, such as that shown in the ideal example of § 26 below. On the other hand, Clausius' *theorem* presents no difficulty; it merely asserts that $k - 1$ is necessarily less than $\frac{2}{3}$ in each of these four cases, as in every case in which there is any rotational or vibrational energy whatever; and proves, from the values found experimentally for $k - 1$ in the four gases, that in each case the total of rotational and vibrational energy is exceedingly small in comparison with the translational energy. It justifies admirably the chemical doctrine that mercury vapour is *practically a monatomic gas*, and it proves that argon, helium, and krypton, are also *practically monatomic*, though none of these gases has hitherto shown any chemical affinity or action of any kind from which chemists could draw any such conclusion.

But Clausius' theorem, taken in connection with Stokes' and Kirchoff's dynamics of spectrum analysis, throws a new light on what we are now calling a "practically monatomic gas." It shows that, unless we admit that the atoms can be set into rotation or vibration by mutual collisions (a most unacceptable hypothesis), it must have satellites connected with it (or ether condensed into it or around it)

and kept, by the collisions, in motion relatively to it with total energy exceedingly small in comparison with the translational energy of the whole system of atom and satellites. The satellites must in all probability be of exceedingly small mass in comparison with that of the chief atom. Can they be the "ions" by which J. J. Thomson explains the electric conductivity induced in air and other gases by ultra-violet light, Röntgen rays, and Becquerel rays?

Finally, it is interesting to remark that all the values of $k - 1$ found by Rayleigh and Ramsay are somewhat less than $\frac{2}{3}$; argon ·64, ·61; helium ·652; krypton ·666. If the deviation from ·667 were accidental they would probably have been some in defect and some in excess.

Example 2.—As a next simplest example let $i = 2$, and as a very simplest case let the two atoms be in stable equilibrium when concentric, and be infinitely nearly concentric when the clusters move about, constituting a homogeneous gas. This supposition makes $P = \frac{1}{2}$, because the average potential energy is equal to the average kinetic energy in simple harmonic vibrations; and in our present case half the whole kinetic energy, according to the Boltzmann-Maxwell doctrine, is vibrational, the other half being translational. We find $k - 1 = \frac{2}{9} = \cdot 2222$.

Example 3.—Let $i = 2$; let there be stable equilibrium, with the centres C, C′ of the two atoms at a finite distance a asunder, and let the atoms be always very nearly at this distance asunder when the clusters are not in collision. The relative motions of the two atoms will be according to three freedoms, one vibrational, consisting of very small shortenings and lengthenings of the distance C C′, and two rotational, consisting of rotations round one or other of two lines perpendicular to each other and perpendicular to C C′ through the inertial centre. With these conditions and limitations, and with the supposition that half the average kinetic energy of the rotation is comparable with the average kinetic energy of the vibrations, or exactly equal to it as according to the Boltzmann-Maxwell doctrine, it is easily proved that in rotation the excess of C C′ above the equilibrium distance a, due to centrifugal force, must be exceedingly small in comparison with the maximum value of C C′ $- a$ due to the vibration. Hence the average potential energy of the rotation is negligible in comparison with the potential energy of the vibration. Hence, of the three freedoms for relative motion there is only one contributory to P, and therefore we have $P = \frac{1}{6}$. Thus we find $k - 1 = \frac{2}{7} = \cdot 2857$.

The best way of experimentally determining the ratio of the two thermal capacities for any gas is by comparison between the observed and the Newtonian velocities of sound. It has thus been ascertained that, at ordinary temperatures and pressures, $k - 1$ differs but little from ·406 for common air, which is a mixture of the two gases nitrogen and oxygen, each diatomic according to modern chemical theory; and the greatest value that the Boltzmann-Maxwell doctrine can give for a diatomic gas is the ·2857 of Ex. 3. This notable discrepance

from observation, suffices to absolutely disprove the Boltzmann-Maxwell doctrine. What is really established in respect to partition of energy is what Clausius' theorem tells us (§ 23 above). We find, as a result of observation and true theory, that the average kinetic energy of translation of the molecules of common air is ·609 of the total energy, potential and kinetic, of the relative motion of the constituents of the molecules.

§ 25. The method of treatment of Ex. 3 above, carried out for a cluster of any number of atoms greater than two not in one line, $j+2$ atoms, let us say, shows us that there are three translational freedoms; three rotational freedoms, relatively to axes through the inertial centre; and $3j$ vibrational freedoms. Hence we have $P = \frac{j}{j+2}$, and we find $k-1 = \frac{1}{3(1+j)}$. The values of $k-1$ thus calculated for a triatomic and tetratomic gas, and calculated as above in Ex. 3 for a diatomic gas, are shown in the following table, and compared with the results of observation for several such gases:

Gas.	Values of $k-1$	
	According to the B.-M. doctrine.	By Observation.
Air	$\frac{2}{7}$ = ·2857	·406
H_2	" "	·40
O_2	" "	·41
Cl_2	" "	·32
CO	" "	·39
NO	" "	·39
CO_2	$\frac{1}{6}$ = ·1667	·30
N_2O	" "	·331
NH_3	$\frac{1}{9}$ = ·1111	·311

It is interesting to see how the dynamics of Clausius' theorem is verified by the results of observation shown in the table. The values of $k-1$ for all the gases are less than $\frac{2}{3}$, as they must be when there is any appreciable energy of rotation or vibration in the molecule. They are different for different diatomic gases; ranging from ·42 for oxygen to ·32 for chlorine, which is quite as might be expected, when we consider that the laws of force between the two atoms may differ largely for the different kinds of atoms. The values of $k-1$ are, on the whole, smaller for the tetratomic and triatomic than for the diatomic gases, as might be expected from consideration of Clausius' principle. It is probable that the differences of $k-1$ for the different diatomic gases are real, although there is considerable uncertainty with regard to the observational results for all or some of the gases other than air. It is certain that

the discrepancies from the values, calculated according to the Boltzmann-Maxwell doctrine, are real and great; and that in each case, diatomic, triatomic, and tetratomic, the doctrine gives a value for $k - 1$ much smaller than the truth.

§ 26. But, in reality, the Boltzmann-Maxwell doctrine errs enormously more than is shown in the preceding table. Spectrum analysis showing vast numbers of lines for each gas makes it certain that the numbers of freedoms of the constituents of each molecule is enormously greater than those which we have been counting, and therefore that unless we attribute vibratile quality to each individual atom, the molecule of every one of the ordinary gases must have a vastly greater number of atoms in its constitution than those hitherto reckoned in regular chemical doctrine. Suppose, for example, there are forty-one atoms in the molecule of any particular gas; if the doctrine were true, we should have $j = 39$. Hence there are 117 vibrational freedoms, so that there might be 117 visible lines in the spectrum of the gas; and we have $k - 1 = \frac{1}{120} = \cdot 0083$. There is, in fact, no possibility of reconciling the Boltzmann-Maxwell doctrine with the truth regarding the specific heats of gases.

§ 27. It is, however, not quite possible to rest contented with the mathematical verdict not proven, and the experimental verdict not true, in respect to the Boltzmann-Maxwell doctrine. I have always felt that it should be mathematically tested by the consideration of some particular case. Even if the theorem were true, stated as it was somewhat vaguely, and in such general terms that great difficulty has been felt as to what it is really meant to express, it would be very desirable to see even one other simple case, besides that original one of Waterston's, clearly stated and tested by pure mathematics. Ten years ago,* I suggested a number of test cases, some of which have been courteously considered by Boltzmann; but no demonstration either of the truth or untruth of the doctrine as applied to any one of them has hitherto been given. A year later, I suggested what seemed to me a decisive test case disproving the doctrine; but my statement was quickly and justly criticised by Boltzmann and Poincaré; and more recently Lord Rayleigh † has shown very clearly that my simple test case was quite indecisive. This last article of Rayleigh's has led me to resume the consideration of several different classes of dynamical problems, which had occupied me more or less at various times during the last twenty years, each presenting exceedingly interesting features in connection with the double question: Is this a case which admits of the application of the Boltzmann-Maxwell doctrine; and if so, is the doctrine true for it?

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* 'On some Test Cases for the Maxwell-Boltzmann Doctrine regarding Distribution of Energy.' Proc. Roy. Soc., June 11, 1891.

† Phil. Mag., vol. xxxiii. 1892, p. 356. 'Remarks on Maxwell's Investigation respecting Boltzmann's Theorem.'

§ 28. Premising that the mean kinetic energies with which the Boltzmann-Maxwell doctrine is concerned are time-integrals of energies divided by totals of the times, we may conveniently divide the whole class of problems, with reference to which the doctrine comes into question, into two classes.

Class I.: Those in which the velocities considered are either constant or only vary suddenly—that is to say, in infinitely small times—or in times so short that they may be omitted from the time-integration. To this class belong:

(*a*) The original Waterston-Maxwell case and the collisions of ideal rigid bodies of any shape, according to the assumed law that the translatory and rotatory motions lose no energy in the collisions.

(*b*) The frictionless motion of one or more particles constrained to remain on a surface of any shape, this surface being either closed (commonly called finite though really endless), or being a finite area of plane or curved surface, bounded like a billiard table, by a wall or walls, from which impinging particles are reflected at angles equal to the angles of incidence.

(*c*) A closed surface, with non-vibratory particles moving within it freely except during impacts of particles against one another or against the bounding surface.

(*d*) Cases such as (*a*), (*b*), or (*c*), with impacts against boundaries and mutual impacts between particles, softened by the supposition of finite forces during the impacts, with only the condition that the durations of the impacts are so short as to be practically negligible, in comparison with the durations of free paths.

Class II.: Cases in which the velocities of some of the particles concerned sometimes vary gradually; so gradually that the times during which they vary must be included in the time-integration. To this class belong examples such as (*d*) of Class I. with durations of impacts not negligible in the time-integration.

§ 29. Consider first Class I. (*b*) with a finite closed surface as the field of motion and a single particle moving on it. If a particle is given, moving in any direction through any point I of the field, it will go on for ever along one determinate geodetic line. The question that first occurs is, does the motion fulfil Maxwell's condition (see § 18 above); that is to say, for this case, if we go along the geodetic line long enough, shall we pass infinitely nearly to any point Q whatever, including I, of the surface an infinitely great number of times in all directions? This question cannot be answered in the affirmative without reservation. For example, if the surface be exactly an ellipsoid it must be answered in the negative, as is proved in the following §§ 30, 31, 32.

§ 30. Let A A′, B B′, C C′, be the ends of the greatest, mean, and least diameters of an ellipsoid. Let U_1 U_2 U_3 U_4 be the umbilics in the arcs A C, C A′, A′ C′, C′ A. A known theorem in the geometry of the ellipsoid tells us, that every geodetic through U_1 passes through U_3, and every geodetic through U_2 passes through

U_4. This statement regarding geodetic lines on an ellipsoid of three unequal axes is illustrated by Fig. 1, a diagram showing for the extreme case in which the shortest axis is zero, the exact construction of a geodetic through U_1 which is a focus of the ellipse shown in the diagram. U_3, C', U_4 being infinitely near to U_1, C, U_2 respectively are indicated by double letters at the same points. Starting from U_1 draw the geodetic $U_1\,Q\,U_3$; the two parts of which $U_1\,Q$ and $Q\,U_3$ are straight lines. It is interesting to remark that in whatever direction we start from U_1 if we continue the geodetic through U_3, and on through U_1 again and so on endlessly, as indicated in the diagram by the straight lines $U_1\,Q\,U_3\,Q'\,U_1\,Q''\,U_3\,Q'''$, and so on, we come very quickly to lines approaching successively more and more nearly to coincidence with the major axis. At every point

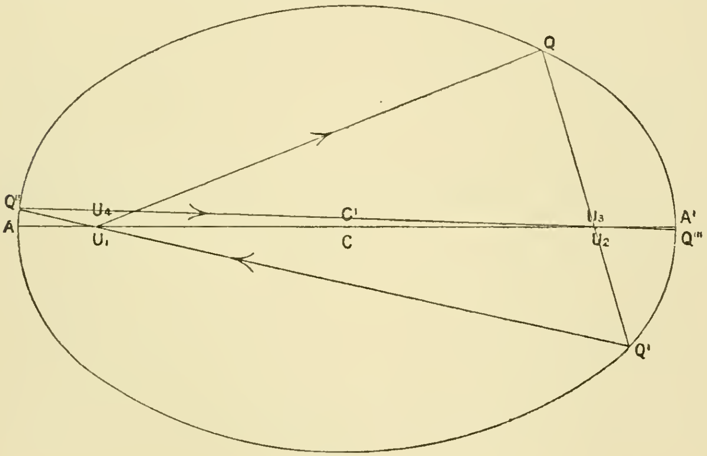


Fig. 1.

where the path strikes the ellipse it is reflected at equal angles to the tangent. The construction is most easily made by making the angle between the reflected path and a line to one focus, equal to the angle between the incident path and a line to the other focus.

§ 31. Returning now to the ellipsoid :—From any point I, between U_1 and U_2, draw the geodetic I Q, and produce it through Q on the ellipsoidal surface. It must cut the arc $A'\,C'\,A$ at some point between U_3 and U_4, and, if continued on and on, it must cut the ellipse $A\,C\,A'\,C'\,A$ successively between U_1 and U_2, or between U_3 and U_4; never between U_2 and U_3, or U_4 and U_1. This, for the extreme case of the smallest axis zero, is illustrated by the path $I\,Q\,Q'\,Q''\,Q'''\,Q^{iv}\,Q^{v}$ in Fig. 2.

§ 32. If now, on the other hand, we commence a geodetic through

any point J between U_1 and U_4, or between U_2 and U_3, it will never cut the principal section containing the umbilics, either between U_1 and U_2 or between U_3 and U_4. This for the extreme case of $C\,C' = 0$ is illustrated in Fig. 3.

§ 33. It seems not improbable that if the figure deviates by ever so little from being exactly ellipsoidal, Maxwell's condition might be fulfilled. It seems indeed quite probable that Maxwell's condition (see §§ 13, 29, above) is fulfilled by a geodetic on a closed surface of any shape in general, and that exceptional cases, in which the question of § 29 is to be answered in the negative, are merely particular surfaces of definite shapes, infinitesimal deviations from which will allow the question to be answered in the affirmative.

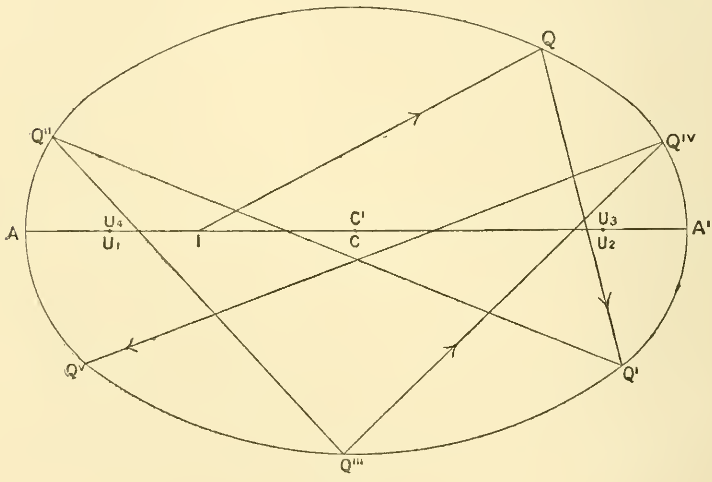


FIG. 2.

§ 34. Now with an affirmative answer to the question—is Maxwell's condition fufilled?—what does the Boltzmann-Maxwell doctrine assert in respect to a geodetic on a closed surface? The mere wording of Maxwell's statement, quoted in § 13 above, is not applicable to this case, but the meaning of the doctrine as interpreted from previous writings both of Boltzmann and Maxwell, and subsequent writings of Boltzmann, and of Rayleigh,* the most recent supporter of the doctrine, is that a single geodetic drawn long enough will not only fulfil Maxwell's condition of passing infinitely near to every point of the surface in all directions, but will pass with equal frequencies in all directions; and as many times within a certain infinitesimal distance $\pm\ \delta$ of any one point P as of any other point P'

* Phil. Mag., January 1900.

anywhere over the whole surface. This, if true, would be an exceedingly interesting theorem.

§ 35. I have made many efforts to test it for the case in which the closed surface is reduced to a plane with other boundaries than an exact ellipse (for which as we have seen in §§ 30, 31, 32, the investigation fails through the non-fulfilment of Maxwell's preliminary condition). Every such case gives, as we have seen, straight lines drawn across the enclosed area turned on meeting the boundary, according to the law of equal angles of incidence and reflection, which corresponds also to the case of an ideal perfectly smooth non-rotating billiard ball moving in straight lines except when it strikes the

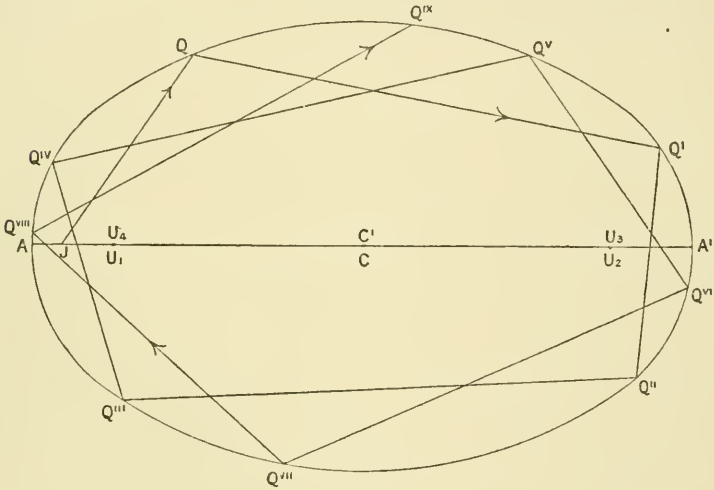


FIG. 3.

boundary of the table; the boundary being of any shape whatever, instead of the ordinary rectangular boundary of an ordinary billiard table, and being perfectly elastic. An interesting illustration, easily seen through a large lecture hall, is had by taking a thin wooden board, cut to any chosen shape, with the corner edges of the boundary smoothly rounded, and winding a stout black cord round and round it many times, beginning with one end fixed to any point, I, of the board. If the pressure of the cord on the edges were perfectly frictionless, the cord would, at every turn round the border, place itself so as to fulfil the law of equal angles of incidence and reflection, modified in virtue of the thickness of the board. For stability, it would be necessary to fix points of the cord to the board by staples pushed in over it at sufficiently frequent intervals, care being taken that at no point is the cord disturbed from its proper straight line by

the staple. [Boards of a considerable variety of shape with cords thus wound on them were shown as illustrations of the lecture.]

§ 36. A very easy way of drawing accurately the path of a particle moving in a plane and reflected from a bounding wall of any shape, provided only that it is not concave externally in any part, is furnished by a somewhat interesting kinematical method illustrated by the accompanying diagram (Fig. 4). It is easily realised by using two equal and similar pieces of board, cut to any desired figure, one of them being turned upside down relatively to the other, so that when the two are placed together with corresponding points in contact, each is the image of the other relative to the plane of contact regarded as a mirror. Sufficiently close corresponding points should be accurately marked on the boundaries of the two figures, and this allows great accuracy to be obtained in the drawing of the free path

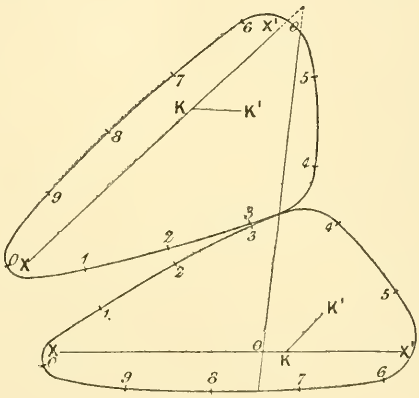


Fig. 4.

after each reflection. The diagram shows consecutive free paths 74·6—32·9 given, and 32·9—54·7, found by producing 74·6—32·9 through the point of contact. The process involves the exact measurement of the length (l)—say to three significant figures—and its inclination (θ) to a chosen line of reference X X′. The summations $\Sigma\, l \cos 2\,\theta$ and $\Sigma\, l \sin 2\,\theta$ give, as explained below, the difference of time integrals of kinetic energies of component motions parallel and perpendicular respectively to X X′, and parallel and perpendicular respectively to K K′, inclined at 45° to X X′. From these differences we find (by a procedure equivalent to that of finding the principal axes of an ellipse) two lines at right angles to one another, such that the time-integrals of the components of velocity parallel to them are respectively greater than and less than those of the components parallel to any other line. [This process was illustrated by models in the lecture.]

§ 37. Virtually the same process as this, applied to the case of a scalene triangle A B C (in which B C = 20 centimetres and the angles A = 97°, B = 29°·5, C = 53°·5), was worked out in the Royal Institution during the fortnight after the lecture, by Mr. Anderson, with very interesting results. The length of each free path (l), and its inclination to B C (θ), reckoned acute or obtuse according to the indications in the diagram, Fig. 5, were measured to the nearest millimetre and the nearest integral degree. The first free path was drawn at random, and the continuation, after 599 reflections (in all 600 paths), were drawn in a manner illustrated by Fig. 5, which shows, for example, a path P Q on one triangle continued to Q R on the other. The two when folded together round the line A B show a path P Q, continued on Q R after reflection. For each path $l \cos 2\theta$ and $l \sin 2\theta$ were calculated and entered in tables with the proper

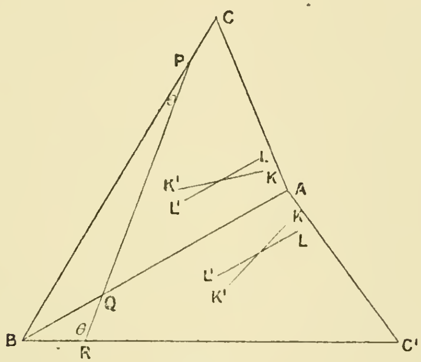


Fig. 5.

algebraic signs. Thus, for the whole 600 paths, the following summations were found:

$$\Sigma l = 3298\,;\ \Sigma l \cos 2\theta = +128\cdot 8\,;\ \Sigma l \sin 2\theta = -201\cdot 9.$$

Remark, now, if the mass of the moving particle is 2, and the velocity one centimetre per second, $\Sigma l \cos 2\theta$ is the excess of the time-integral of kinetic energy of component motion parallel to B C above that of component motion perpendicular to B C, and $\Sigma l \sin 2\theta$ is the excess of the time-integral of kinetic energy of component motion perpendicular to K K′ above that of component motion parallel to K K′; K K′ being inclined at 45° to B C in the direction shown in the diagram. Hence the positive value of $\Sigma l \cos 2\theta$ indicates a preponderance of kinetic energy due to component motion parallel to B C above that of component motion perpendicular to B C; and the negative sign of $\Sigma l \sin 2\theta$ shows preponderance of kinetic energy of component

motion parallel to K K′, above that of component motion perpendicular to K K′. Deducing a determination of two axes at right angles to each other, corresponding respectively to maximum and minimum kinetic energies, we find L L′, being inclined to K K′ in the direction shown, at an angle $= \frac{1}{2}\tan^{-1}\frac{128\cdot 8}{201\cdot 9}$, is what we may call the axis of maximum energy, and a line perpendicular to L L′ the axis of minimum energy; and the excess of the time-integral of the energy of component velocity parallel to L L′ exceeds that of the component perpendicular to L L′ by 239·4 being $\sqrt{128\cdot 8^2 + 201\cdot 9^2}$. This is

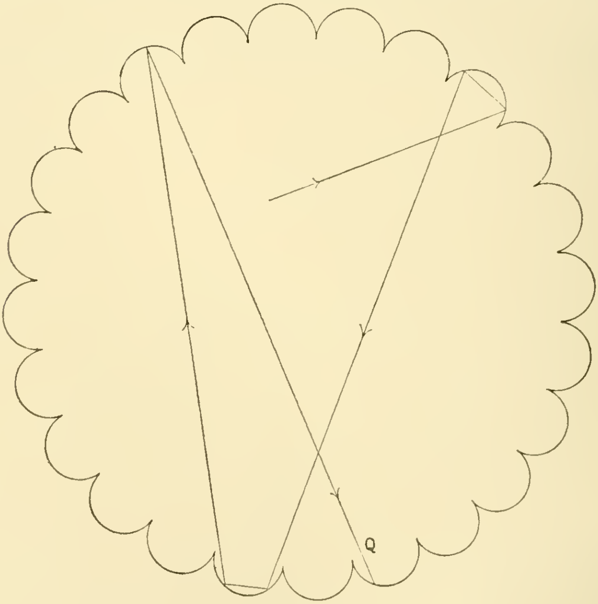


Fig. 6.

7·25 per cent. of the total of $\Sigma\, l$ which is the time-integral of the total energy. Thus, in our result, we find a very notable deviation from the Boltzmann-Maxwell doctrine, which asserts for the present case that the time-integrals of the component kinetic energies are the same for all directions of the component. The percentage which we have found is not very large; and, most probably, summations for several successive 600 flights would present considerable differences, both of the amount of the deviation from equality and the direction of the axes of maximum and minimum energy. Still, I think there is a strong probability that the disproof of the Boltzmann-Maxwell doctrine

is genuine, and the discrepance is somewhat approximately of the amount and direction indicated. I am supported in this view by scrutinising the thirty sums for successive sets of twenty flights: thus I find $\Sigma\, l \cos 2\,\theta$ to be positive for eighteen out of thirty, and $\Sigma\, l \sin 2\,\theta$ to be negative for nineteen out of the thirty.

§ 38. A very interesting test-case is represented in the accompanying diagram, Fig. 6—a circular boundary of semicircular corrugations. In this case it is obvious from the symmetry that the time-integral of kinetic energy of component motion parallel to any straight line must, in the long run, be equal to that parallel to any other. But the Boltzmann-Maxwell doctrine asserts, that the time-integrals of the kinetic energies of the two components, radial and transversal, according to polar co-ordinates, would be equal. To test this, I have taken the case of an infinite number of the semicircular corrugations, so that in the time-integral it is not necessary to include the times between successive impacts of the particle on any one of the semicircles. In this case the geometrical construction would, of course, fail to show the precise point Q at which the free path would cut the diameter AB of the semicircular hollow to which it is approaching; and I have evaded the difficulty in a manner thoroughly suitable for thermodynamic application such as the kinetic theory of gases. I arranged to draw lots for 1 out of the 199 points dividing AB into 200 equal parts. This was done by taking 100 cards,* 0, 1 . . . . 98, 99, to represent distances from the middle point, and, by the toss of a coin, determining on which side of the middle point it was to be (plus or minus for head or tail, frequently changed to avoid possibility of error by bias). The draw for one of the hundred numbers (0 . . . . 99) was taken after very thorough shuffling of the cards in each case. The point of entry having been found, a large scale geometrical construction was used to determine the successive points of impact and the inclination θ of the emergent path to the diameter AB. The inclination of the entering path to the diameter of the semicircular hollow struck at the end of the flight, has the same value θ. If we call the diameter of the large circle unity, the length of each flight is $\sin\theta$. Hence, if the velocity is unity and the mass of the particle 2, the time-integral of the whole kinetic energy is $\sin\theta$; and it is easy to prove that the time-integrals of the components of the velocity, along and perpendicular to the line from each point of the path to the centre of the large circle, are respectively $\theta\cos\theta$, and $\sin\theta - \theta\cos\theta$. The excess of the latter

* I had tried numbered billets (small squares of paper) drawn from a bowl, but found this very unsatisfactory. The best mixing we could make in the bowl seemed to be quite insufficient to secure equal chances for all the billets. Full sized cards like ordinary playing cards, well shuffled, seemed to give a very fairly equal chance to every card. Even with the full-sized cards, electric attraction sometimes intervenes and causes two of them to stick together. In using one's fingers to mix dry billets of card, or of paper, in a bowl, very considerable disturbance may be expected from electrification.

above the former is $\sin\theta - 2\theta\cos\theta$. By summation for 143 flights we have found,

$$\Sigma\sin\theta = 121\cdot 3;\ 2\Sigma\,\theta\cos\theta = 108\cdot 3;$$

whence,

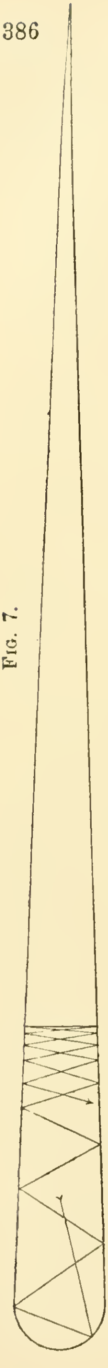
$$\Sigma\sin\theta - 2\,\Sigma\,\theta\cos\theta = 13\cdot 0.$$

This is a notable deviation from the Boltzmann-Maxwell doctrine, which makes $\Sigma(\sin\theta - \theta\cos\theta)$ equal to $\Sigma\,\theta\cos\theta$. We have found the former to exceed the latter by a difference which amounts to $10\cdot 7$ of the whole $\Sigma\sin\theta$.

Out of fourteen sets of ten flights, I find that the time-integral of the transverse component is less than half the whole in twelve sets, and greater in only two. This seems to prove beyond doubt that the deviation from the Boltzmann-Maxwell doctrine is genuine; and that the time-integral of the transverse component is certainly smaller than the time-integral of the radial component.

§ 39. It is interesting to remark that our present result is applicable (see § 38 above) to the motion of a particle, flying about in an enclosed space, of the same shape as the surface of a marlin-spike (Fig. 7). Symmetry shows, that the axes of maximum or minimum kinetic energy must be in the direction of the middle line of the length of the figure and perpendicular to it. Our conclusion is that the time-integral of kinetic energy is maximum for the longitudinal component and minimum for the transverse. In the series of flights, corresponding to the 143 of Fig. 6, which we have investigated, the number of flights is of course many times 143 in Fig. 7, because of the reflections at the straight sides of the marlin-spike. It will be understood, of course, that we are considering merely motion in one plane through the axis of the marlin-spike.

Fig. 7.



§ 40. The most difficult and seriously troublesome statistical investigation in respect to the partition of energy which I have hitherto attempted, has been to find the proportions of translational and rotational energies in various cases, in each of which a rotator experiences multitudinous reflections at two fixed parallel planes between which it moves, or at one plane to which it is brought back by a constant force through its centre of inertia, or by a force varying directly as the distance from the plane. Two different rotators were considered, one of them consisting of two equal masses, fixed at the ends of a rigid massless rod, and each particle reflected on striking either of the planes; the other consisting of

two masses, 1 and 100, fixed at the ends of a rigid massless rod, the smaller mass passing freely across the plane without experiencing any force, while the greater is reflected every time it strikes. The second rotator may be described, in some respects more simply, as a hard massless ball having a mass = 1 fixed anywhere eccentrically within it, and another mass = 100 fixed at its centre. It may be called, for brevity, a biassed ball.

§ 41. In every case of a rotator whose rotation is changed by an impact, a transcendental problem of pure kinematics essentially occurs to find the time and configuration of the first impact; and another such problem to find if there is a second impact, and, if so, to determine it. Chattering collisions of one, two, three, four, five or more impacts, are essentially liable to occur, even to the extreme case of an infinite number of impacts and a collision consisting virtually of a gradually varying finite pressure. Three is the greatest number of impacts we have found in any of our calculations. The first of these transcendental problems, occurring essentially in every case, consists in finding the smallest value of θ which satisfies the equation

$$\theta - i = \frac{\omega a}{v}(1 - \sin\theta);$$

where ω is the angular velocity of the rotator before collision; a is the length of a certain rotating arm; i its inclination to the reflecting plane at the instant when its centre of inertia crosses a plane F, parallel to the reflecting plane and distant a from it; and v is the velocity of the centre of inertia of the rotator. This equation is, in general, very easily solved by calculation (trial and error), but more quickly by an obvious kinematic method, the simplest form of which is a rolling circle carrying an arm of adjustable length. In our earliest work we performed the solution arithmetically, after that kinematically. If the distance between the two parallel planes is moderate in comparison with $2a$ (the effective diameter of the rotator), i for the beginning of the collision with one plane has to be calculated from the end of the preceding collision against the other plane by a transcendental equation, on the same principle as that which we have just been considering. But I have supposed the distance between the two planes to be very great, practically infinite, in comparison with $2a$, and we have therefore found i by lottery for each collision, using 180 cards corresponding to 180° of angle. In the case of the biassed globe, different equally probable values of i through a range of 360° was required, and we found them by drawing from the pack of 180 cards and tossing a coin for plus or minus.

§ 42. Summation for 110 flights of the rotator, consisting of two equal masses, gave as the time-integral of the whole energy, 200·03, and an excess of rotatory above translatory, 42·05. This is just 21 per cent. of the whole; a large deviation from the Boltzmann-

Maxwell doctrine, which makes the time-integrals of translatory and rotatory energies equal.

§ 43. In the solution for the biassed ball (masses 1 and 100), we found great irregularities due to "runs of luck" in the toss for plus or minus, especially when there was a succession of five or six pluses or five or six minuses. We therefore, after calculating a sequence of 200 flights with angles each determined by lottery, calculated a second sequence of 200 flights with the equally probable set of angles given by the same numbers with altered signs. The summation for the whole 400 gave 555·55 as the time-integral of the whole energy, and an excess, 82·5, of the time-integral of the translatory, over the time-integral of the rotatory energy. This is nearly 15 per cent. We cannot, however, feel great confidence in this result, because the first set of 200 made the translatory energy less than the rotatory energy by a small percentage (2·3) of the whole, while the second 200 gave an excess of translatory over rotatory amounting to 35·9 per cent. of the whole.

§ 44. All our examples considered in detail or worked out, hitherto, belong to Class I. of § 28. As a first example of Class II., consider a case merging into the geodetic line on a closed surface S. Instead of the point being constrained to remain on the surface, let it be under the influence of a field of force, such that it is attracted towards the surface with a finite force, if it is placed anywhere very near the surface on either side of it, so that if the particle be placed on S and projected perpendicularly to it, either inwards or outwards, it will be brought back before it goes farther from the surface than a distance h, small in comparison with the shortest radius of curvature of any part of the surface. The Boltzmann-Maxwell doctrine asserts that the time-integral of kinetic energy of component motion normal to the surface, would be equal to half the kinetic energy of component motion at right angles to the normal; by normal being meant, a straight line drawn from the actual position of the point at any time perpendicular to the nearest part of the surface S. This, if true, would be a very remarkable proposition. If h is infinitely small, we have simply the mathematical condition of constraint to remain on the surface, and the path of the particle is exactly a geodetic line. If the force towards S is zero, when the distance on either side of S is $\pm h$, we have the case of a particle placed between two guiding surfaces with a very small distance, $2h$, between them. If S, and therefore each of the guiding surfaces, is in every normal section convex outwards, and if the particle is placed on the outer guide-surface, and projected in any direction in it, with any velocity, great or small, it will remain on that guide-surface for ever, and travel along a geodetic line. If now it be deflected very slightly from motion in that surface, so that it will strike against the inner guide-surface, we may be quite ready to learn, that the energy of knocking about between the two surfaces, will grow up from something very small in the beginning, till, in the long run, its time-integral is

comparable with the time-integral of the energy of component motion parallel to the tangent plane of either surface. But will its ultimate value be exactly half that of the tangential energy, as the doctrine tells us it would be? We are, however, now back to Class I.; we should have kept to Class II., by making the normal force on the particle always finite, however great.

§ 45. Very interesting cases of Class II., § 28, occur to us readily in connection with the cases of Class I. worked out in §§ 38, 41, 42, 43.

§ 46. Let the radius of the large circle in § 38 become infinitely great: we have now a plane F (floor) with *semicircular cylindric hollows*, or semicircular hollows as we shall say for brevity; the motion being confined to one plane perpendicular to F, and to the edges of the hollows. For definiteness we shall take for F the plane of the edges of the hollows. Instead now of a particle after collision flying along the chord of the circle of § 38, it would go on for ever in a straight line. To bring it back to the plane F, let it be acted on either (α) by a force towards the plane in simple proportion to the distance, or (β) by a constant force. This latter supposition (β) presents to us the very interesting case of an elastic ball bouncing from a corrugated floor, and describing gravitational parabolas in its successive flights, the durations of the different flights being in simple proportion to the component of velocity perpendicular to the plane F. The supposition (α) is purely ideal; but, it is interesting because it gives a half curve of sines for each flight, and makes the times of flight from F after a collision and back again to F the same for all the flights, whatever be the inclination on leaving the floor and returning to it. The supposition (β) is illustrated in Fig. 8, with only the variation that the corrugations are convex instead of concave, and that two vertical planes are fixed to reflect back the particle, instead of allowing it to travel indefinitely, either to right or to left.

§ 47. Let the rotator of §§ 41 to 43, instead of bouncing to and fro between two parallel planes, impinge only on one plane F, and let it be brought back by a force through its centre of inertia, either (α) varying in simple proportion to the distance of the centre of inertia from F, or (β) constant. Here, as in § 46, the times of flight in case (α) are all the same, and in (β) they are in simple proportion to the velocity of its centre of inertia when it leaves F or returns to it.

§ 48. In the cases of §§ 46, 47, we have to consider the time-integral for each flight of the kinetic energy of the component velocity of the particle perpendicular to F, and of the whole velocity of the centre of inertia of the rotator, which is itself perpendicular to F. If q denotes the velocity perpendicular to F of the particle, or of the centre of inertia of the rotator, at the instants of crossing F at the beginning and end of the flight, and if 2 denotes the mass of the particle or of the rotator so that the kinetic energy is the same as the square of the velocity, the time-integral is in case (α) $\frac{1}{2}q^2$ T and in case (β) $\frac{1}{3}q^2$ T, the time of the flight being denoted in each case by T.

In both (α) and (β), § 46, if we call 1 the velocity of the particle, which is always the same, we have $q^2 = \sin^2\theta$, and the other component of the energy is $\cos^2\theta$. In § 47 it is convenient to call

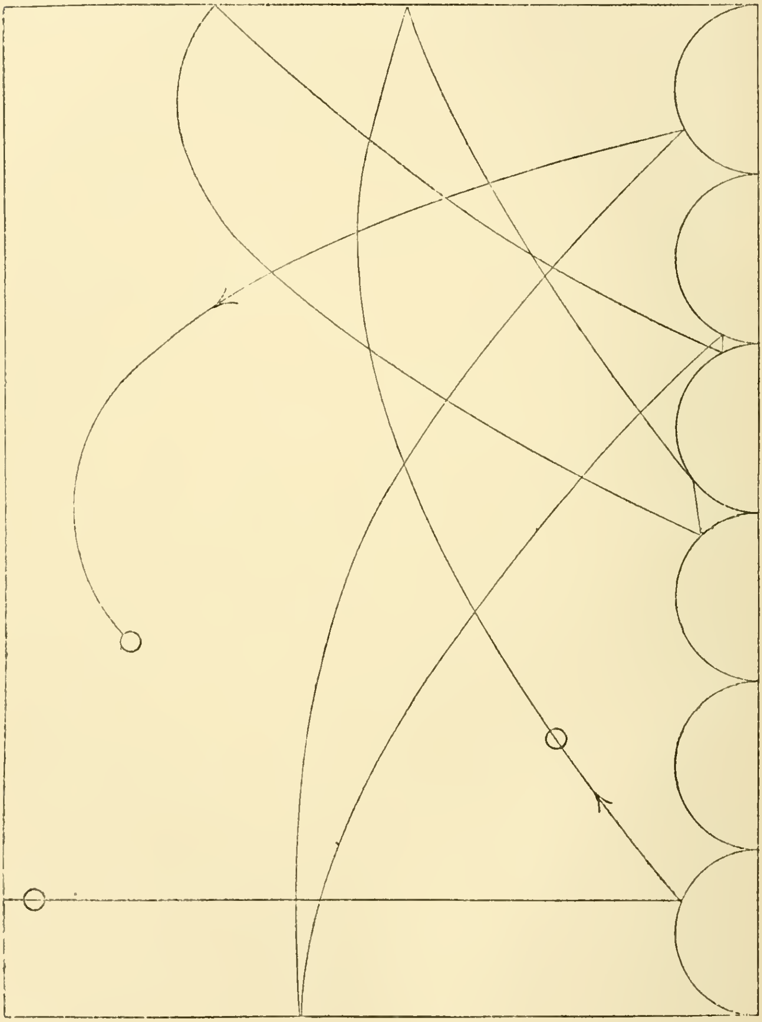


Fig. 8.

the total energy 1; and thus $1 - q^2$ is the total rotational energy, which is constant throughout the flight. Hence, remembering that the times of flight are all the same in case (α) and are proportional

to the value of q in case (β); in case (α), whether of § 46 or § 47, the time-integrals of the kinetic energies to be compared are as $\frac{1}{2}\Sigma q^2$ to $\Sigma(1 - q^2)$, and in case (β) they are as $\frac{1}{3}\Sigma q^3$ and $\Sigma q(1 - q^2)$.

§ 49. Hence with the following notation—

$$\text{In § 46}\begin{cases}\text{time-integral of kinetic energy perpendicular to F,} = \mathrm{V}\\ \quad\text{"} \qquad\qquad \text{"} \qquad\qquad \text{parallel to F,} = \mathrm{U}\end{cases}$$

$$\text{In § 47}\begin{cases}\quad\text{"} \qquad \text{translatory energy} = \mathrm{V}\\ \quad\text{"} \qquad \text{rotatory} \quad \text{"} \quad = \mathrm{R}\end{cases}$$

we have

$$\frac{\mathrm{V}-\mathrm{U}}{\mathrm{V}+\mathrm{U}}\begin{cases}= \dfrac{\Sigma(\frac{3}{2}q^2 - 1)}{\Sigma(1 - \frac{1}{2}q^2)} \text{ in case } (\alpha)\\[2ex] = \dfrac{\Sigma(\frac{4}{3}q^3 - q)}{\Sigma(q - \frac{2}{3}q^3)} \quad \text{"} \quad (\beta)\end{cases}$$

$$\frac{\mathrm{V}-\mathrm{R}}{\mathrm{V}+\mathrm{R}}\begin{cases}= \dfrac{\Sigma(\frac{3}{2}q^2 - 1)}{\Sigma(1 - \frac{1}{2}q^2)} \quad \text{"} \quad (\alpha)\\[2ex] = \dfrac{\Sigma(\frac{4}{3}q^3 - q)}{\Sigma(q - \frac{2}{3}q^3)} \quad \text{"} \quad (\beta)\end{cases}$$

§ 49. By the processes described above, q was calculated for the single particle and corrugated floor (§ 46), and for the rotator of two equal masses each impinging on a fixed plane (§§ 41, 42), and for the biassed ball (central and eccentric masses 100 and 1 respectively, §§ 41, 43). Taking these values of q, summing q, q^2 and q^3 for all the flights, and using the results in § 48, we find the following six results:

Single particle bounding from corrugated floor (semicircular hollows), 143 flights :—

$$\frac{\mathrm{V}-\mathrm{U}}{\mathrm{V}+\mathrm{U}}\begin{cases}= +\cdot 197 \text{ for isochronous sinusoidal flights.}\\ = +\cdot 136 \text{ for gravitational parabolic} \quad \text{"}\end{cases}$$

Rotator of two equal masses, 110 flights :—

$$\frac{\mathrm{V}-\mathrm{R}}{\mathrm{V}+\mathrm{R}}\begin{cases}= -\cdot 179 \text{ for isochronous sinusoidal flights.}\\ = -\cdot 150 \text{ for gravitational parabolic} \quad \text{"}\end{cases}$$

Biassed ball, 400 flights :—

$$\frac{\mathrm{V}-\mathrm{R}}{\mathrm{V}+\mathrm{R}}\begin{cases}= +\cdot 025 \text{ for isochronous sinusoidal flights.}\\ = -\cdot 014 \text{ for gravitational parabolic} \quad \text{"}\end{cases}$$

The smallness of the deviation of the last two results from what the Boltzmann-Maxwell doctrine makes them, is very remarkable when we compare it with the 15 per cent. which we have found (§ 43 above) for the biassed ball bounding free from force, to and fro between two parallel planes.

§ 50. The last case of partition of energy which we have worked out statistically, relates to an impactual problem belonging partly to Class I., § 28, and partly to Class II. It was designed as a nearer approach to practical application in thermodynamics than any of those hitherto described. It is, in fact, a one-dimensional illustration of the kinetic theory of gases. Suppose a row of a vast number of atoms, of equal masses, to be allowed freedom to move only in a straight line between fixed bounding planes L and K. Let P the atom next K be caged between it and a parallel plane C, at a distance from it very small in comparison with the average of the free paths of the other particles; and let Q, the atom next to P, be perfectly free to cross the cage-front C, without experiencing force from it. Thus, while Q gets freely into the cage to strike P, P cannot follow it out beyond the cage-front. The atoms being all equal, every simple impact would produce merely an interchange of velocities between the colliding atoms, and no new velocity could be introduced, if the atoms were perfectly hard (§ 16 above), because this implies that no three can be in collision at the same time. I do not, however, limit the present investigation to perfectly hard atoms. But, to simplify our calculations, we shall suppose P and Q to be infinitely hard. All the other atoms we shall suppose to have the property defined in § 21 above. They may pass through one another in a simple collision, and go asunder each with its previous velocity unaltered, if the differential velocity be sufficiently great; they must recoil from one another with interchanged velocities if the initial differential velocity was not great enough to cause them to go through one another. Fresh velocities will generally be introduced, by three atoms being in collision at the same time, so that even if the velocities were all equal, to begin with, inequalities would supervene in virtue of three or more atoms being in collision at the same time; whether the initial differential velocities be small enough to result in two recoils, or whether one or both the mutual approaches lead to a passage or passages through one another. Whether the distribution of velocities, which must ultimately supervene, is or is not according to the Maxwellian law, we need not decide in our minds; but, as a first example, I have supposed the whole multitude to be given with velocities distributed among them according to that law (which, if they were infinitely hard, they would keep for ever after); and we shall further suppose equal average spacing in different parts of the row, so that we need not be troubled with the consideration of waves, as it were of sound, running to and fro along the row.

§ 51. For our present problem we require two lotteries, to find the influential conditions at each instant, when Q enters P's cage—lottery I. for the velocity (v) of Q at impact; lottery II. for the phase of P's motion. For lottery I. (after trying 837 small squares of paper with velocities written on them and mixed in a bowl, and finding the plan unsatisfactory), we took nine stiff cards, numbered

1, 2 . . . . 9, of the size of ordinary playing cards, with rounded corners, with one hundred numbers written on each in ten lines of ten numbers. The velocities on each card are shown on the following table. The number of times each velocity occurs was chosen to fulfil as nearly as may be the Maxwellian law, which is $C\,d\,v\,\epsilon^{-\frac{v^2}{k}}$ = the number of velocities between $v + \frac{1}{2}\,d\,v$ and $v - \frac{1}{2}\,d\,v$. We took $k = 1$, which, if $d\,v$ were infinitely small, would make the mean of the squares of the velocities equal exactly to ·5; we took $d\,v = {\cdot}1$ and $C\,d\,v = 108$, to give, as nearly as circumstances would allow, the

TABLE SHOWING THE NUMBER OF THE DIFFERENT VELOCITIES ON THE DIFFERENT CARDS.

Velocity.	·1	·2	·3	·4	·5	·6	·7	·8	·9	1·0	1·1	1·2	1·3	1·4	1·5	1·6	1·7	1·8	1·9	2·0	2·1	2·2	
Card 1	100																						
„ 2	7	93																					
„ 3		10	90																				
„ 4			9	91																			
„ 5				1	84	15																	
„ 6						60	40																
„ 7							26	57	17														
„ 8									31	40	29												
„ 9											3	26	19	15	11	9	6	4	3	2	1	1	$\Sigma\,v$
Sums of velocities	107	103	99	92	84	75	66	57	48	40	32	26	19	15	11	9	6	4	3	2	1	1	900

Maxwellian law, and to make the total number of different velocities 900. The sum of the squares of all these 900 velocities is 468·4, which divided by 900 is ·52. In the practice of this lottery, the numbered cards were well shuffled and then one was drawn; the particular one of the hundred velocities on this card to be chosen was found by drawing one card from a pack of one hundred numbered 1, 2 . . . 99, 100. In lottery II. a pack of one hundred cards is used to draw one of one hundred decimal numbers from ·01 to 1·00. The decimal drawn, called a, shows the proportion of the whole period of P from the cage-front C, to K, and back to C, still unperformed at the instant when Q crosses C. Now remark, that if Q overtakes P in the first half of its period, it gives its velocity, v, to P and follows it inwards; and therefore there must be a second impact when P meets it after reflection from K and gives it back the velocity v which it had on entering. If Q meets P in the second half of its period, Q

will, by the first impact, get P's original velocity, and may with this velocity escape from the cage. But it may be overtaken by P before it gets out of the cage, in which case it will go away from the cage with its own original velocity v unchanged. This occurs always if, and never unless, u is less than $v\ a$; P's velocity being denoted by u, and Q's by v. This case of Q overtaken by P can only occur if the entering velocity of Q is greater than the speed of P before collision. Except in this case, P's speed is unchanged by the collision. Hence we see, that it is only when P's speed is greater than Q's before collision, that there can be interchange, and this interchange leaves P with less speed than Q. If every collision involved interchange, the average velocity of P would be equalised by the collisions to the average velocity of Q, and the average distribution of different velocities would be identical for Q and P. Non-fulfilment of this equalising interchange can, as we have seen, only occur when Q's speed is less than P's, and therefore the average speed and the average kinetic energy of P must be less than the average kinetic energy of Q.

§ 52. We might be satisfied with this, as directly negativing the Boltzmann-Maxwell doctrine for this case. It is, however, interesting to know, not only that the average kinetic energy of Q is greater than that of the caged atom, but, further, to know how much greater it is. We have therefore worked out summations for 300 collisions between P and Q, beginning with $u^2 = \cdot 5$ $(u = \cdot 71)$, being approximately the mean of v^2 as given by the lottery. It would have made no appreciable difference in the result if we had begun with any value of u, large or small, other than zero. Thus, for example, if we had taken 100 as the first value of u, this speed would have been taken by Q at the first impact, and sent away along the practically infinite row, never to be heard of again; and the next value of u would have been the first value drawn by lottery for v. Immediately before each of the subsequent impacts, the velocity of P is that which it had from Q by the preceding impact. In our work, the speeds which P actually had at the first sixteen times of Q's entering the cage were ·71, ·5, ·3, ·2, ·2, ·1, ·1, ·2, ·2, ·5, ·7, ·2, ·3, ·6, 1·5, ·5—from which we see how little effect the choice of ·71 for the first speed of P had on those that follow. The summations were taken in successive groups of ten; in every one of these $\Sigma\, v^2$ exceeded $\Sigma\, u^2$. For the 300 we found $\Sigma\, v^2 = 148\cdot 53$ and $\Sigma\, u^2 = 61\cdot 62$, of which the former is 2·41 times the latter. The two ought to be equal according to the Boltzmann-Maxwell doctrine. Dividing $\Sigma\, v^2$ by 300 we find ·495, which chances to more nearly the ·5 we intended than the ·52 which is on the cards (§ 51 above). A still greater deviation (2·71 instead of 2·41) was found by taking $\Sigma\, v^3$ and $\Sigma\, u'^2\, v$ to allow for greater probability of impact with greater than with smaller values of v; u' being the velocity of P after collision with Q.

§ 53. We have seen in § 52 that $\Sigma\, u^2$ must be less than $\Sigma\, v^2$, but it seemed interesting to find how much less it would be with

some other than the Maxwellian law of distribution of velocities. We therefore arranged cards for a lottery, with an arbitrarily chosen distribution, quite different from the Maxwellian. Eleven cards, each with one of the eleven numbers 1, 3 . . . . 19, 21, to correspond to the different velocities ·1, ·3 . . . . 1·9, 2·1, were prepared and used instead of the nine cards in the process described in § 51 above. In all except one of the eleven tens, Σv^2 was greater than Σu^2, and for the whole 110 impacts we found $\Sigma v^2 = 179 \cdot 90$, and $\Sigma u^2 = 97 \cdot 66$; the former of these is 1·84 times the latter. In this case we found the ratio of Σv^3 to $\Sigma u'^2 v$ to be 1·87.

§ 54. In conclusion, I wish to refer, in connection with Class II. § 28, to a very interesting and important application of the doctrine, made by Maxwell himself, to the equilibrium of a tall column of gas under the influence of gravity. Take, first, our one-dimensional gas of § 50, consisting of a straight row of a vast number of equal and similar atoms. Let now the line of the row be vertical, and let the atoms be under the influence of terrestrial gravity, and suppose, first, the atoms to resist mutual approach, sufficiently to prevent any one from passing through another with the greatest relative velocity of approach that the total energy given to the assemblage can allow. The Boltzmann-Maxwell doctrine (§ 18 above) asserting as it does that the time integral of the kinetic energy is the same for all the atoms, makes the time-average of the kinetic energy the same for the highest as for the lowest in the row. This, if true, would be an exceedingly interesting theorem. But now, suppose two approaching atoms not to repel one another with infinite force at any distance between their centres, and suppose energy to be given to the multitude sufficient to cause frequent instances of two atoms passing through one another. Still the doctrine can assert nothing but that the time-integral of the kinetic energy of any one atom is equal to that of any other atom, which is now a self-evident proposition, because the atoms are of equal masses, and each one of them in turn will be in every position of the column, high or low. (If in the row there are atoms of different masses, the Waterston-Maxwell doctrine of equal average energies would, of course, be important and interesting.)

§ 55. But now, instead of our ideal one-dimensional gas, consider a real homogeneous gas, in an infinitely hard vertical tube, with an infinitely hard floor and roof, so that the gas is under no influence from without, except gravity. First, let there be only two or three atoms, each given with sufficient velocity to fly against gravity from floor to roof. They will strike one another occasionally, and they will strike the sides and floor and roof of the tube much more frequently than one another. The time-averages of their kinetic energies will be equal. So will they be if there are twenty atoms, or a thousand atoms, or a million, million, million, million, million atoms. *Now* each atom will strike another atom much more frequently than the sides or floor or roof of the tube. In the long run

each atom will be in every part of the tube as often as is every other atom. The time-integral of the kinetic energy of any one atom will be equal to the time-integral of the kinetic energy of any other atom. This truism is simply and solely all that the Boltzmann-Maxwell doctrine asserts for a vertical column of a homogeneous monatomic gas. It is, I believe, a general impression that the Boltzmann-Maxwell doctrine, asserting a law of partition of the kinetic part of the whole energy, includes obviously a theorem that the average kinetic energy of the atoms in the upper parts of a vertical column of gas, are equal to those of the atoms in the lower parts of the column. Indeed, with the wording of Maxwell's statement, § 18, before us, we might suppose it to assert that two parts of our vertical column of gas, if they contain the same number of atoms, must have the same kinetic energy, though they be situated, one of them near the bottom of the column, and the other near the top. Maxwell himself, in his 1866 paper ('The Dynamical Theory of Gases'),* gave an independent synthetical demonstration of this proposition, and did not subsequently, so far as I know, regard it as immediately deducible from the partitional doctrine generalised by Boltzmann and himself several years after the date of that paper.

§ 56. Both Boltzmann and Maxwell recognised the experimental contradiction of their doctrine presented by the kinetic theory of gases, and felt that an explanation of this incompatibility was imperatively called for. For instance, Maxwell, in a lecture on the dynamical evidence of the molecular constitution of bodies, given to the Chemical Society, Feb. 18, 1875, said: "I have put before you "what I consider to be the greatest difficulty yet encountered by the "molecular theory. Boltzmann has suggested that we are to look for "the explanation in the mutual action between the molecules and the "ethereal medium which surrounds them. I am afraid, however, that "if we call in the help of this medium we shall only increase the "calculated specific heat, which is already too great." Rayleigh, who has for the last twenty years been an unwavering supporter of the Boltzmann-Maxwell doctrine, concludes a paper 'On the Law of Partition of Energy,' published a year ago in the Phil. Mag., Jan. 1900, with the following words: "The difficulties connected with "the application of the law of equal partition of energy to actual gases "have long been felt. In the case of argon and helium and mercury "vapour, the ratio of specific heats (1·67) limits the degrees of freedoms "of each molecule to the three required for translatory motion. The "value (1·4) applicable to the principal diatomic gases, gives room for "the three kinds of translation and for two kinds of rotation. Nothing "is left for rotation round the line joining the atoms, nor for relative "motion of the atoms in this line. Even if we regard the atoms as "mere points, whose rotation means nothing, there must still exist

* Addition, of date December 17, 1866. Collected works, vol. ii. p. 76.

"energy of the last-mentioned kind, and its amount (according to law) "should not be inferior.

"We are here brought face to face with a fundamental difficulty, "relating not to the theory of gases merely, but rather to general "dynamics. In most questions of dynamics, a condition whose violation "involves a large amount of potential energy may be treated as a "constraint. It is on this principle that solids are regarded as rigid, "strings as inextensible, and so on. And it is upon the recognition "of such constraints that Lagrange's method is founded. But the law "of equal partition disregards potential energy. However great may "be the energy required to alter the distance of the two atoms in a "diatomic molecule, practical rigidity is never secured, and the kinetic "energy of the relative motion in the line of junction is the same as if "the tie were of the feeblest. The two atoms, however related, remain "two atoms, and the degrees of freedom remain six in number.

"What would appear to be wanted is some escape from the "destructive simplicity of the general conclusion."

The simplest way of arriving at this desired result is to deny the conclusion; and so, in the beginning of the twentieth century, to lose sight of a cloud which has obscured the brilliance of the molecular theory of heat and light during the last quarter of the nineteenth century.

[K.]

ANNUAL MEETING,

Tuesday, May 1, 1900.

Sir James Crichton-Browne, M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1899, testifying to the continued prosperity and efficient management of the Institution, was read and adopted, and the Report on the Davy Faraday Research Laboratory of the Royal Institution, which accompanied it, was also read.

Sixty-three new Members were elected in 1899.

Sixty Lectures, Seventeen Evening Discourses and two Centenary Commemoration Lectures were delivered in 1899.

The Books and Pamphlets presented in 1899 amounted to about 280 volumes, making, with 672 volumes (including Periodicals bound) purchased by the Managers, a total of 952 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Honorary Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

President—The Duke of Northumberland, K.G. F.S.A.
Treasurer—Sir James Crichton-Browne, M.D. LL.D. F.R.S.
Secretary—Sir William Crookes, F.R.S.

Managers.

Sir Frederick Abel, Bart. K.C.B. D.C.L. LL.D. F.R.S.
Sir Frederick Bramwell, Bart. D.C.L. LL.D. F.R.S. M.Inst.C.E.
Thomas Buzzard, M.D. F.R.C.P.
Sir William James Farrer, M.A. F.S.A.
Hugh Leonard, Esq. F.S.A. M.Inst.C.E.
The Right Hon. Lord Lister, M.D. D.C.L. LL.D. Pres.R.S. F.R.C.S.
Frank McClean, Esq. M.A. LL.D. F.R.S. F.R.A.S.
Raphael Meldola, Esq. F.R.S. F.R.A.S.
Ludwig Mond, Esq. Ph.D. F.R.S.
Sir Andrew Noble, K.C.B. F.R.S. M.Inst.C.E.
Alexander Siemens, Esq. M.Inst.C.E.
Basil Woodd Smith, Esq. F.R.A.S. F.S.A.
William Hugh Spottiswoode, Esq. F.C.S.
The Hon. Sir James Stirling, M.A. LL.D.
Sir Henry Thompson, Bart. F.R.C.S. F.R.A.S.

Visitors.

Charles Edward Beevor, M.D. F.R.C.P.
William Henry Bennett, Esq. F.R.C.S.
W. A. B. Burdett-Coutts, Esq. M.P. M.A.
Joseph G. Gordon, Esq. F.C.S.
Maures Horner, Esq. J.P. F.R.A.S.
Sir Alexander Campbell Mackenzie, Mus.Doc.
Henry Francis Makins, Esq. F.R.G.S.
Carl Edward Melchers, Esq.
John Callander Ross, Esq.
William James Russell, Esq. Ph.D. F.R.S.
Alan A. Campbell Swinton, Esq. M.Inst.C.E.
Sir James Vaughan, B.A. J.P.
John Jewell Vezey, Esq. F.R.M.S.
Colonel H. Watkin, C.B. R.A.
James Wimshurst, Esq. F.R.S.

WEEKLY EVENING MEETING,

Friday, May 4, 1900.

Sir William Crookes, F.R.S., Honorary Secretary and Vice-President, in the Chair.

Professor T. E. Thorpe, Ph.D. LL.D. D.Sc. F.R.S. *M.R.I.*

Pottery and Plumbism.

When I came in here this afternoon to see to the arrangement of the specimens with which I wish to illustrate what I have to bring before you to-night, I was so forcibly impressed with the unwonted appearance of this table—an appearance, it seemed to me, of domesticity bordering on the commonplace, that I feel that something almost in the nature of an apology is due to you. I venture, however, to remind you that the subject of my discourse is precisely of that character which the eminent founder of this Institution had in view when he established it. Count Rumford created the Royal Institution with the object "of diffusing a knowledge and facilitating the general introduction of useful mechanical inventions and improvements; and for teaching, by courses of philosophical lectures and experiments, the application of science to the common purposes of life."

I need hardly tell you that the craft of the potter largely depends upon the intelligent application of scientific principles. Whether, however, science has entered into it to the extent that might be desired is, perhaps, open to question. At all events, I shall have failed in my object to-night if I do not succeed in showing you that the application of simple chemical principles may largely obviate one of the evils with which that craft, as practised in this country, is attended.

Many in this room are, no doubt, aware that within recent times public attention has been pointedly drawn to the serious amount of lead poisoning which follows the use of compounds of lead in various operations in the manufacture of pottery. This lead poisoning is mainly to be attributed to the solubility of these compounds in the secretions of the body, as in the saliva, mucus, and especially in the gastric juice. The lead may be introduced into the system in a variety of ways—thus, it may be breathed as dust derived from the dried and finely-divided glazing material, or from pigments used in decoration; or it may be conveyed to the mouth by eating food with imperfectly washed hands, and in other ways which I need not particularise. It is not necessary to trouble you with any account of the

distressing nature of lead poisoning; of the total or partial paralysis, sometimes ending in death, to which it gives rise; of the blindness, debility, and prolonged misery which, even in its less acute forms, it occasions. Nor do I care to dwell upon the statistics the authorities have been able to collect respecting its prevalence. It is sufficient to say that it is allowed by all to have existed to an extent which constitutes a grave reflection upon the conduct of one of the most important of our staple industries, and public sentiment has demanded that some steps shall be taken to remove the evil.

A very few words will serve to explain to you how the mischief arises. Without attempting to be comprehensive or to enter into too great detail, I may say that, broadly speaking, the articles of pottery with which I shall concern myself to-night, in order to explain the position, group themselves into the two main classes of earthenware and china or porcelain.

Earthenware is made of a mixture of so called ball-clay, china-clay, china-stone, and flint. The ball-clay and china-clay are substantially more or less pure silicate of alumina, derived from the decomposition of felspar; the china-stone is a silicate of alumina containing more or less undecomposed felspar; and flint is practically pure silica. These substances, in various proportions, and intimately commingled, constitute the paste from which the article of earthenware is fashioned. The various modes of fashioning the article, whether by throwing on the wheel, pressing, or casting, I need not stop to explain.

The articles so made, after a preliminary drying, are packed in large earthenware vessels made of fireclay, technically known as "saggers," and are heated to a temperature, depending on their nature, in an oven. In this form the fired ware is known as "biscuit." It is hard and sonorous, but is pervious to liquids, and on account of its slightly roughened surface, would rapidly collect dust, and thereby become soiled. For most purposes, therefore, it requires to be glazed—that is, coated with some sufficiently fusible material capable of rendering it impervious to liquids.

The greater part of the porcelain made in this country is of the variety known as soft-paste porcelain, and differs essentially from that made in China or in various Continental countries. Chinese and Continental porcelains consist mainly of mixtures of china-clay and felspar. English porcelain is composed of china-clay and china-stone, mixed with nearly their joint weight of bone-ash or phosphate of lime. The English porcelain or bone "biscuit," like earthenware "biscuit," is pervious to liquids, and therefore requires to be glazed.

The glazing of both varieties is done in substantially the same manner—that is, the articles are dipped in a thin cream-like fluid containing, in a state of suspension, finely divided double silicates, or silico-borates of alumina, alkalis and alkaline-earths, associated for the most part with lead compounds in amount often reaching to half the weight of the glazing material.

The capillarity of the porous biscuit draws in a certain amount of the cream-like liquid, causing the deposition upon its surface of a thin film of the glazing material. The article is then allowed to dry, is trimmed or cleaned, if necessary, to remove any aggregation or superfluity of the glazing material from its edges, or other projecting parts, and again placed in the "saggers," and re-heated in another oven, technically known as a "glost oven," so as to cause the glazing material to fuse, and spread evenly over the body of the ware as a transparent vitreous covering. If the article is decorated by a coloured design, this is usually done before dipping, the "biscuit" being either painted in so-called underglaze colours, or the design transferred to it from a lithographic print, or otherwise manipulated, depending on the character of the decoration.

(The lecturer here exhibited several slides upon the screen of photographs lent by the Rev. Malcolm Graham, vicar of St. Paul's, Burslem, and Mr. J. H. Walmsley, H.M. Inspector of factories, Stoke. These showed groups of workpeople engaged in the various processes of the manufacture of pottery in which lead is used.)

Proceeding, he said :—Some judgment is required in the selection of the materials needed to form an appropriate glaze. The glaze to be efficient should be clear, transparent and lustrous. It should not only be without injurious action on any colours it may have to cover, but ought, if possible, to enhance their brilliancy. It must be sufficiently thin, and of a proper degree of fusibility, so as not to interfere with or impair any modelling on the ware. It must be sufficiently hard and insoluble to resist a fair amount of wear, especially in culinary articles, or those intended for general domestic use; and it must not be attacked by such acids as may be found in food. Lastly, it must not "craze": that is, it must have substantially the same thermal expansibility as the body of the article to which it adheres, otherwise it will chip off, or break up into cracks. Crazing not only renders the ware unsightly, but causes it, in the course of time, especially in the case of culinary and table ware, to absorb oils and fats, &c., and thus to become unclean, and, it is said, to harbour even the ubiquitous microbe.

These conditions are, on the whole, fairly well realised in ordinary earthenware. It is, however, in hard porcelain, and more especially in the masterpieces of Oriental manufacture, and in the produce of the leading Continental factories, and, to a large extent in the soft porcelain made in this country by the best makers, that the finest results are obtained. This, in the case of hard porcelain, is due mainly to the circumstance that the chemical nature of the glaze more nearly approaches that of the body of the ware than is the case with earthenware, and that at the high temperature at which the porcelain is fired, there is a more complete interfusion of the glaze and the biscuit.

Moreover, the glaze of hard porcelain, being practically fused felspar, is harder even than glass, and much more resistant to the

action of ordinary acids and alkalis. For this reason the analytical chemist prefers to use vessels of hard porcelain rather than of glass in operations in which it is desirable to exclude minute portions of foreign substances. At the same time, experience has shown that the susceptibility of glass to the action of water, or solutions of alkalis, acids and salts, may be considerably modified by due regard to its composition.

By far the greater portion of the domestic and sanitary ware and china made in this country, together with a great variety of articles, such as glazed bricks, wall- and hearth-tiles, so-called "china furniture"—door-knobs, finger-plates, escutcheons, bell-handle fittings, discs for water-taps, &c.,—electrical sundries, as insulators and fittings for electric-light installations—and countless other articles employed partly for use and partly for ornament, are glazed with materials containing compounds of lead.

The lead may be present in the dipping tub as white lead or red lead, together known technically as "raw" lead, or these substances or litharge may be previously heated with some form of sufficiently pure silica—usually in this country calcined and powdered flint—whereby a fusible lead silicate is formed, which is then powdered and mixed with the other materials of the glaze. Or the red lead or litharge may be added to a mixture of powdered flint and china-stone, or china-clay and felspar, and the whole fused together to form what is substantially a double silicate of lead and alumina, with, it may be, small quantities of admixed lime and alkalis, this fritt being added as before, in the requisite proportion to the rest of the glazing materials, such as borax, chalk, flint, china-stone, also, for the most part, previously fritted together; or, lastly, the whole materials of the glaze may be melted together, so as to form a vitreous, homogeneous mass.

The practice differs in different works, and is, for the most part, purely empirical; each manufacturer adhering as closely as he can to the composition which he finds by experience gives what he is content to regard as satisfactory when used in conjunction with the particular mixture of ball-clay, china-stone, china-clay and flint, &c., he employs for the body of the ware, and when fired or otherwise manipulated as he deems best. Hence it follows that no two manufacturers use a glaze of precisely the same proximate or ultimate chemical composition, unless, indeed, they have been supplied, as not infrequently happens, with their glazing material from a professional glaze-maker.

The amount of lead, calculated as lead oxide, and on the dry material of the glaze, in the case of earthenware or ordinary English china, may vary from 12 or 13 per cent. to as much as 21 or 22 per cent. That used for covering tiles, especially the decorated and coloured varieties, may contain upwards of half its weight of lead oxide. This lead oxide may either be as "raw" lead—that is, white lead or occasionally red lead; or it may be "fritted" lead—that is,

a simple silicate of lead containing about 70 per cent. of lead oxide; or it may be as a double or compound silicate of lead, alumina and lime, &c., containing from 20 to 50 per cent., or even more, of oxide of lead.

There must, of course, be some advantage attending the use of lead compounds in earthenware and soft china glazes, otherwise their employment would not be practically universal. There can be no question that lead glazes do, on the whole, fairly fulfil the properties needed in a glaze. They are transparent and lustrous, have no hurtful action on such colour-producing oxides as are used in decoration; they are sufficiently hard, unless the amount of lead is excessive, and hence stand a reasonable amount of wear; they are not absolutely unattacked by acids, but provided that the amount of lead is not too large, vinegar and the acids which may be present in food are without any very marked solvent action. Lastly, unless the covering is very thick, or its composition is such as to constitute an injudicious adaptation to that of the body, the glazing does not, as a rule, "craze" to an inconvenient extent. To these advantages must be added that of ready fusibility; hence the "glost oven" may be fired at a relatively low temperature, which means not only economy in time and fuel, but less risk of loss by deformation of the ware.

Unfortunately, as you have been told, these advantages have been purchased at the cost of a good deal of human suffering, and, as you have learned, of late years the evil of plumbism in the potteries has grown to such an extent as to constitute a grave scandal. The Home Office, with whom is vested the superintendence of the execution of the Factory and Workshops Acts, has more than once attempted to deal with this evil. A section of the Press has roused the attention of the public to its magnitude, and Parliament has at length intervened and demanded that something shall be done to arrest it.

About a couple of years ago the Home Secretary determined to institute a special inquiry into the hygienic question involved in the use of lead compounds in pottery processes, with a view of ascertaining—

(1) How far the danger might be diminished or removed by substituting for the "raw" lead ordinarily used either a less soluble compound of lead or a "leadless" glaze.

(2) Whether such substitutes lent themselves to the varied practical requirements of the manufacturers.

(3) What other preventive measures could be adopted.

From the report, which was made in the spring of last year, and which is printed as a Parliamentary paper, it appears that the workers in all departments of the manufacture in which lead compounds are used, are liable to become, as it is called, "leaded." Among these operatives were the glaze-dippers and their assistants; those who clean or trim the dipped ware, and those who place it in the glost oven; majolica paintresses, and others, men and women, engaged in various ways in colouring and decorating the ware with pigments containing lead compounds.

Compounds of lead have been employed by the potter in one form or other from time immemorial: they are such valuable adjuncts that it is not to be supposed that he will lightly abandon their use. Indeed absolute prohibition of lead compounds in the pottery industry is hardly practicable, for there are certain branches of the manufacture in which it appears to be impossible to dispense with them. Moreover, no such prohibition is in force among Continental manufacturers who compete with our own producers, not only at home, but in almost every foreign market.

Lead poisoning among pottery manufacturers is not unknown on the Continent; but it is by no means so common as with us. This fact at once seems to show that it is rather the manner in which the lead compounds are used than their actual use which occasions the mischief. When we inquire what it is in Continental procedure that occasions this marked difference, we are at once struck with the fact that what is known as "raw" lead—that is, the white lead or red lead—is comparatively seldom employed on the Continent in the mixture in the glaze-tub. In the greater number of well-organised Continental factories the lead employed is mainly "fritted," that is, it is used in the form of a double silicate. The fact that "fritted" lead is, as a rule, more innocuous than "raw" lead is not unknown to the pottery world, and in an inquiry which was instituted by the Home Office in 1893, manufacturers whose names deservedly carry authority in the pottery districts strongly urged the substitution of "fritted" lead for "raw" lead in all glazes. Unfortunately, however, this recommendation was not enforced. This may have been due, partly at least, to the circumstance that cases of plumbism occurred from time to time in works where "fritted" lead was exclusively used. The fact is, there is "fritted" lead *and* "fritted" lead. And now I come to the first of the two main points of my lecture.

You are aware that the toxic action of lead depends upon its solubility in the system, especially by the aid of the gastric juice. I wish, therefore, to explain to you, as shortly as possible, the results of a recent inquiry into the conditions which determine the ease with which lead may be dissolved out from a "fritt" by dilute acids, such as are present in gastric juice. The conditions which determine its solubility may be said to determine its toxic action.

In the course of this inquiry I have, with the assistance of Messrs. Simmonds, More, and Fox, assistants in the Government laboratory, analysed a considerable number of Continental and English "fritts," and have determined the relative ease with which they may be attacked by a dilute acid comparable in strength with that existing in normal gastric juice. In the first place, I found that, speaking generally, such English "fritts" as I could obtain yielded a far larger amount of lead to solvents than those made in Holland, Belgium, Germany, or Sweden. Indeed, some English specimens of "fritted" lead were found to be hardly less soluble than "raw" lead. This may be seen from the following:—

TABLE *showing that from certain kinds of fritts and glazes, lead oxide is dissolved by 0·25 per cent. HCl to practically the same extent as from "raw" lead.*

		Lead oxide dissolved, expressed as percentage of total lead oxide present.
Lead silcate O_1		99·6
" " O_2		99·6
Glaze S, made with lead silicate		99·2
Glaze H, " "		99·6
Various forms of "raw" lead ..	Litharge	100·0
	Red lead	100·0
	White lead	100·0

Next the inquiry showed that there was no necessary relation between the amount of lead oxide in a "fritt" and the extent to which it would yield lead to solvents comparable as regards their action with animal secretions. Some of the compounds richest in lead were, in fact, among those least attacked by solvents.

TABLES *showing that the amount of lead oxide dissolved from a fritt by 0·25 per cent. HCl bears no direct relation to the amount of lead oxide in the fritt.*

I.—Solubility practically the same; amounts of lead oxide in the fritt very different.

——	Percentage of Lead oxide in fritt.	Solubility per cent. on fritt.
Maastricht fritt (preparation)	18·0	Traces
Fritt B 103a (preparation) ..	40·4	Traces
Boch's fritt	22·4	0·7
Fritt B 103	41·3	0·7
" B 2a (preparation) ..	52·3	0·4

II.—Solubilities very different; amounts of lead oxide in fritt practically the same.

——	Percentage of Lead oxide in fritt.	Solubility per cent. on fritt.
English fritt	37·9	28·0
Fritt M_1	36·2	1·4
Owen's fritt	45·8	10·8
Ålmström's fritt	44·1	2·1

TABLE *showing that in fritts high percentages of lead are compatible with relatively low solubility of the lead.*

—	Percentage of Lead oxide in fritt.	Solubility per cent. on fritt.
Fritt M_1	36·2	1·4
„ M_2	36·4	2·3
„ B 103a	40·4	0·2
„ B 102a	40·7	0·2
„ B 103	41·3	0·7
„ W_1	41·1	3·0
„ B 102	41·4	0·8
„ A	44·1	2·1
„ B 3a	47·5	0·2
„ B 3..	49·3	1·5
„ B 2a	52·3	0·4
„ B 2..	53·2	2·0
„ B 1..	59·3	5·1

Further inquiry elicited the fact that the extent to which the fritt gave up lead to the solvent depended upon two conditions—

(1) The existence of a definite numerical relation between the basic and acidic oxides in the fritt, and

(2) Complete chemical union.

The proof of the first fact is seen in the following table:

TABLE *showing dependence of amount of lead oxide dissolved from a fritt by 0·25 per cent. HCl upon the value of the ratio—*

$$\frac{\text{Basic oxides (as PbO)}}{\text{Acidic oxides (as SiO}_2\text{)}}$$

$$= \frac{\text{Sum of equivalent percentages of basic oxides (as PbO)}}{\text{Sum of equivalent percentages of acidic oxides (as SiO}_2\text{)}}$$

—	Percentage of Lead oxide.	Solubility per cent. of fritt.	Ratio.
Maastricht fritt (preparation)	18·0	traces	1·34
Boch's fritt (preparation)	21·8	traces	1·44
Maastricht fritt	19·0	1·2	1·50
Boch's fritt	22·4	0·7	1·52
Ålmström's fritt	44·1	2·1	1·56
Fritt B 101a	24·0	0·2	1·57
„ B 103a	40·4	0·2	1·68
„ B 101	24·5	0·6	1·70
„ B 3a	47·5	0·2	1·73
„ B 102a	40·7	0·2	1·77

—	Percentage of Lead oxide.	Solubility per cent. of fritt.	Ratio.
Fritt M_1	36·2	1·4	1·79
„ B 103	41·3	0·7	1·80
Owen's glaze fritt	16·2	1·7	1·82
Fritt B 2a	52·3	0·4	1·83
„ B 102	41·4	0·8	1·87
„ M_2	36·4	2·3	1·87
„ W_1	41·1	3·0	1·91
„ B_3	49·3	1·5	1·92
„ B_2	53·2	2·0	1·97
Owen's fritt	45·8	10·8	2·61
Fritt B_1	59·3	5·1	2·79
English fritt	37·9	28·0	2·92
Fritt O_2	70·4	67·3	3·26
„ O_1	71·2	70·0	3·80

It will be seen that, provided the ratio obtained by dividing the sum of equivalent percentages of acidic oxides, calculated as SiO_2 into the sum of equivalent percentages of basic oxides, calculated as PbO, does not exceed 2, the amount of lead dissolved, whilst in the main tending slightly to increase with the increase of the ratio, seldom exceeds 2 per cent., calculated on 100 parts of the fritt, and that the amount of lead dissolved bears no relation to the quantity of lead present.

Another fact which came to light was that, provided the ratio of acids to bases is below about 2, the nature of the basic oxides has little or no effect upon the amount of the lead oxide dissolved. This is seen from the following table:

TABLE *showing that the nature of the basic oxides has little or no effect on the amount of lead oxide dissolved from a fritt by* 0·25 *per cent. hydrochloric acid.*

—	Lead oxide.	Alumina.	Lime.	Alkalis.	Solubility per cent. on fritt.
Maastricht fritt	19·0	8·1	9·0	4·9	1·2
Owen's glaze fritt	16·2	10·3	8·5	9·2	1·7
Ålmström's fritt	44·1	5·5	0·9	3·4	2·1

The solubilities, it will be seen, differ but slightly, although the relative proportion of the basic oxides and their nature vary considerably.

TABLE *showing that the amount of lead oxide dissolved from a fritt by* 0·25 *per cent. hydrochloric acid does not depend simply upon the amount of silica in the fritt.*

I.—Solubility practically the same; amounts of silica very different.

—	Percentage of Silica in fritt.	Solubility per cent. on fritt.
Fritt B 101	48·4	0 6
Boch's fritt	52·9	0·7
Fritt B 102	42·3	0·8
Maastricht fritt	53·2	1·2
Fritt B 3	39·9	1·5

II.—Solubilities very different; amounts of silica approximating to equality.

—	Percentage of Silica in fritt.	Solubility per cent. on fritt.
Fritt B 3	39·9	1·5
English fritt	37·6	28·0
Fritt B 1	29·8	5·1
Fritt O_2	25·1	67·3

A remarkable illustration of this fact is to be seen in the case of flint glass, which is substantially a double silicate of lead and alkali. It is practically unacted upon by dilute acids, yet each of its proximate constituents—lead silicate and alkali silicate—is readily attacked.

The second consideration, which seems to affect the ease with which lead is yielded to solvents, is complete chemical union. Merely to flux together the ingredients of a fritt, with no regard to its composition as a definite chemical compound, and with no regard to the time or temperature needed to complete the chemical changes for the formation of chemical compounds, is not the proper way to make a fritt. The analogy of the practice of the glass workers may again be quoted in support of this fact.

But I think I can offer, in addition, direct chemical testimony from a study of the fritts themselves. I found, very early in the course of the inquiry, that the Continental fritts, which conformed to the ratio and were distinguished by their comparative insolubility, were very difficult to break up by the action of strong acids, and yielded only relatively minute portions of soluble matter, much of which, however,

consisted of lead, whereas the English fritts were for the most part very easily decomposed by the same treatment, and gave up the greater part of their lead to solution.

This led to the surmise that the Continental fritts consisted, for the most part, of comparatively stable chemical compounds, the minute quantity of lead dissolved being due to some lead compound —oxide or silicate—in a state of incomplete or unstable chemical union. Experiment showed that this surmise was correct. By treating a fritt, compounded so as to be within the limiting ratio, with acid, by far the greater proportion of the soluble or incompletely fixed lead may be removed, and an almost absolutely insoluble lead double silicate is left.

Preparations from Fritts.

The powdered fritt shaken for six hours with a solution of 0·25 per cent. hydrochloric acid ; insoluble residue (*a*) tested for solubility of lead.

—	Solubility.	Ratio.	Lead oxide in Fritt per cent.
No. 2 (original fritt)	2·0	1·97	53·20
No. 2a (preparation)	0·4	1·83	52·30
No. 3	1·5	1·92	49·31
No. 3a	0·2	1·73	47·46
No. 101	0·6	1·70	24·47
No. 101a	0·2	1·57	24·02
No. 102	0·8	1·87	41·38
No. 102a	0·2	1·77	40·66
No. 103	0·7	1·80	41·26
No. 103a	0·2	1·68	40·44

These results have so far strengthened the hands of the Home Secretary that he has felt justified in now requiring the potters to abandon the use of "raw" lead, and he has given them a definite time in which to make the change to "fritted" lead. But he has gone further than this.

He has also indicated to them that, after a further interval, a standard of safety must be definitely fixed by special rules, and that in order to allow them ample time in which to provide themselves with glazes answering to such a standard as is prescribed in a circular letter which he has caused to be sent to the whole trade, the Secretary of State proposes to allow an interval of two years before bringing it compulsorily into force.

I am gratified to be able to tell you that a number of manufacturers and professional fritt makers, acting in conformity with the suggestions which have been put forward, and in response to the invita-

tion of the Home Secretary to have their glazes tested in the Government laboratory, are now producing lead fritts, the solubility of which is even below the standard provisionally suggested in the Home Office circular.

I am not sufficiently sanguine, however, to suppose that the adoption of these measures will altogether stamp out plumbism in the potteries, even as regards glazes. We may possibly have scotched the evil, but I fear we have not absolutely killed it.

For it must be clearly understood that complete immunity from lead poisoning can never be obtained so long as lead compounds continue to be used.

The question may be asked—Are lead compounds actually necessary to the potter?

I unhesitatingly reply that as regards glazes they are not.

Leadless glazes of sufficient brilliancy, covering power, and durability, and adapted to all kinds of table, domestic, and sanitary ware, are now within the reach of the manufacturer.

Leadless glazes can be applied without difficulty to the large and varied class of ware which is known as "china furniture." They are equally applicable to white, cream, buff, and printed tiles. Insulators and electric fittings of the most varied kind can readily be coated with leadless glaze.

It was pointed out in the report to the Home Secretary, to which I have alluded, that much of the ware supplied to the order of various Government Departments, such as the Post Office, the Office of Works, the Admiralty, the War Office, the India Office, &c., could be dipped in leadless glaze, if so specified, without detriment to its character, and with no increase to its cost. The articles supplied to the various School Boards, the crockery and sanitary ware furnished to Poor-Law Unions, Asylums, and Hospitals, could also, if so specified, be coated with leadless glaze.

As the result of this representation, wares made with leadless glaze are now in actual use by some departments of the Government, and steps are being taken by other departments for their introduction.

Lord Reay informs me that the London School Board has resolved to insert a clause in all specifications for new works strictly prohibiting the use of any pottery goods involving lead glaze wherever practicable. I can only hope that this example will be widely followed.

There can be no doubt that if the public insisted on being supplied with leadless-glazed ware its demands would be met.

The fact that the use of leadless glazes has passed beyond the experimental stage is so obvious that the Secretary of State now proposes to relax the special rules issued by the Factory Department in regard to the pottery industry in the case of factories or processes in which no compounds of lead are used.

Any manufacturer who decides to abandon the use of lead in any

form will be released from the obligations imposed by certain rules, and only those will be enforced which are aimed at the prevention of danger from dust or at securing general cleanliness.

Leadless glazes have the disadvantage of being less fusible than those containing a relatively large quantity of oxide of lead, and hence require a higher temperature in the glost oven. But it may be doubted, even in this case, whether sufficient regard has been paid to the circumstance that mixtures of silicates melt at a lower temperature than the mean melting-point of their constituents. A properly compounded glaze does not melt as a whole to begin with; one portion only softens in the outset, and gradually acts as a flux towards the rest. Much, too, depends upon the fineness to which the glazing material has been ground.

Every intelligent potter will concede that there is an ample field for investigation, by modern methods of attack, on problems connected even with the first principles of his art, and that it is not unlikely such investigations would lead to far-reaching results in practice.

I see little prospect at present that such investigations will be made. The schoolmaster may be abroad among the potters, but the science master, I am afraid, is not.

Such laboratories as are to be met with in association with works like the Aluminia factory at Copenhagen, or that of Villeroy and Boch at Dresden, are altogether unknown or undreamt-of in Staffordshire.

There is probably no industry in the world—certainly none in England—which is so conservative in its operations as that of the potter. It is true that the best of English earthenware still enjoys, by common consent, the pre-eminence which the skill and aptitude of Wedgwood and his immediate followers imparted to it. The great potter was fully abreast—as, indeed, his letters to Priestley abundantly show—of the physical science of his day, and was quick to test or take advantage of any discovery which seemed to promise to be of service to his art. But, whilst his methods, or some of them, may still be used, it is, perhaps, open to doubt whether the spirit of Wedgwood has altogether descended to his successors, for there can be no question that the exercise of his spirit—that is, the intelligent application of simple chemical principles—would, years ago, have obviated, to a large extent at least, this evil of plumbism among the pottery workers.

At the conclusion of the lecture Professor Thorpe exhibited numerous specimens of leadless-glazed ware, which, through the kindness of several well-known manufacturers, who have adopted the process, had been forwarded to him for the purpose of illustrating his address.

[T. E. T.]

GENERAL MONTHLY MEETING,

Monday, May 7th, 1900.

His Grace The Duke of Northumberland, K.G. President, in the Chair.

Mrs. Elizabeth Sarah Beale,
Ernest Callard, Esq.
Edward J. Duveen, Esq.
Ernest Pearson, Esq.
Lord Russell of Killowen, G.C.M.G. LL.D.
Jules Alphonse Thierry, Esq.

were elected Members of the Royal Institution.

The special thanks of the Members were returned to Professor F. Clowes for his donation of £20 to the Fund for the Promotion of Experimental Research at Low Temperatures.

His Grace the President announced that he had nominated the following Vice-Presidents for the ensuing year: Sir F. Bramwell, The Right Hon. Lord Lister, Dr. Ludwig Mond, Sir A. Noble, Mr. A. Siemens, Hon. Sir J. Stirling, Sir J. Crichton-Browne, *Treasurer*, and Sir W. Crookes, *Hon. Secretary*.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

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Education for April, 1900.
Electrical Engineer for April, 1900. fol.
Electrical Review for April, 1900. 8vo.
Electricity for April, 1900. 8vo.
Engineer for April, 1900. fol.
Engineering for April, 1900. fol.
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WEEKLY EVENING MEETING,

Friday, May 11, 1900.

The Hon. Sir James Stirling, M.A. LL.D., Manager and Vice-President, in the Chair.

Sidney Lee, Esq.

Shakespeare and True Patriotism.

Mr. Lee said the suggestion had been made that an English national fête day should be instituted, but the champions of the proposed festival had not yet perfected their programme. Some urged that a fitting date would be April 23, the day consecrated in the calendars of the Roman and Greek churches to St. George, who was popularly reckoned the tutelary saint of England. St. George had been identified with two shadowy figures in history. The single fact about them that was uncontradicted was that neither of them was an Englishman or had any connection with England. They were both natives of Cappadocia, in Asia Minor, some 1500 years ago. Gibbon, perhaps, wrongly identified the tutelary saint with a disreputable St. George of Cappadocia—" an odious stranger," Gibbon called him—who, having exhausted the varied possibilities of the careers of army contractor and tax-gatherer, ventured on the experiment of becoming an Archbishop, but his flock marked their resentment at this change of profession by tearing him limb from limb. The reputed encounter with the dragon was mythical, and it seemed eccentric to associate with cobwebs of romantic myth the annual celebration of the greatness of a nation which prided itself on its practical common sense and love of solid fact. It was a happy coincidence that had identified St. George's day with the birth of a hero by no means fabulous, William Shakespeare. If at the beginning of a new century a patron saint was chosen anew, and the choice lay between a mythical native of Cappadocia and Shakespeare, the native of Stratford-on-Avon, the straitest of cosmopolitan intellects among us could hardly defy the sentiment that gave the preference to the Englishman. The cosmopolitan might argue that Shakespeare was the property of the world. The Germans treated Shakespeare as one of themselves, and the only complaint that they had been known of late years to make of him was that he had the bad taste to be born an Englishman. In France, too, the elder Dumas gave pointed expression to his faith in Shakespeare's pre-eminence in the pantheon not of a single nation, but of the universe.

Dumas set Shakespeare next to God in the cosmic system, saying "After God, Shakespeare has created most." If assertion of the fact that Shakespeare was an Englishman were a weakness of English flesh, it was a weakness beneficial to the Englishman's mental health to yield to. Personally, Shakespeare had homely ambitions. Bacon bequeathed his name and memory to foreign nations. Shakespeare made no testamentary disposition of his name and memory, and by his default, his name and memory were the heritage of the English-speaking race, his next of kin. Patriotism manifested itself nowhere more safely or more sanely than in the due recognition of those heroes of a nation's past, whose achievements had helped to confer on it its title to the respect of other nations. A large part of this nation's prestige—its intellectual prestige—was due to its kinship with Shakespeare. Thirty-five years ago, Cardinal Wiseman had promised to deliver a Friday evening discourse on Shakespeare at the Royal Institution. His death interrupted the design, but the notes prepared for the lecture survived. There the Cardinal pointed out how Shakespeare and Sir Isaac Newton had given England the sovereignty respectively of literature and science in the civilised world. Without Newton and Shakespeare Englishmen would lack much of the consideration they now enjoyed in the sight of the world.

Coming more closely to his special subject, Mr. Lee said the Shakespearian drama illustrated how the patriotic instinct might be virtuously trained, and how the morbid symptoms incident to its excess or defect might be averted. The play of 'Coriolanus' proved how the presence of patriotic instinct in some form or other was needed to the proper conduct of life. Bolingbroke in 'Richard II.' showed how a sincere love of one's country softened harshness of character and purified ambition. Henry V. deprecated vaunts, in the name of patriotism, of the superiority of the English over the French. His patriotism, though stiffened by war, was not unmindful of the interests of peace. The warlike play of 'Henry V.' ended with a powerful appeal to France and England to cherish "neighbourhood and Christianlike accord." The true patriot was encouraged by Shakespeare to speak out boldly when he thought his country erred. Shakespeare exposed freely and with good-humoured cynicism the failings and errors of his countrymen. Extravagance of dress, the contemptuous ignorance of foreign languages, addiction to excess, as in drink, the nation's patronage of undignified shows and sports, the want of balance that commonly infects the popular judgment, all came under Shakespeare's condemnation. The historical plays of Shakespeare illustrated the brittleness rather than the brilliance of kingly glory, and inferentially of national glory. The glory of a nation, as of a king, was only stable when the nation, as the king, lived soberly, virtuously, and wisely, and was courageous, magnanimous, and ambitious of knowledge. In the most eloquent of all the direct avowals of patriotism in Shakespeare's plays, in

the great dying speech of John of Gaunt, we were warned that all the greatness and glory with which nature and history had endowed England ran a risk of dissipation, if her rulers proved selfish and frivolous, and unequal to the responsibilities that a great past laid on their shoulders.

In two ways we seemed to be neglecting opportunities of doing outwardly and visibly our Shakespearian duty. It became us to encourage the study of Shakespeare's works, to bring his work prominently to the notice of all. It could not be brought more prominently to the notice of the nation at large than on the stages of our theatres. Yet less was now done for the interpretation of Shakespeare's work in our theatres than was done by former generations in England, or than was being done at the moment in the theatres of France and Germany. One of the causes of the practical suppression of Shakespeare on the London stage was due to the current fallacy, that the Shakespearian drama required spectacular magnificence which involved managers in expenses not easily borne. The interpretation of Shakespeare must chiefly depend on the acting. The relations between the Shakespearian and the modern drama were something like those subsisting between Westminster Abbey and a "desirable" suburban villa. The scheme of decoration that fitted the one was ridiculously out of place in the other. To establish a Shakespearian theatre in London where the Shakespearian drama should be represented constantly and in its variety, with efficient actors, and with the scenery subordinated to the dramatic interest, would be a very welcome and profitable manifestation of true patriotism. We failed, too, in our filial piety to Shakespeare in indifferently permitting so many of the original editions of his works to leave our shores. Valuable Shakespeariana now passed almost automatically, when offered for sale here, to our cousins in America. These Shakespearian volumes should be treated as national heirlooms, and heirlooms only passed to cousins when the direct line was extinct. With regard to the First Folio, of which very few, less than twenty, quite perfect copies were known, it was greatly to be wished that, when copies henceforth came into the market, they might be acquired, through the private munificence of true patriots, by one or other of the great public libraries in the great centres of population.

[S. L.]

WEEKLY EVENING MEETING,

Friday, May 18, 1900.

Ludwig Mond, Esq., Ph.D. F.R.S., Vice-President, in the Chair.

Professor J. A. Ewing, M.A. F.R.S. M. Inst. C.E.

The Structure of Metals.

Much information has been obtained regarding the structure of metals by the methods of microscopic examination initiated by Sorby, and successfully pursued by Andrews, Arnold, Martens, Osmond, Roberts-Austen, Stead and others. When a highly polished surface of metal is lightly etched and examined under the microscope, it reveals a structure which shows that the metal is made up, in general, of irregularly shaped grains with well defined bounding surfaces. The exposed face of each grain has been found to consist of a multitude of crystal facets with a definite orientation. Seen under oblique illumination, these facets exhibit themselves by reflecting light in a uniform manner over each single grain, but in very various manners over different grains, and when the angle of incidence of the light is changed one or another grain is seen to flash out with a uniform brightness over its whole surface, while other grains which were bright before become dark.

These grains are deformed when the metal is severely strained in any way, such as by stretching in the testing machine, or cold-rolling, or wire-drawing. On polishing and etching a strained piece, the grains are found to be on the whole longer in the direction in which the metal has been extended. But when the piece is sufficiently heated, a reformation of structure occurs, and the grains are found to have assumed forms in which there is no direction of predominating length. This process of recrystallisation is what gives rise to the change in mechanical quality associated with annealing.

The grains are apparently produced in the first instance, while the metal is solidifying from a liquid state, by crystallisation proceeding more or less simultaneously from many different centres or nuclei. The irregular boundaries between one grain and another are due to the casual meeting of these various crystal growths. Each grain is, in fact, a crystal, with all its elementary parts oriented in one way, but its boundaries give no indication of its crystalline character. When polished and etched the true crystalline character of each grain is demonstrated in several ways, notably by the

uniform brightness of the grain under oblique light, the sudden variations of brightness which occur as the direction of the incident light is changed, and the evidence which high power microscopic examination gives of geometrical forms in the texture of the grain. In many cases the etching produces pits here and there on the grain, and in each grain these pits have one and the same orientation. Numerous lantern slides of micro-photographs were exhibited in illustration of these points, and the experiment was shown of directly projecting on the screen the light reflected from grains of a piece of iron, prepared by Mr. Stead and lent to the lecturer by Sir W. Roberts-Austen, in which the granular structure was exceptionally large.

The lecturer proceeded to give an account of recent researches made by him in conjunction with Mr. W. Rosenhain, in which the effects of straining and the subsequent influence of temperature in causing partial annealing were particularly examined.

By watching the polished surface of the metal during straining, it was seen that as soon as the elastic limit was passed and the metal began to take "permanent set," a large number of lines appeared on the surface of each grain. Seen under vertical illuminations, these were black, and looked like crevasses, but oblique illumination showed them to be really steps or abrupt changes of level. It was shown that these were produced by sudden slips occurring on cleavage or "gliding" planes in the crystal. Plastic yielding on the part of the metal took place by means of these slips, which in cases of severe strain were seen to occur on two, three, four, or perhaps more sets of independent planes. Micro-photographs were exhibited, showing the slips in iron, lead, gold, copper and other metals. In many cases, severe straining was also found to develop twin crystals.

Twin crystals were also a usual characteristic of metal, which, after being severely strained in the process of manufacture, had been more or less annealed. This was readily seen by examining rolled copper after it had been softened by heat. But perhaps the most striking instance was to be found in sheet lead. Ordinary plumbers' lead usually showed a very large crystalline structure, with many brilliant examples of twin crystals. This led the lecturer and his colleague to suspect that prolonged exposure to atmospheric temperature was sufficient to cause crystalline growth to occur in lead, and they succeeded in verifying this supposition. The growth which goes on after severe straining is a function of the time as well as of the temperature to which the metal is exposed. At comparatively high temperature it occurs fast, and the metal quickly reaches an apparently stable state. At lower temperature, it goes on more slowly, and its progress may be traced for weeks or months.

Photographs were exhibited illustrating the gradual process of re-crystallisation in pieces of lead which were severely strained by compression, and were then kept under observation at various constant temperatures, one of which was the ordinary temperature of a room.

It was shown that certain crystals, more aggressive than their neighbours, grew by absorbing other crystals, the process of growth usually taking place by a crystal's throwing out skeleton arms to form a network, the detail of which became filled up later as the process of growth continued.

Professor Ewing concluded by stating a theory which Mr. Rosenhain had advanced to explain the process of crystalline growth in a solid metal. According to this, the action occurs through solution of the metal into and deposit from the film of eutectic alloy, which forms a cement between one crystal and another. This eutectic is due to the presence of impurities. It appears probable that the action is electrolytic, for it is only after straining has broken the films of eutectic, and has brought the crystals into contact, that the process of growth occurs. This theory received much support from what was observed to happen in the case of welds. Two freshly scraped surfaces of lead could readily be welded cold, by application of severe pressure. When subsequently heated the weld showed no crystal growth across it. But when a few fragments of some metallic impurity, capable of forming a eutectic with the lead, were introduced before the weld was made, subsequent heating was found to make crystals grow readily across the plane of the weld. This confirmed the view that a film of eutectic alloy was an essential intermediary in the process by which one crystal grew at the expense of another.

[J. A. E.]

WEEKLY EVENING MEETING,

Friday, May 25, 1900.

His Grace the Duke of Northumberland, K.G. F.S.A., President, in the Chair.

Francis Fox, Esq., M. Inst. C.E. *M.R.I.*

The Great Alpine Tunnels.

The subject for this evening's discourse is that of the three great tunnels through the Alps—viz., the Mont Cenis, the St. Gothard, and that which is now in course of construction, the Simplon.

But before dealing with the details of these particular works, it will be desirable to consider what tunnelling is, and also some of the more remarkable instances of it in bygone days.

One great drawback in connection with the subject—so far as a discourse is concerned—is its unsuitability for the photographic art. Unlike a battleship, or a splendid bridge, or a grand block of buildings, which can be made into fine views and pictures, the work of the mole is hardly adapted to the sensitive plate. I therefore propose to make use of the "language of the pencil," and to make a few rough sketches on the blackboard: by these means I trust I may be able to explain some of the difficulties which have to be encountered, and also show how a tunnel is constructed. The child's definition of drawing, "first you think, and then you draw a line round your think," will come to our aid.

The art of tunnelling dates back to very remote ages, and there are records of such works which were constructed 500 to 600 years before the Christian era.

An interesting account is given by one of your most distinguished members, in an article in the 'Encyclopædia Britannica,' of the tunnel under the river Euphrates at Babylon. This city, similar in some respects to London, lay half on one side and half on the other side of the river. High walls, penetrated by occasional gates, surrounded the city, and lined each of the banks of the river. These gates (of which a pair of the great hinges can be seen in the British Museum) were closed at night and during war; and a tunnel was constructed below the bed of the river by means of what is technically known as the "cut-and-cover" system. In those days the Greathead shield was unknown, and consequently the river had to be diverted, so that the excavation could be made in the dry bed and

cut open to daylight, the arch being built, the ground restored, and the river allowed to resume its former course. The tunnel is said to have been 15 feet in width, and 12 feet in height, built of brick.

Herodotus gives an account of the diversion of the river into a great excavation or artificial lake 40 miles square, and states that the besieging enemy, so soon as the water was drawn off, entered into the city by the river bed. It is believed that this same excavation was made use of for the construction of the tunnel. It is, however, desirable to state that doubts have been thrown on the subject, and it is possible that it may have to be relegated to mythology.

The next instance of a tunnel is that referred to by Herodotus in the Island of Samos,* and it is satisfactory to know that although very considerable doubts were expressed as to the accuracy of his statements, recent investigations prove that he was exactly correct. The description given by him, when expressed in English words and figures, is as follows : " They have a mountain which is 910 feet in height; entirely through this they have made a passage, the length of which is 1416 yards. It is, moreover, 8 feet high, and as many wide. By the side of this there is also an artificial canal, which in like manner goes quite through the mountain; and though only 3 feet in breadth, is 30 feet deep. This, by the means of pipes, conveys to the city the waters of a copious spring."

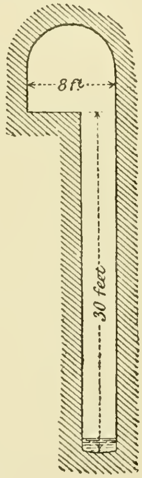


Fig. 1.—Cross Section of the Aqueduct of Eupalinos, in the Island of Samos.

The commentators on this passage say that Herodotus must have made a mistake, but the Rev. H. F. Tozer, in his book 'The Islands of the Ægean,' page 167, gives the results of a personal visit.

He says the tunnel is 7 to 8 feet in width; that two-thirds of its width is occupied by a footpath, the other third being a water-course, 30 feet deep at one end. He and other writers consider that insufficient allowance was made for the fall of the water, and that the water channel had to be deepened. To describe it in more modern language, the resident engineer evidently made a mistake in his levels, necessitating a much deeper excavation than was at first anticipated.

Another, and, if possible a more interesting, instance of tunnelling is that described in the 'Proceedings of the Palestine Exploration Society,' in connection with the Pool of Siloam, made by Hezekiah, B.C. 710, 2 Kings, xx. 20.† (See Fig. 2.)

About 710 B.C. a tunnel was driven from the spring to the well—

* Herodotus, iii. p. 60. † 'Palestine Exploration,' 1882, p. 178.

by actual tunnelling—the work being commenced at the two ends, and by shafts, and the workmen met in the middle. The tunnel was only 2 feet in width, and 3 feet in height, except at the probable point of meeting, where the height is 4 feet 6 inches. The length is 1708 feet, and there is a fall of 1 foot in this distance. About the middle of its course there are apparently two false cuts, as if a wrong direction had been taken: but possibly these were intentional, and provided passing places for the workmen and material.

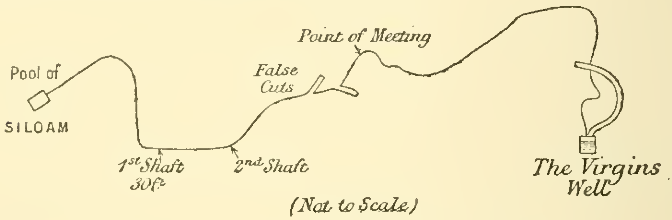


Fig. 2.—Plan of Tunnel from Spring to Pool of Siloam.

On the soffit of the tunnel is carved an inscription, of which the following is a translation :—

"Behold the excavation. Now this had been the history of the excavation. While the workmen were still lifting up the pick, each towards his neighbour, and while 3 cubits (4 feet 6 inches) still remained to cut through, each heard the voice of the other, who called to his neighbour, since there was an excess of rock on the right hand and on the left. And on the day of the excavation the workmen struck each to meet his neighbour pick against pick, and there flowed the waters from the spring to the pool for a thousand two hundred cubits (1820 feet), and a hundred cubits (151 feet) was the height of the rock over the head of the workmen."

A Roman engineer gives an account of a tunnel which was being driven under his directions for an aqueduct. And as he was only able to visit the work occasionally, he describes how on one of his visits he found the two headings had missed each other, and he says that had his visit been deferred much longer there would have been two tunnels.

The accurate meeting of the headings or driftways of a tunnel can only be attained by the exercise of great care, both as regards direction as well as level.

We need not go very far to find instances of such an error as inaccurate meeting, but there is one well-known case on an important main line in the Midland Counties where the engineers failed to meet, and to this day reverse curves exist in the tunnel to overcome the difficulty.

To attain this accurate meeting fine wires are hung down the shafts of a tunnel, with heavy plumb-bobs suspended from them in

buckets of water, or of tar, to bring their oscillations to rest; the accurate direction being given by means of a theodolite or transit instrument on the surface.

The wires are capable of side movement by means of a delicate instrument [which is on the table], and are gradually brought exactly into the same vertical plane: hence, if they are correct at "bank," or surface, they must also be correct below ground. The engineers below have to drive the galleries or headings so that only *one* wire is visible from their instrument: so long as one wire exactly eclipses the other wire, the gallery is being driven in the right direction.

As regards accuracy in levels, this is done by ordinary levelling; but it will be seen at once how much depends on care being devoted to both these operations.

Assume two shafts, 1000 yards apart, between which a gallery has to be driven; and, allowing a distance of 10 feet between the wires, which are $\frac{1}{40}$th inch in diameter, an error of the diameter of the wire at the shaft will cause a mistake of nearly 4 inches at the point of meeting, or of $7\frac{1}{2}$ inches if a similar error occurs at the other shaft in the opposite direction. The trickling of water down the wires increases their diameter so appreciably, and therefore conduces to further inaccuracy, that it is found necessary to fix a small shield or umbrella on the wire to deflect the water. [This shield is to be seen on the table.]

Some years ago, a tunnel which had been commenced, but not finished, had to be completed. The first thing to be done by the engineers was to make an accurate survey of the then condition of the work—this rough sketch (see Fig. 3) indicates what was discovered. The explanation given by the former "ganger" was, that he found the rock too hard, and he thought that by bearing round somewhat to the right, he might get into more easily excavated material!

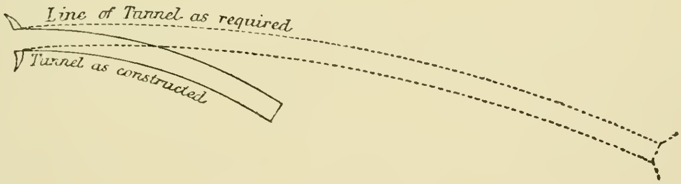


FIG. 3.—Plan.

When the wires are hung down the shaft it is sometimes almost impossible to prove that they are not touching, and consequently being deflected from the true vertical line by some rope or pipe, staging or timber in the shaft. To overcome this, an electrical current was passed down the wire—a galvanometer being in circuit. If the wire proved absolutely silent, and no deflection was obtained

in the galvanometer, the conclusion could be safely drawn that the wire was hanging freely and truly.

In driving the necessary adit or heading for drainage purposes beneath a sub-aqueous tunnel, a rising gradient from the shaft bottom of 1 in 500 is allowed, to enable the water at the "face" to flow away from the workmen to the pumps in the "sump" or shaft bottom (see Fig. 4).

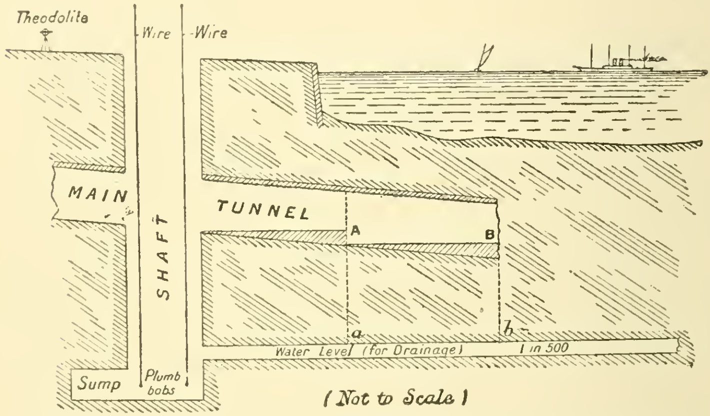


FIG. 4.—Diagrammatic Section to illustrate Method of Constructing Tunnel below River Bed.

When the heading is driven sufficiently forward to justify the commencement of the main tunnel, a fresh difficulty presents itself. This main tunnel has to be driven down hill, and consequently the water collects at the working face A: the bottom cannot therefore be removed until a bore-hole is put down from A to *a*. When this is done the remaining excavation can be taken out, and a further length of tunnel driven to B. A bore-hole is now sunk from B to *b*, whilst that from A to *a* can be plugged up: and thus the tunnel is gradually advanced.

By the adoption of the Greathead shield much of this difficulty can be avoided; but one sub-aqueous tunnel through water-bearing strata, at considerable depth, is sufficient for a lifetime.

As an illustration of the danger to which men are exposed in such work, it is stated, with much regret, that in a certain tunnel, notwithstanding every precaution being taken, all the men engaged in driving the drainage heading by means of a tunnelling machine have died; and in the case of the first Vyrnwy tunnel crossing of the river Mersey—driving by Greathead shield under pressure—the mortality was great.

Having explained in very general terms some of the difficulties of tunnel construction, we will proceed to the case of the great tunnels through the Alps, and for the purpose of rendering the subject more easily intelligible, the following particulars may be given:—

	St. Gothard.	Mont Cenis.	Arlberg.	Simplon.
Length of tunnel in miles ..	9·3	7·98	6·36	12·26
North or east portal above sea-level, feet	3639	3766	4296	2254
South or west portal above sea-level, feet	3757	4164	3998	2080
Highest level	3788	4248	4300	2314
Maximum grade in tunnel per 1000	5·82	30	15	7
Maximum height of mountain above tunnel, feet	5598	5428	2362	7005
Possible maximum temperature of rock, deg. Fahr.	85°	85°	65°	104°

Mont Cenis Tunnel.

The Mont Cenis, or as it is more accurately called, the Frejus Tunnel, is nearly 8 miles in length. It is for a double line of way—width being 26 feet, and height above rails 20 feet 6 inches. The construction is of excellent character, and it is lined throughout with either masonry or brickwork, except for two lengths of 100 metres and 70 metres respectively. In these two lengths solid white quartz was encountered, and two years were occupied in penetrating it. The gallery of direction is straight throughout the actual tunnel, being curved away to the portals.

The system of setting out will be described in more detail when we come to consider the case of the Simplon, but in passing we may remark one peculiarity which does not attach to the other tunnels, viz. that the gallery of direction on the Italian side is shut off by a massive grating from the railway tunnel, and is occupied by guns and Gatlings and by a detachment of artillery, the French portal being commanded by an armour-plated fort.

The approaches to the tunnel, both on the Italian and French sides, are severe, amounting to 30 per thousand or 1 in 33 on the former, and 25 per thousand or 1 in 40 on the latter.

Owing to an alteration during construction on the Bardonnechia side, it became necessary to introduce an ascending gradient for about 1 kilometre in length at the Italian end of the tunnel, and this has resulted in seriously compromising the ventilation.

A rough diagram will serve to give an idea of the gradients and the consequent difficulty in working the traffic.

Trains coming from France with an ascending gradient of 1 in

40 against them for a length of 7 kilometres, when followed by a current of air in the same direction, produce a most disastrous state of things. In this, as in all other steep tunnels, engines having a heavy load behind them, go through with their regulator full open, ejecting great volumes of smoke and steam which travel concurrently with the train, and the inconvenience and discomfort produced are very great.

At each kilometre in the tunnel a refuge or "grande chambre," is provided for the men, and this is supplied with compressed air, fresh water, a telephone in each direction out, a medicine chest, barometer, and thermometer.

The custodians of the tunnel go in pairs, and if one man is affected by the want of oxygen or dense smoke, the other can render assistance or telephone for further help. The men can retire into these chambers, close the door, turn on the air, and wait either for the tunnel to clear or for a locomotive to fetch them out.

The temperature in the middle of the tunnel remains nearly constant, summer and winter, and is about 19° to 20° C. = 66° to 68° Fahr.

The altitude of the tunnel is 4248 feet above sea-level, and the height of the mountain above the tunnel is 5428 feet: the temperature of the rock is greatly influenced by this latter fact.

The question of the temperature of the rocks passed through in the construction of a tunnel is one of great interest, as it depends upon several conditions: (1) the character of the rock; (2) the inclination of the beds—those which attain a vertical or nearly vertical position being less able to confine the heat than those which are more or less horizontal; (3) the height of the mountain above the tunnel, or in other words, the thickness of the blanket.

A diagram is shown (see Fig. 5), giving the temperature actually encountered in the St. Gothard and Arlberg Tunnels, and from these, aided by the carefully prepared geological section along the centre line of the Simplon Tunnel, an approximate line (in red) is given of the temperatures which are expected.

The possibility of cooling the rocks and the air of the tunnel will be dealt with later on, but there is in addition a permanent lowering of the temperature after the tunnel is complete, particulars of which will be given under the description of the St. Gothard.

For each 144 feet of superincumbent rock or earth the increase is found to be 1° Fahr.

The St. Gothard Tunnel.

This, which is at present the longest railway tunnel in the world, is 9·3 miles in length, and constitutes the summit of the "Gothard bahn," that is, the railway which runs from Lucerne to Chiasso on the Italian frontier. There are about 100 tunnels in all, most of

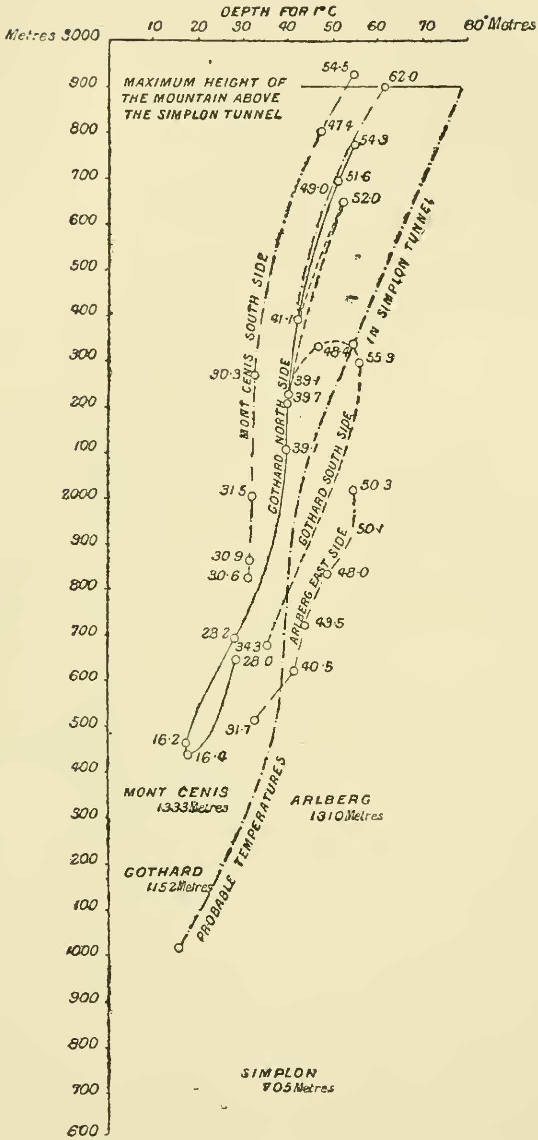


FIG. 5.—Curves showing Depths corresponding to an Increase in Temperature of 1° C. for the Mont Cenis, Gothard, and Arlberg Tunnels (with curve of probable temperature for the Simplon Tunnel).

which are for double line of way, the permanent way being very heavy, the rails weighing 100 lbs. to the yard.

The altitude of the tunnel at its north portal is 3639 feet, and at its south portal 3757 feet above the sea. A gallery of direction was driven throughout, and the gradient of the rails is only such as to provide for efficient drainage, viz. 5·82 per thousand, or about 1 in 172.

The following table may be of interest, giving the result of investigations as to the cooling of the rocks.

TEMPERATURE OF THE ROCK IN THE ST. GOTHARD TUNNEL.

Date.	7·3 kilo. from the North Portal.			7·05 kilo. from the South Portal.		
	Temperature.	Lowering.		Temperature.	Lowering.	
		Successive.	Total.		Successive.	Total.
April and May, 1880, the year when the tunnel was pierced	$30\overset{\circ}{\cdot}46$	$\overset{\circ}{..}$	$\overset{\circ}{..}$	$30\overset{\circ}{\cdot}53$	$\overset{\circ}{..}$	$\overset{\circ}{..}$
June, 1882	23·73	6·73	..	23·39	7·14	..
July, 1883	22·20	1·53	8·26	23·1	0·29	7·43

Above are Centigrade.

Although the works were carried on with energy, and with all the best appliances then known, the time occupied was ten years; but the most serious feature of the work was the heavy mortality amongst the men: no less than 600 deaths occurred, including those of both the Engineer and Contractor.

From the experience then gained, great improvements have been introduced into the works of the Simplon, as will be described later on; but the heavy loss of life in the St. Gothard was due to insufficient ventilation; the high temperature; the exposure of the men to the Alpine climate after emerging from the tunnel; the want of care as to the changing of the men's wet mining clothes; and the poor character of the food with which the men supplied themselves. All this has been greatly ameliorated, and even in English tunnels certain improvements have been introduced, which were brought from Switzerland.

The traffic through the tunnel has so largely increased that the question of ventilation became of pressing importance, and the system of Signor Saccardo, the well known Government Inspector of Railways and Engineer of Bologna, has been installed, which is an ingenious application of the Injector system. One of the first introductions of this method was in the case of the Pracchia tunnel, on

the main line between Florence and Bologna, through the Apennines. This is a railway of single line, and was built many years ago by the late Mr. Brassey. There are 52 tunnels in all, but those on the eastern side are of comparatively little importance. On the western slope the gradient nearly throughout is 25 per thousand (or 1 in 40), and it is here the greatest difficulty exists. There are several tunnels whose lengths approximate to 1000, 2000, and 3000 yards, and the traffic is both heavy and frequent, the locomotives very powerful, with eight wheels coupled.

Under any conditions of wind the state of the longest tunnel is bad, but when the wind is blowing in at the lower end at the same time that a heavy goods or passenger train is ascending the gradient, a state of affairs is produced which is almost insupportable, and one might as conveniently travel in a furnace flue.

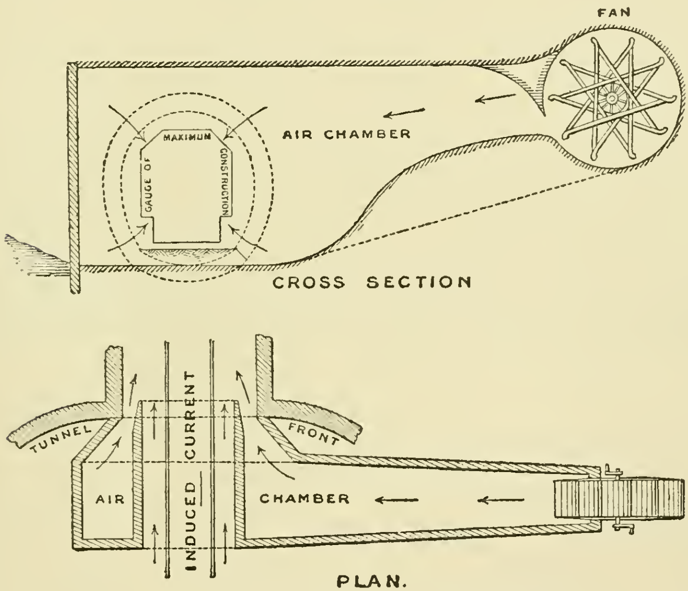


Fig. 6.—The Saccardo System of Ventilating Tunnels.

A heavy train of dining and sleeping carriages, with two engines, conveying one of the crowned heads of Europe and suite, arrived at the exit of Pracchia tunnel with both engine-men and both firemen insensible: and in other cases passengers have been seriously affected.

Owing to the height of the mountain no shafts are available; but Signor Saccardo places a ventilating fan near the mouth of the tunnel, and blows air into it through the annular space which exists

between the arch of the tunnel, and the gauge of maximum construction (see Fig. 6). The results are remarkable; the volumes of air thrown into the tunnel per minute being as follows:—

	cub. ft.
Direct from the fan	161,860
Induced draught through open tunnel mouth	48,140
Total	210,000

Or 100 cubic metres per second.

The temperature of the tunnel air before the fan was started was 107° F., with 97 per cent. of moisture, whereas, after the fan had been running a few minutes the temperature was 81° F., or a lowering of 26° F., and the tunnel was cool and free from smoke and vapour.

One can travel through with both windows open and feel no inconvenience, the only remark of the brakesman riding on the top of the wagons and carriages being that he finds it almost too cold.

This application is without doubt the solution of the difficult problem of tunnel ventilation under high mountains, and elsewhere where shafts are not available, and where electric traction is not applicable.

This system has within the last twelve months been brought into operation on the St. Gothard, with the most satisfactory results. Careful experiments are being made, but there is no doubt that the problem has been solved.

In addition to these tunnels, the Saccardo system has been applied to the Giovi Tunnel near Genoa—3300 metres in length—and is being installed on the Giovi Tunnel on the Genoa-Ronco Railway, 8303 metres in length, besides on some seven other tunnels in Italy; and plans are being prepared for the Mont Cenis.

The Simplon Tunnel.

This tunnel is now in rapid course of construction, the total length of gallery driven up to end of April being as follows:—

	yards.
On the north or Brigue side of the Alps	3228
On the south or Iselle „ „	2350

Or over three miles in little more than 18 months, including the necessarily slow progress at the commencement.

The total distance between the two portals will be 21,564 yards, or 12·26 miles. A gallery of direction has been driven at both ends until the actual tunnels are reached, so as to form a directly straight line for the accurate alignment of the work, from end to end.

This great undertaking will consist of two single-line tunnels running parallel one to the other, at a distance apart from centre

to centre of 55 feet 9 inches; and one of the chief features is the much lower altitude of the rails above sea level than any of the other Alpine tunnels. This altitude is at its highest point 2314 feet, being 1474 feet lower level than that of the St. Gothard, 1934 feet lower than that of the Mont Cenis, and 1986 feet than that of the Arlberg. This is a matter of great importance in the question of haulage of all the traffic.

The tunnel enters the mountain at the present level of the railway at Brigue, so that no costly approaches are requisite on this side; but on the Iselle side, the connecting line with the existing railway at Domo d'Ossola necessitates heavy work with one helical tunnel. The gradient on the northern portion of the tunnel will only be that sufficient for drainage, viz. 1 in 500, but on the southern portion the gradient will be 7 per 1000, or 1 in 142.

Admirable arrangements have been made for the welfare of the men, to avoid the heavy death-rate which occurred on the St. Gothard, and it may be interesting to state what some of these are. For every cubic foot of air sent into the latter tunnel, fifty times as much will be delivered into the Simplon. Special arrangements are made for cooling the air by means of fine jets of water and spray.

The men on emerging from their work, wet through and fatigued, are not allowed to go from the warm headings into the cold Alpine air outside, but pass into a large building which is suitably warmed, and where they change their mining clothes and are provided with hot and cold douche baths. They put on warm dry clothes, and can obtain excellent food at a moderate cost before returning to their homes. Their wet and dirty mining clothes are taken charge of by appointed custodians, who dry and clean them ready for the morrow's work. These and other precautions are expected to reduce the death-rate to a very great extent.

With a view to the rapid advancement of the work, the late M. Brandt, whose death is greatly to be deplored, devised after his long experience on the St. Gothard, his now well-known drill. As details of this have been published, and as they would be too technical for this evening's discourse, it will only be necessary to refer to them briefly. This drill is non-percussive, nor is it armed with diamond. It is a rotatory drill 3 inches in diameter with a pressure on the cutting points of 10 tons moving at slow speed, but capable of being accelerated at pleasure, and of being rapidly withdrawn. It is armed with a steel tool with 3 cutters, of which samples are on the table. The carriage on which it is mounted enables it to work in any direction. The face of the tunnel is attacked by 10 to 12 holes in the case of the hardest rock, those in the centre being 3 feet 3 inches in depth, whilst those round the circumference are 4 feet 7 inches. The drills are driven by hydraulic pressure of 100 atmospheres or 1470 lbs. to the inch, and the cutter having a $\frac{3}{4}$-inch hole along its centre, all the waste water is discharged right on to the cutting edges, thus keeping them cool, and washing out the débris.

The time taken for each portion of the attack in the hard Antigorio gneiss, is as follows:—

Bringing up and adjustment of drills	20 minutes.
Drilling	$1\frac{3}{4}$ to $2\frac{1}{2}$ hours.
Charging and firing	15 minutes.
Clearing away débris	2 hours.

or a total of between $4\frac{1}{2}$ to $5\frac{1}{2}$ hours, resulting in an advance of 3 feet 9 inches, or a daily advance of nearly 19 feet 6 inches.

The progress of each of the two faces during the month of April last has averaged 17 feet $3\frac{1}{2}$ inches per day, and is a remarkable corroboration of the speed estimated by the engineers four years ago. The estimate was as follows:—

1st year, the daily progress at each face would be	8·85 feet
2nd " " " "	17·22 "
3rd " " " "	19·18 "
4th " " " "	21·32 "
5th " " " "	31·16 "

The work is now in its second year, so that the estimated speed is being exceeded. In other words, the tunnel is being driven through granite at a higher speed than is attained in London clay.

Water power is abundant, and the waters of the Rhone are harnessed to the work, whilst those of the "Diveria" provide the power at Iselle.

Views are given of the intake from the Rhone, the concrete aqueduct, the metallic conduit pipes, 3 feet, and 3 feet 3 inches in diameter, which carry a pressure of 250 lbs. to the inch. The further necessary increase in pressure is obtained by high pressure pumps in the power house.

It was at one time intended to sink a 20-inch bore-hole from the village of Berisal to the tunnel, a depth of some 2400 feet, for the purpose of delivering water at high-pressure for the works. This may still be done, but the meandering of the tool might result in the awkward dilemma of having to search for it, in solid rock, below ground.

Some few years ago a rather amusing incident occurred in connection with a tunnel, which is worth recording. A certain railway company were constructing a tunnel beneath and nearly at right angles to an existing tunnel of one of the large English railway companies. As the legal formalities were not actually completed the engineers were requested to stay proceedings until all was in order, and they instructed the contractors accordingly, but the latter were anxious not to incur any delay, and they quietly and surreptitiously continued to drive their heading through. The engineer of the existing railway suspected this, and sank a bore-hole on the centre line of the new work, expecting his tool would, at the correct level, drop into the heading, at a depth of 70 feet. The contractors looked

for a similar result, and therefore placed a sheet of steel on the roof of their drift, so that the tool, when it encountered the steel plate, would simply grind away on the top.

But, to the mutual surprise of both the engineer of the existing company and of the contractors for the new work, no drill was encountered, although it had gone to a lower depth than was necessary, some 90 feet. The engineer thereupon lowered, in a foolhardy manner, an explosive charge, and blew in the side of the heading, the tool having meandered several feet to one side. Fortunately no one was hurt, but the engineer was still in ignorance as to what had happened. A bright idea struck him—namely, to lay on the town fire-supply of water down the hole to see if he could fill it. The result was he nearly washed the men away in the heading!

Electric Traction.—It is desirable to point out how very necessary it may be, in the case of this and other long tunnels, that electric traction should be adopted. Abundant power close at hand already exists; the air of the tunnel would not be vitiated—a matter of great importance where briquet fuel is used—and the rapidity of conducting the traffic would be improved.

In Baltimore an electric locomotive is attached to the through expresses, which takes them through, steam engine and all, at fifty to sixty miles an hour. No stoppage of the express is required at the further end, the electrical locomotive running ahead into a siding; and some of the very heaviest freight trains, including the locomotive and tender (far heavier than are ever seen in Great Britain), are hauled against a gradient of 1 in 138 at fifteen miles an hour.

In fact, in England, we are most lamentably backward in the employment of electricity, and unless the Central and the Local Authorities can be aroused from their lethargy, and from their opposition to all such enterprises, England will continue to lag in the rear of other nations, instead of, as in past years, teaching them a more perfect method.

In conclusion, may I ask for the sympathy, nay more, for a silent prayer on behalf of our tunnel and railway heroes, when we are passing along some of the great railway works of the country, or of the world.

Need I refer to that young Resident Engineer who, when a length of a certain tunnel during construction through quicksand fell in, burying eleven men, volunteered at the risk of his life, in consequence of the men being panic-stricken, to go down the shaft and rebuild the damaged work with his own hands and alone? And to that ganger who, having held back for a time, seeing that the engineer was determined to do the work, jumped into the bucket with some strong language to the effect "that he wouldn't see the master killed alone," and went down, and they two completed the next length before the men would return to work.

There are heroes on our railways as there are in our army and navy, and they deserve better recognition. May I plead on behalf

of our inspectors and superintendents of our great railway stations, who are in many cases almost worked to death, and yet have to be attentive and courteous to all ; albeit, except in certain honourable exceptions, they are unable to make proper provision for old age ? And should it be necessary for a station-master, after six years' work at a great railway junction, to drop into his grave with the simple epitaph "tired out"?

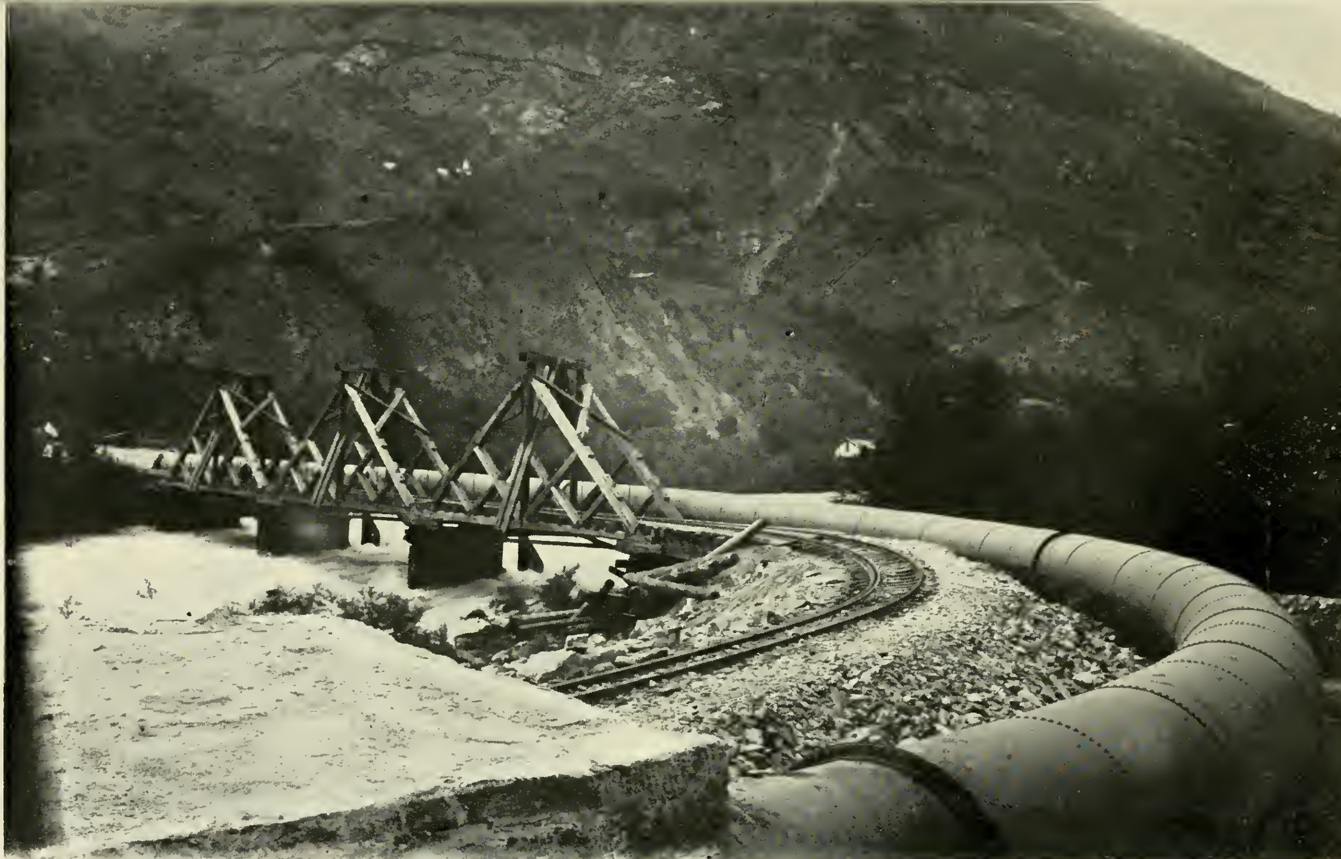
[F. F.]

Simplon Tunnel—Locomotive used inside the Tunnel, provided with Large Boiler to avoid firing up whilst under the Mountain.

Simplon Tunnel—Miners about to enter the Tunnel at Brigue, in the Rhone Valley. The Method of Transporting

Simplon Tunnel—The Brandt Hydraulic Rock Drill Entering Tunnel with its Gang of Workmen, showing the Rack Bar holding Three Separate Boring Machines.

Simplon Tunnel—Entrances of Tunnels, on the Italian side, in the Gorge of the Diveria, near Iselle.



Simplon Tunnel—Crossing of the Rhone by the Steel Hydraulic Main (3 ft. 3 in. diameter) Conducting Water from the Water House to Power House, a Distance of about Two Miles (250 lbs. pressure per square inch).

WEEKLY EVENING MEETING.

Friday, June 1, 1900.

Ludwig Mond, Esq., Ph.D. F.R.S., Vice-President, in the Chair.

Sir Henry Roscoe, Ph.D. D.C.L. LL.D. F.R.S.

Bunsen.

I hope that none of my audience will fall into the mistake of confusing the chemist Bunsen with the chevalier of the same name. This happened at a garden party when I was present, at the late Mr. Gassiot's in 1862, and at which Bunsen and Kirchhoff were honoured guests. A lady addressing the former, asked, 'Pray sir, have you yet completed your great work on God in History? "Alas! no," replied the chemist, with the genial humour which was one of his chief attractions, "Alas! no, madam, my untimely death prevented me from completing my task."

For more than fifty years, Bunsen, the great investigator, the great teacher, devoted himself solely to the advancement of his science. Born in a German university town, Göttingen, in 1811, he spent his life in discharging the quiet duties of a German University Professor; dying in August, 1899, at Heidelberg. To such men, life passes away in an outwardly uneventful manner. A scientific excursion to Iceland, or holiday visits to Italy or Switzerland, may occasionally break the monotony of the yearly recurring course of lectures, or of the still more tiresome perfunctory duties of a University Examiner. But the inner life of a man like Bunsen, is full of events of great and even of phenomenal interest. The discovery of a fact which changes or overthrows our ideas on a whole branch of science; the experimental proof of a law hitherto unrecognised; the employment of known facts in a new and happy combination to effect results of general applicability and usefulness: these are some of the peaceful victories of the man of science, which may by many seem to outweigh the popular achievements of the more public professions. Such things come to all in greater or lesser degree—who, like Bunsen, devote themselves unselfishly and completely to the furtherance of the knowledge of nature—and to him they came in rich abundance.

To give more than an idea of the unbroken scientific work of half a century in sixty minutes is an impossible task. I must therefore content myself with referring to a few of the more salient points of Bunsen's discoveries, and attempt, in I fear a most incomplete way,

to make known to you what manner of man he was—how he worked and taught, so that you may understand the feelings of admiration and affection with which all those who came under his influence regarded him, and why they look back upon his memory as that of one whom it has been a privilege to know. Many people—even some chemists I fear—have heard of the Bunsen Burner, and the Bunsen Battery, but have no further knowledge of the great work which he accomplished.

Let me, to begin with, give you an idea of Bunsen's personality, and show you his likeness when he was in the height of his strength, in 1862, and again in 1887, when age had mellowed but not weakened his features. It has been well said, you know, that Davy's greatest discovery was Faraday—so too we may add was Bunsen's discovery of Kirchhoff; for this brought about one of the greatest steps in the progress of modern science—the foundation of spectrum analysis.

It was in the autumn of 1852 that I first became acquainted with the man who afterwards—for nearly fifty years—was one of my most intimate friends, to whom I owe more than I can tell. At that time Bunsen was at the height of his power, both physical and mental; he had just been called to the Chair of Chemistry at Heidelberg, and was facile princeps amongst the active workers and teachers of the science. He stood fully six feet high, his manner was simple yet dignified, and his expression of rare intelligence and of great kindness. This first impression of his bearing and character only became stronger as my knowledge of him was more intimate, and the feelings of respect and affection with which I regarded him, were only expressive of the attitude of all with whom he came in contact. His singular amiability was not a sign of weakness but of strength of character. His modesty was natural and in no degree assumed. In his lectures, when giving an account of some discovery he had made, or some new apparatus or method of work which he had instigated, I never heard him mention himself. It was always "man hat dies gefunden," or "es hat sich so herausgestellt." In his old age, and looking back on his life-work, he writes to me that he "feels as keenly as ever how modest and contemptibly small is the amount which I have added to the building of science." And yet the contributions of this man have been equalled by few. He was the pioneer in some of the most important scientific discoveries of the century. His work was not only of a truly original character, but to use an expression which has lately come into vogue, it reached the bed-rock of the subject. He laid the foundations of many branches of chemical science. In pure chemistry, in chemistry applied to the arts and industry, in physical chemistry, and in chemical geology his researches have long ranked as classics, and will thus remain for time to come.

Let me give you a few examples. But first I would ask you to look in at his laboratory at Heidelberg and see him at work. At eight o'clock every morning he lectured on General Chemistry.

These lectures were, like everything he touched, marked by originality of treatment. He did not attempt to catch the attention of his audience by brilliancy of style or by firework experiments; but his exposition was luminous, and his experiments, always made with his own hands, exactly illustrative of the matter under discussion. If during a lecture he had occasion to refer to his own discoveries, no hint—as I have said—as to its origin escaped his lips; but the students were well aware of the fact, and gave him a warm round of applause as he concluded his discourse. Then he would bow, smile as acknowledging the compliment, and quickly retire. As soon as the lecture was over, Bunsen went into his laboratory. There he would find about a hundred men waiting for his assistance and advice, and there he spent the whole of his day, superintending the practical work of the students. To work with Bunsen was a real pleasure, and to every student who showed interest in his work this pleasure came. He did not confine his attention merely to those who were engaged in original work; even the beginner had the benefit of seeing how the master worked, and some of the most elementary operations in analytical chemistry would be performed by the Professor, standing at the working bench of the pupil. Thus, he taught us not only by precept, but by example, and from him we learnt what accurate work meant. We saw how to eliminate errors of experimentation, and to find out where more errors lay. It was this complete devotion to his science and to his students, that drew men from all quarters of the globe to work under him; and no one who cared to benefit from his teaching was ever sent empty away, and all who had worked in the Heidelberg laboratory, looked back upon the time spent there as one of the most fruitful of their lives. But it was specially to the advanced students engaged in original investigation that Bunsen's heart went out, and to them he gave unstintedly his time and labour. For to these men he knew the future of the science belonged, and it was they who would hand down, burning more or less brilliantly, the torch of progress. There would be, perhaps, twenty men thus engaged, not, as in many laboratories, all working on closely cognate subjects, but each one on matters differing widely from the other, and therefore requiring much greater grasp and attention on the part of the teacher, to whom the initiation and often the general conduct of the research was due. This constant presence of the master, this participation by him in the work of the pupils both young and old, bore in on the minds of all the lesson that it is the personal and daily contact with the leader which creates a successful school; and that whilst fine buildings and well-equipped laboratories are good things in their way, they are as tinsel and dross, unless accompanied by the devotion and collaboration of the teacher, illustrating the truth of the proverb that 'mind is greater than matter.'

How, it may well be asked, could Bunsen thus devoted to supervising the work of others in the laboratory; who had to deliver a lecture every day, and had much perfunctory university business to

transact as well, how could he possibly find time to carry on laborious experimental investigations of his own? for it must be remembered that he never kept an assistant to work at his researches for him, but did all the experimental work with his own hands. Well, it is always the busy man who has most time for work—or, at least who does most—and so it was with Bunsen. Spending the whole day in the laboratory he was often able to spare an hour or two to devote to his own work, either of devising and testing some new form of apparatus, of separating the rare earth metals, or of preparing and determining the crystalline form of a series of salts. Then he was an early riser, and when I lived with him, I know that it was his habit to rise often before dawn in the summer, to complete an experiment or to edit a research. The long summer holiday again was a great time for work. We often spent a few weeks together in excursions in Switzerland or the Tyrol to rub off the fatigues of the session, but we soon returned to Heidelberg, and in those quiet days we got through much experimental work, for I had the good fortune for many years to be associated with Bunsen in a research on Photochemical Measurements, about which I shall say a few words later on.

Let me now glance rapidly at some of his more important investigations.

The research which stamped Bunsen as a first rate experimentalist, was his investigation on the cacodyl compounds, on which he laboured for six years. It is remarkable as an example of how the most difficult and dangerous problems of experimental chemistry can be solved by a master hand. It was a frightfully difficult and dangerous research, because the compounds are both poisonous and explosive. In 1846 Bunsen was nearly killed and poisoned when examining the properties of cacodyl cyanide. "It is remarkable," he says, "that when one is exposed to the smell of these compounds the tongue becomes covered with a black coating, and the smell produces giddiness and even insensibility." I therefore do not propose now to illustrate these properties experimentally.

The cacodyl research claims our interest, not only because it furnishes us with the first example of an isolable radicle, but also because it assisted Frankland and Kekulé in more exactly illustrating the term "chemical valency." For it is not too much to say that the subsequent researches of Frankland on the organo-metallic bodies, and on the so-called alcohol radicles, as well as those of the French chemists, and, I may add, those of Baeyer, received their first impulse from the cacodyl investigation. This indebtedness was acknowledged by one whose voice and face, once familiar to this audience, we all of us now sadly miss—the late Sir Edward Frankland—in the graceful and modest words which appear in the dedication of the volume of his collected researches:—

"To my friend and teacher, Robert William Bunsen, whose researches on cacodyl, on the gases of the iron furnaces, and on the

volcanic phenomena of Iceland, I have always regarded as models of investigation in pure, applied and physical chemistry, I dedicate these pages, both as a testimony of my regard and in gratitude f r the teaching whereby he imbued me with the necessity for thoroughness and accuracy in all scientific work. Would that they were more worthy of such a high standard."

Thus it is seen that although this remarkable research is the only one of any importance which was carried out by Bunsen in the domain of organic chemistry, it was destined to exert such an influence on the later developments of that branch of the science, that he may with truth be regarded as one of the pioneers of modern organic chemistry.

The next research to which I shall refer is one of a totally different character, but not of less importance than the last. It is interesting as the first attempt, and a successful one, to introduce accurate scientific methods and inquiry into so important an industry as that of iron smelting. Up to 1845 the production of cast iron in the blast furnace was carried on mainly in ignorance of the scientific principles upon which it depends. The waste of fuel was enormous, amounting often to 80 per cent. of the whole. Bunsen, by analysing the escaping gases, showed how this loss could be made good; how the heat of the burning gases could be utilized, and, in conjunction with the late Lord (then Lyon) Playfair, he conducted a series of experiments which have resulted in economies the value of which may be reckoned by millions rather than by thousands of pounds. But in other divisions clear light was thrown upon the chemistry of the blast furnace by these researches. Thus the formation of cyanogen in the furnace was unknown until discovered accidentally, as thus described by Playfair. "Bunsen was engaged below," at the blast furnaces at Alfreton, in Derbyshire, "and I above, passing the gases through water to collect any soluble products, when I was alarmed by being told that my friend had become suddenly ill. I ran down and saw white fumes coming out of a lateral tube, and Bunsen apparently recovering from a fainting condition. I applied my nose to the orifice and smelt the vapour of cyanide of potassium, which gave an entirely new light to the processes of the furnace."

These important results could not have been achieved if Bunsen had not previously elaborated an accurate method of gas analysis. These processes enabled him to do what had hitherto been impossible for want of exact methods. No one could before his time undertake accurate determinations of the several constituents of a gaseous mixture. His book on gasometry—the only book he ever wrote—is a remarkable one. For originality of conception, for success in overcoming difficulties, for ingenuity in the construction of apparatus, and for accurate methods, this book as a record of experimental work is, I believe, unequalled. It was always a matter of congratulation with the Heidelberg student when he was set to learn the process of gas analysis, for then he came into direct contact with the master, who would spend half the morning in the gas-analysis room going through

all the detailed manipulation of the exact measurement and separation of the various constituents of a sample of coal-gas, and pointing out how the calculations are to be made.

Many were the physical properties of gases which formed the subject of Bunsen's investigation. He devised new methods of attack; he invented novel instruments for effecting his object, and was then able to study with accuracy the phenomena of gaseous diffusion and absorption. All these researches were masterpieces of experimental skill and of accurate and pains-taking work.

Of all Bunsen's useful and ingenious inventions, that of the gas-burner which bears his name is the most widely known and valued. There is scarcely a household or a manufactory where this little lamp is not in daily use for one purpose or another. About its discovery an interesting tale can be told.

Some short time before the opening of the new laboratory in 1856, the town of Heidelberg was for the first time lighted with gas, and Bunsen had to consider what kind of gas-burner he would use for laboratory purposes. Returning from my Easter vacation in London, I brought back with me an Argand burner with copper chimney and wire-gauze top, which was the form commonly used in English laboratories at that time for working with a smokeless flame. This arrangement did not please Bunsen in the very least; the flame was flickering, it was too large, and the gas was so much diluted with air that the flame-temperature was greatly depressed. He would make a burner in which the mixture of gas and air would burn at the top of the tube without any gauze whatsoever, giving a steady, small and hot, non-luminous flame under such conditions that it not only would burn without striking down when the gas supply was turned on full, but also when the supply was diminished until only a minute flame was left. This was a difficult, some thought it an impossible problem to solve; but, after many fruitless attempts and many tedious trials, he succeeded, and the "Bunsen burner" came to light.

In this burner an important principle is involved. The mixture of air and gas in the tube must never reach the point at which an explosive mixture is produced, viz. one volume of gas to about ten volumes of air, either when the supply of gas is full on or turned nearly off.

This can only be effected by varying the volume of the aspirated air in proportion to that of the issuing gas, and this is done automatically by variation in a zone of diminished pressure caused by the flow of the issuing gas. This same principle is illustrated by Faraday's well-known experiments with the two cards.

The carbon-zinc battery (1841) which goes by his name is one of Bunsen's best known discoveries. The carbon cylinders, which replace the platinum of Grove, rendered this form of battery the cheapest and most reliable source of electricity until the genius of Faraday rendered the dynamo possible. It is interesting to remember that as early as 1843 Bunsen foreshadowed the production of light

by electricity, and made the first step towards the modern system of arc lighting. In 1852 Bunsen turned his attention to applying his battery to the electrolytic production of metals, the reduction of which had hitherto baffled the attacks of the chemist. Magnesium first yielded, next came the metals of the alkaline earths—calcium, strontium, and later on cerium, lanthanum and didymium. In 1856 Bunsen and I determined the actinic value of the light of burning magnesium; we found that this was so great that it might be used for photographic purposes, and the first photographic portrait by the magnesium light was taken by myself in this room on May 6, 1864, when, during a lecture which I gave, Faraday was photographed as he sat, and also Sir Henry Holland, who presided on that occasion.

Another well-known instrument invented by Bunsen (1844) is his photometer, used now almost exclusively for measuring the illuminating power of coal gas. The essential feature of this apparatus is a disc of paper having a grease spot in the centre, a comparison of the luminous intensity being made when by the approximation of the source of light to the illuminated disc the spot becomes invisible. When this instrument was shown and explained to the late Emperor Frederick he remarked, "For the first time in my life I now know the value of a spot of grease."

The only relaxation from his scientific labours which Bunsen throughout life allowed himself was travelling, and this he thoroughly enjoyed. During many autumn vacations, I had the pleasure of accompanying him in rambles throughout Switzerland and the Tyrol. He walked well, and had a keen appreciation of natural beauty, especially of mountain and woodland scenery, whilst he took great interest in the geology and physical characteristics of the districts through which he passed, and this it was that led him to turn his mind to chemico-geological studies. So early as 1844, in company with Pilla and Matteucci, he visited and carefully examined the carboniferous deposits occurring in the well known fumerole districts of the Tuscan Maremma, and in 1846 he undertook his journey to Iceland, where he spent three and a half months, and the outcome of which was the well-known series of investigations on the volcanic phenomena of that island. No doubt it was the eruption of Hecla in 1845 which served as the incentive to this expedition, for he desired not only to examine the composition of the Icelandic rocks, which are entirely of volcanic origin, but especially the pseudovolcanic phenomena which present themselves in greater force immediately after a period of activity than at other times.

The expedition to Iceland was an official one promoted by the Danish Government. Bunsen was accompanied by Sartorius von Waltershausen and Bergman, both colleagues at Marburg, as well as by the French mineralogist Des Cloizeaux. They left Copenhagen on May 4, 1846, reaching Reykiavik after a short but stormy passage of eleven days. The party spent ten days at the foot of Hecla, where Bunsen collected the gases emitted by the fumeroles, and investigated

the changes which these gases effect on the volcanic rocks with which they come into contact. Eleven more days were given to the investigation of the phenomena of the geysers, and at the end of August Bunsen left the island, having in the short space of about three months collected a mass of material the working up of which, as he writes to Berzelius, "will tax all my energies for some length of time"—a prediction which was subsequently fully realised.

To enlarge upon this research is beyond the province of the present discourse. Suffice it to say that Bunsen's original investigation lies at the foundation of modern petrology, and has opened out an unbounded field for research, the cultivation of which has already yielded great results and will in future yield still greater ones. Tyndall's remarkable experimental illustration of Bunsen's geyser theory will be remembered by many of the older members of this Institution.

Of all Bunsen's researches the one which will undoubtedly stand out pre-eminent as time rolls on is that on spectrum analysis. The most important discovery made by Bunsen during the short duration of his residence in Breslau was, as I have said, the discovery of Kirchhoff, who was then Professor of Physics in that University, and whose great ability the elder man at once recognised. No sooner had Jolly removed to Munich, in 1854, than Bunsen took care that Kirchhoff should be his successor in the Heidelberg Chair of Physics. And thus came about that great twin research which has made the name of these men known through the wide world. To dilate upon the importance of the discovery is unnecessary; to follow out the growth of this branch of science in its height and depth and breadth is here impossible. All that is now possible is to remind you of Kirchhoff's great discovery—namely, the full explanation of Fraunhofer's lines in the solar spectrum, pointing the way to a knowledge of the chemical composition of the sun and fixed stars—and that of Bunsen's application of the principles of spectrum analysis to the examination of terrestrial matter.

On March 1, 1861, I had the honour of bringing before a Royal Institution audience the results of these discoveries—viz., those of Kirchhoff and Bunsen, made in the autumn of 1859; those of Bunsen, in 1860 and 1861. In that discourse I gave an account of the two new metals cæsium and rubidium. I ventured to say whilst these researches were in their earliest infancy, that the dawn of a new stellar and terrestrial chemistry had been announced, thus opening out for investigation a bright prospect of vast fields of unexplored truth. And the subsequent work of forty years, and of many investigators, has not falsified this prediction.

In a letter to me dated April 10, 1860, Bunsen writes as follows:—

"Do not be annoyed with me, dear Roscoe, that I have done nothing with our light investigation. I have left everything untouched, because I have obtained full certainty, by means of spectrum analysis,

that besides Ka, Na, and Li, a fourth alkali metal must exist, and all my time has been occupied in endeavouring to isolate some compounds of the new substance. Where the presence of this body is indicated, it occurs in such minute quantity that I almost give up hope of isolating it unless, indeed, I am fortunate enough to find a material which contains it in larger amount."

On November 6, 1860, Bunsen describes to me his further work on the new metal as follows:—

"I have been very fortunate with my new metal. I have got 50 grams of the nearly chemically pure chloro-platinic compound. It is true that this 50 grams has been obtained from no less than 40 tons of the mineral water, from which 2·5 lbs. of lithium carbonate have been prepared by a simple process as a by-product. I am calling the new metal "cæsium," from "cæsius," blue, on account of the splendid blue line in its spectrum. Next Sunday I hope to find time to make the first determination of the atomic weight."

The rare combination of mental and manual dexterity characteristic of Bunsen is nowhere more strikingly shown than in the investigation of the cæsium compounds. From these 17 grams of cæsium chloride, obtained as above described, he not only succeeded in preparing and analysing all the more important compounds, but in crystallising the salts in such a form that he was able to determine their crystallographic constants, and then to supply all the necessary data for fixing the position of this new element and its compounds in relation to its well-known relatives potassium and sodium.

All the world knows that, shortly after his discovery of cæsium, the birth of another new alkali-metal, rubidium,* was announced by Bunsen, and the application of spectrum analysis led to the isolation of thallium in 1861, indium in 1863, germanium in 1866, gallium in 1875, and scandium in 1879, but alongside of these came announcements of the discovery of other new metals whose existence was more than doubtful. Concerning these he writes to myself: "The frivolous way in which new metals are now discovered by dozens, and sent forth into the world duly christened, is certainly no gain to science; only later inquirers will be able to decide what remains new and serviceable out of this chaos of material."

I had the good fortune to be associated with Bunsen for many years in a somewhat difficult and most interesting research on the measurement of the chemical action of light. So long ago as April 4, 1856, I brought before a Royal Institution audience the first fruits of this work, and on May 22, 1863, and again, June 1, 1866, I explained the further results which we had obtained. It is impossible for me to do more on the present occasion than to remind you by an experiment that it is the more refrangible rays of the solar spectrum which have the greatest power of setting up chemical change, and to

* Berlin Monatsh. 1861, vi. 273.

give you an idea of the value attached by chemists to the results of this investigation by quoting the opinion of one eminently qualified to judge, namely, Professor Ostwald of Leipzig:—

"In no other research in this domain of science," says Ostwald, "do we find exhibited such an amount of chemical, physical and mathematical dexterity, of ability in devising experiments, of patience and perseverance in carrying them out, of attention given to the minutest detail, or of breadth of view as applied to the grander meteorological and cosmical phenomena of nature."

In this connection I think that the following letter from Bunsen to myself, now published for the first time, may interest my audience. It requires only a word or two of explanation. On my return to England from Heidelberg for the Christmas holidays, 1855–6, I heard for the first time of Draper's previous work on a chlorine and hydrogen "tithonometer," and, somewhat downcast by this discovery, I wrote to Bunsen on the subject. His wise and encouraging words put new heart into me, and I returned to work at Heidelberg determined to do my best to prove equal to the task that lay before me.

"Heidelberg, 13*th January*, 1856.

"My dear Friend,

"I think that Draper's experiments will not require to be repeated by us any more than Witwer's. Independently of much that appears to me to be inexplicable in them, the pressure to which the luting liquid saturated with H and Cl is subjected constantly changes. I therefore conclude that Draper's instrument will not indicate proportionality, etc., especially as the volume of the insolated gas is so small compared with that of the luting liquid. At any rate I see no grounds for interrupting our experiments, still less do I consider that it is a misfortune that the results which we have obtained should have been to some extent previously described by him. It appears to me that the value of an investigation is not to be measured by whether something is described in it for the first time, but rather by what means and methods a fact is proved beyond doubt or cavil, and in this respect I think that Draper has left plenty for us to do. Do not, therefore, let your discovery of Draper's work disconcert you. I am now busy getting my Eudiometry ready for press, and I hope by Easter to have made an end of it. My best greetings to Williamson, in hearty friendship,

Yours,

R. W. Bunsen."

In 1889 Bunsen retired from active University life, resigning his professorship, and therefore his official residence, and retiring to a pretty little villa in "Bunsen Strasse," which he had purchased, where he spent the remainder of his days in quiet repose. His chief relaxation and enjoyment throughout his life in Heidelberg was to

wander with Kirchhoff or Helmholtz, or some other of his intimate friends, through the chestnut woods which cover the hills at the foot of which the town lies. As the infirmities of age increased, and his walking powers diminished, he was obliged to take to driving through the woods along the charming roads which intersect the hills in all directions. Writing became a difficulty, and in his latter days the news of him came to me through our mutual friends, Quincke and Königsberger. One of the last letters I received from him is dated June 4, 1890:—

". . . . I have been suffering for weeks from the after-effects of influenza, and I am still so weak that I have to spend my days on the sofa, and have scarcely strength to walk the few yards to dinner at the Grand Hotel. When I think that next March I enter on my eightieth year, I must resign myself to the fact that such a state of things is inevitable. My hearing, too, becomes more and more difficult, and my eyes are worse, so I have to deny myself all social intercourse, and only see now and then one of my old friends who comes to look me up. But in spite of all this, I can still feel the humour of life."

Rarely at any time or place has a University enjoyed the presence of so remarkable a galaxy of eminent men as was enjoyed for many years by the Carola Ruperto from about the middle of the present century. The central figure of this illustrious band was Bunsen; round him were grouped, to name only some of his colleagues and friends in the faculty of science, Kirchhoff, and after him Quincke; Helmholtz, and after him Kühne; Fuchs, and after him Leo Königsberger; and last, but not least, Hermann Kopp and Rosenbusch, and the ever-to-be-lamented Victor Meyer; whilst in other faculties were Schlosser, Gervinus and Hausser, the historians and statesmen; von Mohl, the jurist and diplomatist; Zeller, the theologian; Vangerow, the great Pandectanist; and other scarcely less distinguished men who at that time adorned the faculties of Philosophy, Medicine and Law in the University of Heidelberg.

Almost up to the last Bunsen continued to take a vivid interest in the progress of scientific discovery, and, though suffering from pain and weakness, ever preserved the equanimity which was one of his lifelong characteristics. Three days before his death, so Quincke writes to me, he lay in a peaceful slumber, his countenance exhibiting the fine intellectual expression of his best and brightest days. Thus passed away, full of days and full of honours, a man, equally beloved for his great qualities of heart as he is honoured for those of his fertile brain, the memory of whom will always remain green amongst all who were fortunate enough to number him amongst their friends.

[H. E. R.]

WEEKLY EVENING MEETING,

Friday, June 8, 1900.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Allan Macfadyen, M.D. B.Sc., Director of the Jenner Institute of Preventive Medicine.

The Effect of Physical Agents on Bacterial Life.

(Abstract.)

The fact that life did not exist upon the earth at a remote period of time, the possibility of its present existence as well as the prospect of its ultimate extinction, can be traced to the operation of certain physical conditions. These physical conditions upon which the maintenance of life as a whole depends are in their main issues beyond the control of man. We can but study, predict and it may be utilise their effects for our benefit. Life in its individual manifestations is, therefore, conditioned by the physical environment in which it is placed. Life rests on a physical basis, and the main springs of its energies are derived from a larger world outside itself. If these conditions, physical or chemical, are favourable, the functions of life proceed ; if unfavourable, they cease—and death ultimately ensues. These factors have been studied and their effects utilised to conserve health or to prevent disease. It is our purpose this evening to study some of the purely physical factors, not in their direct bearing on man, but in relation to much lower forms in the scale of life—forms which constitute in number a family far exceeding that of the human species, and of which we may produce at will in a test-tube, within a few hours, a population equal to that of London. These lowly forms of life—the bacteria—belong to the vegetable kingdom, and each individual is represented by a simple cell.

These forms of life are ubiquitous in the soil, air and water, and are likewise to be met with in intimate association with plants and animals, whose tissues they may likewise invade with injurious or deadly effects. Their study is commonly termed bacteriology—a term frequently regarded as synonymous with a branch of purely medical investigation. It would be a mistake, however, to suppose that bacteriology is solely concerned with the study of the germs of disease. The dangerous microbes are in a hopeless minority in comparison with the number of those which are continually performing varied and most useful functions in the economy of nature. Their wide importance is due to the fact that they ensure the resolution and

redistribution of dead and effete organic matter, which if allowed to accumulate would speedily render life impossible on the surface of the earth. If medicine ceased to regard the bacteria, their study would still remain of primary importance in relation to many industrial processes in which they play a vital part. It will be seen, therefore, that their biology presents many points of interest to scientific workers generally. Their study as factors that ultimately concern us really began with Pasteur's researches upon fermentation. The subject of this evening's discourse, the effect of physical agents on bacterial life, is important not merely as a purely biological question, though this phase is of considerable interest, but also on account of the facts I have already indicated, viz. that micro-organisms fulfil such an important function in the processes of nature, in industrial operations, and in connection with the health of man and animals. It depends largely on the physical conditions to be met with in nature whether the micro-organisms exercise their functions, and likewise whether they die or remain inactive. Further, the conditions favouring one organism may be fatal to another, or an adaptability may be brought about to unusual conditions for their life. To the technologist the effect of physical agents in this respect is of importance as a knowledge of their mode of action will guide him to the means to be employed for utilising the micro-organisms to the best advantage in processes of fermentation. The subject is of peculiar interest to those who are engaged in combating disease, as a knowledge of the physical agents that favour or retard bacterial life will furnish indications for the preventive measures to be adopted. With a suitable soil and an adequate temperature the propagation of bacteria proceeds with great rapidity. If the primary conditions of soil and an adequate temperature are not present, the organisms will not multiply, they remain quiescent or they die. The surface layers of the soil harbour the vast majority of the bacteria, and constitute the great storehouse in nature for these forms of life. They lessen in number in the deeper layers of the soil, and few or none are to be met with at a depth of 8–10 feet. As a matter of fact, the soil is a most efficient bacterial filter, and the majority of the bacteria are retained in its surface layers and are to be met with there. In the surface soil, most bacteria find the necessary physical conditions for their growth, and may be said to exist there under natural conditions. It is in the surface soil that their main scavenging functions are performed. In the deeper layers, the absence of air and the temperature conditions prove inimical to most forms.

Amongst pathogenic bacteria the organisms of lockjaw and of malignant œdema appear to be eminently inhabitants of the soil. As an indication of the richness of the surface soil in bacteria, I may mention that 1 gramme of surface soil may contain from several hundred thousand to as many as several millions of bacteria. The air is poorest in bacteria. The favouring physical conditions to be met with in the soil are not present in the air. Though bacteria are to be met with in the air, they are not multiplying forms as is the case in

the soil. The majority to be met with in air are derived from the soil. Their number lessens when the surface soil is moist, and it increases as the surface soil dries. In a dry season the number of air organisms will tend to increase.

Town air contains more bacteria than country air, whilst they become few and tend to disappear at high levels and on the sea. A shower of rain purifies the air greatly of bacteria. The organisms being, as I stated, mainly derived from the surface of the ground, their number mainly depends on the physical condition of the soil, and this depends on the weather. Bacteria cannot pass independently to the air, they are forcibly transferred to it with dust from various surfaces. The relative bacterial purity of the atmosphere is mainly, therefore, a question of dust. Even when found floating about in the air the bacteria are to be met with in much greater number in the dust that settles on exposed surfaces, e.g. floors, carpets, clothes and furniture. Through a process of sedimentation the lower layers of the air become richer in dust and bacteria, and any disturbance of dust will increase the number of bacteria in the air.

The simple act of breathing does not disseminate disease germs from a patient, it requires an act of coughing to carry them into the air with minute particles of moisture. From the earliest times great weight has been laid upon the danger of infection through air-borne contagia, and with the introduction of antiseptic surgery the endeavour was made to lessen this danger as much as possible by means of the carbolic spray, etc. In the same connection numerous bacteriological examinations of air have been made with the view of arriving at results of hygienic value. The average number of micro-organisms present in the air is 500–1000 per 1000 litres; of this number only 100–200 are bacteria, and they are almost entirely harmless forms. The organisms of suppuration have been detected in the air, and the tubercle bacillus in the dust adhering to the walls of rooms. Investigation has not, however, proved air to be one of the important channels of infection. The bactericidal action of sunlight, desiccation and the diluting action of the atmosphere on noxious substances will always greatly lessen the risk of direct aerial infection.

The physical agents that promote the passage of bacteria into the air are inimical to their vitality. Thus, the majority pass into the air not from moist but from dry surfaces, and the preliminary drying is injurious to a large number of bacteria. It follows that if the air is rendered dust-free, it is practically deprived of all the organisms it may contain. As regards enclosed spaces, the stilling of dust and more especially the disinfection of surfaces liable to breed dust or to harbour bacteria are more important points than air disinfection, and this fact has been recognised in modern surgery. In an investigation, in conjunction with Mr. Lunt, an estimation was arrived at of the ratio existing between the number of dust particles and bacteria

in the air. We used Dr. Aitken's Dust-counter, which not only renders the dust particles visible, but gives a means of counting them in a sample of air. In an open suburb of London we found 20,000 dust particles in 1 cubic centimetre of air; in a yard in the centre of London about 500,000. The dust contamination we found to be about 900 per cent. greater in the centre of London than in a quiet suburb. In the open air of London there was on an average just one organism to every 38,300,000 dust particles present in the air, and in the air of a room, amongst 184,000,000 dust particles, only one organism could be detected.

These figures illustrate forcibly the poverty of the air in micro-organisms even when very dusty, and likewise the enormous dilution they undergo in the atmosphere. Their continued existence is rendered difficult through the influence of desiccation and sunlight. Desiccation is one of nature's favourite methods for getting rid of bacteria. Moisture is necessary for their development and their vital processes, and constitutes about 80 per cent. of their cell-substance. When moisture is withdrawn most bacterial cells, unless they produce resistant forms of the nature of spores, quickly succumb. The organism of cholera air-dried in a thin film dies in three hours. The organisms of diphtheria, typhoid fever and tuberculosis show more resistance, but die in a few weeks or months.

Dust containing tubercle bacilli may be carried about by air currents, and the bacilli in this way transferred from an affected to a healthy individual. It may, however, be said that drying attenuates and kills most of these forms of life in a comparatively short time. The spores of certain bacteria may, on the other hand, live for many years in a dried condition, e.g. the spores of anthrax bacilli which are so infective for cattle and also for man (wool-sorters' disease). Fortunately few pathogenic bacteria possess spores, and, therefore, drying by checking and destroying their life is a physical agent that plays an important rôle in the elimination of infectious diseases. This process is aided by the marked bactericidal action of *sunlight.* Sunlight, which has a remarkable fostering influence on higher plant life, does not exercise the same influence on the bacteria. With few exceptions we must grow them in the dark in order to obtain succesful cultures; and a sure way of losing our cultures is to leave them exposed to the light of day. Direct sunlight is the most deadly agent, and kills a large number of organisms in the short space of one to two hours; direct sunlight proves fatal to the typhoid bacillus in half an hour to two hours, to the diphtheria bacillus in half an hour to one hour, and to the tubercle bacillus in a few minutes to several hours. Even anthrax spores are killed by direct light in three and a half hours. Diffuse light is also injurious, though its action is slower. By exposing pigment-producing bacteria to sunlight colourless varieties can be obtained, and virulent bacteria so weakened that they will no longer produce infection. The germicidal action of the sun's rays is most marked at the blue end of the spec-

trum, at the red end there is little or no germicidal action. It is evident that the continuous daily action of the *sun* along with *desiccation* are important physical agents in arresting the further development of the disease germs that are expelled from the body.

It has been shown that sunlight has an important effect in the spontaneous purification of rivers. It is a well-known fact that a river, despite contamination at a given point, may show little or no evidence of this contamination at a point further down in its course. Buchner added to water 100,000 colon bacilli per cubic centimetre, and found that all were dead after one hour's exposure to sunlight. He also found, that in a clear lake the bactericidal action of sunlight extended to a depth of about six feet. Sunlight must therefore be taken into account as an agent in the purification of waters, in addition to sedimentation, oxidation and the action of algæ.

Air or the oxygen it contains has important and opposite effects on the life of bacteria. In 1861, Pasteur described an organism in connection with the butyric acid fermentation which would only grow in the absence of free oxygen. And since then a number of bacteria, showing a like property, have been isolated and described. They are termed anaerobic bacteria as their growth is hindered or stopped in the presence of air. The majority of the bacteria, however, are aerobic organisms inasmuch as their growth is dependent upon a free supply of oxygen. There is likewise an intermediate group of organisms, which show an adaptability to either of these conditions, being able to develop with or without free access to oxygen. Preeminent types of this group are to be met with in the digestive tract of animals, and the majority of disease-producing bacteria belong to this adaptive class. When a pigment-producing organism is grown without free oxygen its pigment production is almost always stopped. For anaerobic forms N and H_2 give the best atmosphere for their growth, whilst CO_2 is not favourable and may be positively injurious, as e.g. in the case of the cholera organism.

The physical conditions favouring the presence and multiplication of bacteria in water under natural conditions are a low altitude, warmth, abundance of organic matter and a sluggish or stagnant condition of the water. As regards water-borne infectious diseases such as typhoid or cholera, their transmission to man by water may be excluded by simple boiling or by an adequate filtration. The freezing of water, whilst stopping the further multiplication of organisms, may conserve the life of disease germs by eliminating the destructive action of commoner competitive forms. Thus the typhoid bacillus may remain frozen in ice for some months without injury. Employment of ordinary cold is not therefore a protection against dangerous disease germs.

As regards *electricity*, there is little or no evidence of its direct action on bacterial. life, the effects produced appear to be of an indirect character due to the development of heat or to the products of electrolysis.

Ozone is a powerful disinfectant, and its introduction into polluted water has a most marked purifying effect. The positive effects of the electric current may therefore be traced to the action of the chemical products and of heat. I am not aware that any direct action of the X-rays on bacteria has up to the present been definitely proved.

Mechanical agitation, if slight may favour, and if excessive may hinder bacterial development. Violent shaking or concussion may not necessarily prove fatal so long as no mechanical lesion of the bacteria is brought about. If, however, substances likely to produce triturating effects are introduced, a disintegration and death of the cells follows. Thus Rowland, by a very rapid shaking of tubercle bacilli in a steel tube with quartz sand and hard steel balls, produced their complete disintegration in ten minutes.

Bacteria appear to be very resistant to the action of *pressure*. At 300–450 atmospheres putrefaction still takes place, and at 600 atmospheres the virulence of the anthrax bacillus remained unimpaired. Of the physical agents that affect bacterial life, temperature is the most important. Temperature profoundly influences the activity of bacteria. It may favour or hinder their growth, or it may put an end to their life. If we regard temperature in the first instance as a favouring agent, very striking differences are to be noted. The bacteria show a most remarkable range of temperature under which their growth is possible, extending from zero to 70° C. If we begin at the bottom of the scale we find organisms in water and in soil that are capable of growth and development at zero. Amongst these are certain species of phosphorescent bacteria which continue to emit light even at this low temperature. At the Jenner Institute we have met with organisms growing and developing at 34–40° F. The vast majority of interest to us find however the best conditions for their growth from 15° up to 37° C. Each species has a minimum, an optimum and a maximum temperature at which it will develop. It is important in studying any given species that the optimum temperature for their development be ascertained, and that this temperature be maintained. In this respect we can distinguish three broad groups. The first group includes those for which the optimum temperature is from 15–20° C. The second group includes the parasitic forms, viz. those which grow in the living body and for which the optimum temperature is at blood heat, viz. 37° C. We have a third group for which the optimum temperature lies as high as 50–55° C. On this account this latter group has been termed thermophilic on account of its growth at such abnormally high temperatures—temperatures which are fatal to other forms of life. They have been the subject of personal investigation in conjunction with Dr. Blaxall. We found that there existed in nature an extensive group of such organisms to which the term thermophilic bacteria was applicable. Their growth and development occurred best at temperatures at which ordinary

protoplasm becomes inert or dies. The best growths were always obtained at 55–65° C. Their wide distribution was of a striking nature. They were found by us in river water and mud, in sewage, and also in a sample of sea water. They were present in the digestive tract of man and animals and in the surface and deep layers of the soil as well as in straw and in all samples of ensilage examined. Their rapid growth at high temperatures was remarkable, the whole surface of the culture medium being frequently overrun in from fifteen to seventeen hours. The organisms examined by us (fourteen forms in all) belonged to the group of the Bacilli. Some were motile, some curdled milk, and some liquefied gelatin in virtue of a proteolytic enzyme. The majority possessed reducing powers upon nitrates and decomposed proteid matter. In some instances cane sugar was inverted and starch was diastased. These facts well illustrate the full vitality of the organisms at these high temperatures, whilst all the organisms isolated grew best at 55°–65° C. A good growth in a few cases occurred at 72° C. Evidence of growth was obtained even at 74° C. They exhibited a remarkable and unique range of temperature, extending as far as 30° of the Centigrade scale.

As a concluding instance of the activity of these organisms we may cite their action upon cellulose. Cellulose is a substance that is exceedingly difficult to decompose, and is therefore used in the laboratory for filtering purposes in the form of Swedish filter paper, on account of its resistance to the action of solvents. We allowed these organisms to act on cellulose at 60° C. The result was that in ten to fourteen days a complete disintegration of the cellulose had taken place, probably into CO_2 and marsh gas. The exact conditions that may favour their growth, even if it be slow at subthermophilic temperatures, are not yet known—they may possibly be of a chemical nature.

Organisms may be gradually *acclimatised* to temperatures that prove unsuited to them under ordinary conditions. Thus the anthrax bacillus with an optimum temperature for its development of 37° C., may be made to grow at 12° C., and at 42° C. Such anthrax bacilli proved pathogenic for the frog with a temperature of 12° C., and for the pigeon with a temperature of 42° C.

Let us in a very few words consider the inimical action of temperature on bacterial life. An organism placed below its minimum temperature ceases to develop, and if grown above its optimum temperature becomes attenuated as regards its virulence, etc., and may eventually die. The boiling point is fatal for non-sporing organisms in a few minutes. The exact thermal death-point varies according to the optimum and maximum temperature for the growth of the organism in question. Thus for water bacteria with a low optimum temperature, blood heat may be fatal; for pathogenic bacteria developing best at blood heat, a thermophilic temperature may be fatal (60° C.); and for thermophilic bacilli any temperature above

75° C. These remarks apply to the bacteria during their multiplying and vegetating phase of life. In their resting or spore stage the organisms are much more resistant to heat. Thus the anthrax organism in its bacillary phase is killed in one minute at 70° C.; in its spore stage it resists this temperature for hours, and is only killed after some minutes by boiling. In the soil there are spores of bacteria which require boiling for sixteen hours to ensure their death. These are important points to be remembered in sterilisation or disinfection experiments, viz. whether an organism does or not produce these resistant spores. Most non-sporing forms are killed at 60° C. in a few minutes, but in an air-dry condition a longer time is necessary. Dry heat requires a longer time to act than moist heat: it requires 140° C. for three hours to kill anthrax spores. Dry heat cannot therefore be used for ordinary disinfection on account of its destructive action. Moist heat in the form of steam is the most effectual disinfectant, killing anthrax spores at boiling point in a few minutes, whilst a still quicker action is obtained if saturated steam under pressure be used. No spore, however resistant, remains alive after one minute's exposure to steam at 140° C. The varying thermal death-point of organisms and the problems of sterilisation cannot be better illustrated than in the case of milk, which is an admirable soil for the growth of a large number of bacteria. The most obvious example of this is the souring and curdling of milk that occurs after it has been standing for some time. This change is mainly due to the lactic acid bacteria, which ferment the milk sugar with the production of acidity.

Another class of bacteria may curdle the milk without souring it in virtue of a rennet-like ferment, whilst a third class precipitate and dissolve the casein of the milk, along with the development of butyric acid. The process whereby milk is submitted to a heat of 65° to 70° C. for twenty minutes is known as pasteurisation, and the milk so treated is familiar to us all as pasteurised milk. Whilst the pasteurising process weeds out the lactic acid bacteria from the milk, a temperature of 100° C. for one hour is necessary to destroy the butyric acid organisms: and even when this has been accomplished there still remain in the milk the spores of organisms which are only killed after a temperature of 100° C. for three to six hours. It will therefore be seen that pasteurisation produces a partial, not a complete sterilisation of the milk as regards its usual bacterial inhabitants. The sterilisation to be absolute would require six hours at boiling point. But for all ordinary practical purposes pasteurisation is an adequate procedure. All practical hygienic requirements are likewise adequately met by pasteurisation, if it is properly carried out, and the milk is subsequently cooled. Milk may carry the infection of diphtheria, cholera, typhoid and scarlet fevers as well as the tubercle bacillus from a diseased animal to the human subject. For the purpose of rendering the milk innocuous, freezing and the addition of preservatives are inadequate methods

of procedure. The one efficient and trustworthy agent we possess is heat. Heat and cold are the agents to be jointly employed in the process, viz. a temperature sufficiently high to be fatal to organisms producing a rapid decomposition of milk, as well as to those which produce disease in man; this to be followed by a rapid cooling to preserve the fresh flavour and to prevent an increase of the bacteria that still remain alive. The pasteurising process fulfils these requirements.

In conjunction with Dr. Hewlett, I had occasion to investigate in how far the best pasteurising results might be obtained. We found that 60° to 68° C. applied for twenty minutes weeded out about 90 per cent. of the organisms present in the milk, leaving a 10 per cent. residue of resistant forms. It was found advisable to fix the pasteurising temperature at 68° C., in order to make certain of killing any pathogenic organisms that may happen to be present. We passed milk in a thin stream through a coil of metal piping, which was heated on its outer surface by water. By regulating the length of the coil, or the size of the tubing, or the rate of flow of the milk, almost any desired temperature could be obtained. The temperature we ultimately fixed at 70° C. The cooling was carried out in similar coils placed in iced water. The thin stream of milk was quickly heated and quickly cooled as it passed through the heated and cooled tubing, and whilst it retained its natural flavour, the apparatus accomplished at 70° C. in thirty seconds a complete pasteurisation, instead of in twenty minutes, i.e. about 90 per cent. of the bacteria were killed, whilst the diphtheria, typhoid, tubercle and pus organisms were destroyed in the same remarkably short period of time, viz. thirty seconds. This will serve to illustrate how the physical agent of heat may be employed, as well as the sensitiveness of bacteria to heat when it is adequately employed.

Bacteria are much more sensitive to high than to low temperatures, and it is possible to proceed much further downwards than upwards in the scale of temperature, without impairing their vitality. Some will even multiply at zero, whilst others will remain alive when frozen under ordinary conditions.

I will conclude this discourse by briefly referring to experiments recently made with most remarkable results upon the influence of low temperatures on bacterial life. The experiments were conducted at the suggestion of Sir James Crichton-Browne and Professor Dewar. The necessary facilities were most kindly given at the Royal Institution, and the experiments were conducted under the personal supervision of Professor Dewar. The action of liquid air on bacteria was first tested. A typical series of bacteria was employed for this purpose, possessing varying degrees of resistance to external agents. The bacteria were first simultaneously exposed to the temperature of liquid air for twenty hours (about − 190° C.). In no instance could any impairment of the vitality of the organisms be detected as regards their growth or functional activities. This was strikingly illustrated

in the case of the phosphorescent organisms tested. The cells emit light which is apparently produced by a chemical process of intracellular oxidation, and the phenomenon ceases with the cessation of their activity. These organisms therefore furnished a very happy test of the influence of low temperatures on vital phenomena. These organisms when cooled down in liquid air became non-luminous, but on re-thawing the luminosity returned with unimpaired vigour as the cells renewed their activity. The sudden cessation and rapid renewal of the luminous properties of the cells despite the extreme changes of temperature was remarkable and striking. In further experiments the organisms were subjected to the temperature of liquid air for seven days. The results were again *nil.* On re-thawing the organisms renewed their life processes with unimpaired vigour. We had not yet succeeded in reaching the limits of vitality. Prof. Dewar kindly afforded the opportunity of submitting the organisms to the temperature of liquid hydrogen—about $-250°$ C. The same series of organisms was employed, and again the result was *nil.* This temperature is only 21° above that of the absolute zero, a temperature at which, on our present theoretical conceptions, molecular movement ceases and the entire range of chemical and physical activities with which we are acquainted either cease, or it may be, assume an entirely new rôle. This temperature again is far below that at which any chemical reaction is known to take place. The fact then that life can continue to exist under such conditions affords new ground for reflection as to whether after all life is dependent for its continuance on chemical reactions. We, as biologists, therefore follow with the keenest interest Prof. Dewar's heroic attempts to reach the absolute zero of temperature; meanwhile his success has already led us to reconsider many of the main issues of the problem. And by having afforded us a new realm in which to experiment, Prof. Dewar has placed in our hands an agent of investigation from the effective use of which, we who are working at the subject at least hope to gain a little further insight into the great mystery of life itself.

[A. M.]

GENERAL MONTHLY MEETING,

Monday, June 11, 1900.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Charles E. Baxter, Esq.
Cyril Coward, Esq.
August Dupré, Esq. Ph.D. F.R.S.
Lewis Vernon Harcourt, Esq.
W. C. Prescott, Esq.
Mrs. Mary F. Thorne,

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Mr. Harold Swithinbank, for his donation of £50 to the Fund for the Promotion of Experimental Research at Low Temperatures.

The Managers reported, that at their Meeting held this day the following Resolution was unanimously agreed to:—

"The Managers of the Royal Institution of Great Britain, on the occasion of the retirement of Sir Frederick Bramwell from the office of Honorary Secretary, desire to place on permanent record an expression of their high appreciation of the admirable way in which he has performed the duties of that office and of his signal services to the Institution generally.

"Elected a Member of the Royal Institution in 1876, Sir Frederick Bramwell has since then delivered seven Friday Evening Discourses on subjects cognate to that branch of applied science with the progress of which in this country, during the Victorian Era, his name must ever remain honourably associated.

"Having joined the Board of Managers in 1879, he was induced in 1885, notwithstanding professional engagements of the most onerous and responsible character, to undertake the additional burden of the duties of Honorary Secretary to the Institution. For fifteen years these duties have absorbed no inconsiderable proportion of his time, and have been discharged with incomparable energy, business ability and courtesy. Himself a generous patron of the Institution, and foremost to support every project for its advantage, he has been able to suggest improvements in the administration of its property which have added to its material resources. Mainly concerned in the arrangement of the courses of Lectures and Friday Evening Discourses, he has succeeded with no small expenditure of labour in maintaining these at a high level of educational value and in making them attractive and popular and representative of every modern advancement in the arts and sciences. While extending the usefulness of the Institution in every direction, and introducing into it many new members, he has by his genial personality done much to promote smoothness and harmony of working in its several departments.

"The Managers feel that the Royal Institution has been singularly fortunate in having so long enjoyed the services of Sir Frederick Bramwell in the capacity

of Honorary Secretary, and they rejoice to know that although he is no longer to fill that office, they are still to have the benefit of his counsels at their Board. Sir Frederick Bramwell's name is indelibly stamped upon the history of the Royal Institution for the last quarter of the nineteenth century. He will always be gratefully remembered by its Members, but the Managers wish to add to personal remembrance this formal record of their cordial recognition of his merits."

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. 1° Semestre, Vol. IX. Fasc. 8. Classe di Scienze Morali, Storiche, etc. Vol. IX. Fasc. 1, 2. 8vo. 1900.

American Academy of Arts and Sciences—Proceedings, Vol. XXXV. Nos. 10–15. 8vo. 1899-1900.

American Geographical Society—Bulletin, Vol. XXXII. No. 2. 8vo. 1900.

American Philosophical Society—Proceedings, Vol. XXXVIII. No. 160. 8vo. 1899.

Astronomical Society, Royal—Monthly Notices, Vol. LX. No 7. 8vo. 1900.

Bankers, Institute of—Journal, Vol. XXI. Part 5. 8vo. 1900.

Berlin, Royal Prussian Academy of Sciences—Sitzungsberichte, 1900, Nos. 1–22. 8vo. 1900.

Blyth, Sir James, Bart. J.P. M.R.I.—The Microcosm of London. 3 vols. 4to. *n.d.*

Bollack, Léon, Esq. (the Author)—The Blue Language; an International Practical Language. 8vo. 1900.

Boston Public Library—Monthly Bulletin, Vol. V. No. 5. 8vo. 1900.

Boston Society of Medical Sciences—Journal, Vol. IV. No. 8. 8vo. 1900.

British Architects, Royal Institute of—Journal, 3rd Series, Vol. VII. Nos. 13, 14. 4to. 1900.

British Astronomical Association—Memoirs, Vol. VIII. Part 3; Vol. IX. Part 1. 8vo. 1900.
Journal, Vol. X. Nos. 6, 7. 8vo. 1900.

Cambridge Philosophical Society—Proceedings, Vol. X. Part 5. 8vo. 1900.

Camera Club—Journal for May, 1900. 8vo.

Chemical Industry, Society of—Journal, Vol. XIX No. 4. 8vo. 1900.

Chemical Society—Proceedings, Nos. 223, 224. 8vo. 1900.
Journal for May, 1900. 8vo.

Cracovie, l'Académie des Sciences—Bulletin International. 1900, No. 3. 8vo.

Editors—American Journal of Science for May, 1900. 8vo.
Analyst for May, 1900. 8vo.
Anthony's Photographic Bulletin for May, 1900. 8vo.
Astrophysical Journal for May, 1900. 8vo.
Athenæum for May, 1900. 4to.
Author for May, 1900. 8vo.
Bimetallist for April, 1900. 8vo.
Brewers' Journal for May, 1900. 8vo.
Chemical News for May, 1900. 4to.
Chemist and Druggist for May, 1900. 8vo.
Electrical Engineer for May, 1900. fol.
Electrical Review for May, 1900. 8vo.
Electricity for May, 1900. 8vo.
Engineer for May, 1900. fol.
Engineering for May, 1900. fol.
Homœopathic Review for May, 1900. 8vo.
Horological Journal for May, 1900. 8vo
Invention for May, 1900.
Journal of the British Dental Association for May, 1900. 8vo.
Journal of State Medicine for May, 1900. 8vo.

Editors—continued.
Law Journal for May, 1900. 8vo.
Lightning for May, 1900. 8vo.
London Technical Education Gazette for May, 1900.
Machinery Market for May, 1900. 8vo.
Nature for May, 1900. 4to.
New Church Magazine for May, 1900. 8vo.
Nuovo Cimento for March, 1900. 8vo.
Photographic News for May, 1900. 8vo.
Popular Science Monthly for May, 1900.
Public Health Engineer for May, 1900. 8vo.
Science Abstracts, Vol. III. Part 5. 8vo. 1900.
Science Siftings for May, 1900. 8vo.
Telephone Magazine for April, 1900.
Travel for May, 1900. 8vo.
Tropical Agriculturist for May, 1900. 8vo.
Zoophilist for May, 1900. 4to.
Edinburgh, Royal College of Physicians—Reports from the Laboratory, Vol. VII. 8vo. 1900.
Edwards, F. G. Esq. (*the Author*)—Chemistry, an exact Mechanical Philosophy. 8vo. 1900.
Electrical Engineers, Institution of—Journal, Vol. XXIX. No. 145. 8vo. 1900.
Florence, Reale Accademia dei Georgofili—Atti, Vol. XXII. Disp. 3, 4. 8vo. 1899.
Franklin Institute—Journal for May, 1900. 8vo.
Geographical Society, Royal—Geographical Journal for May, 1900. 8vo.
Great Eastern Railway Co.—Guide to Yarmouth and the Broads.
Water Ways of Norfolk and Suffolk. 8vo. 1900.
Holiday Haunts on East Anglian Coast. 1900.
Interesting Dovercourt. 4to. 1899.
Eastern Counties Pictorial Guide 4to.
Harlem, Société Hollandaise des Sciences—Archives Néerlandaises, Serie II. Tome III. Livr. 5. 8vo. 1900.
Imperial Institute—Imperial Institute Journal for May, 1900.
Johns Hopkins University—American Chemical Journal for May, 1900. 8vo.
Leighton, John, Esq. M.R.I.—Journal of the Ex-Libris Society for May, 1900. 4to.
Life-Boat Institution, Royal National—Annual Report for 1900. 8vo.
Linnean Society—Journal, No. 178. 8vo. 1900.
Manchester Literary and Philosophical Society—Memoirs and Proceedings, Vol. XLIV. Part 3. 8vo. 1900.
Manchester Steam Users' Association—Boiler Explosions Acts. Reports, Nos. 1121–1188. 8vo. 1899.
Massachusetts Institute of Technology—Technology Quarterly, Vol. XIII. No. 1. 8vo. 1900.
Mendeleeff, Professor D. Hon. M.R.I. (*the Author*)—Recherches expérimentales sur les oscillations de la Balance. 4to. 1900.
Mexico, Sociedad Cientifica "Antonio Alzate"—Memorias, Tomo XIV. Nos. 1, 2. 8vo. 1899.
Navy League—Navy League Journal for May, 1900. 8vo.
Numismatic Society—Numismatic Chronicle, 1900, Part 1.
Odontological Society—Transactions, Vol. XXXII. No. 6. 8vo. 1900.
Pharmaceutical Society of Great Britain—Journal for May, 1900. 8vo.
Photographic Society, Royal—Photographic Journal for April, 1900. 8vo.
Physical Society—Proceedings, Vol. XVII. Part 1. 8vo. 1900.
Rome, Ministry of Public Works—Giornale del Genio Civile, 1900, Fasc. 3. 8vo.
Royal Irish Academy—Proceedings, Vol. V. No. 4. 8vo. 1900.
Royal Society of Edinburgh—Proceedings, Vol. XXII. No. 7; Vol. XXIII. No. 1. 8vo. 1900.
Transactions, Vol. XXXIX. Part 3. 4to. 1900.

Royal Society of London—Philosophical Transactions, Vol. CXCII. B, Nos. 183, 184. 4to. 1900.
Proceedings, No. 429. 8vo. 1900.
Saxon Society of Sciences, Royal—
Philologisch-Historische Classe—
Berichte, 1900, No. 2. 8vo.
Selborne Society—Nature Notes for May, 1900. 8vo.
Society of Arts—Journal for May, 1900. 8vo.
Swedish Academy of Sciences, Royal—Handlingar, Band XXII. 4to. 1899-1900.
Tacchini, Prof. P. Hon. Mem. R.I. (the Author)—Memorie della Società degli Spettroscopisti Italiani, Vol. XXIX. Disp. 2. 4to. 1900.
Toronto, University of—Studies: Psychological Series, Nos. 2, 3. 8vo. 1899.
United Service Institution, Royal—Journal for May, 1900. 8vo.
United States Patent Office—Official Gazette, Vol. XCI. Nos. 4-7. 8vo. 1900.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1900, Heft 4. 8vo.
Waugh, Rev. F. G. (the Author)—The Athenæum Club and its Associations. (Privately Printed.) 8vo.
Yerkes Observatory—Publications, Vol. I. 4to. 1900.
Zurich, Naturforschende Gesellschaft—Vierteljahrsschrift, 1899, Heft 3, 4. 8vo. 1900.
Neujahrsblatt, 1900. 8vo.

GENERAL MONTHLY MEETING,

Monday, July 2nd, 1900.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

George Livesey, Esq.

was elected a Member of the Royal Institution.

The special thanks of the Members were returned to Dr. Ludwig Mond, F.R.S. for his donation of £150 to the Fund for the Promotion of Experimental Research at Low Temperatures.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Lords of the Admiralty—Nautical Almanac Circular, No. 18. 8vo. 1900.
The British Museum Trustees—Catalogue of the Hindi, Punjabi and Hindustani MSS. By J. F. Blumhardt. 4to. 1899.
Catalogue of Drawings by British Artists. By L. Binyon. Vol. II. 1900.
Facsimiles of Autographs, Fifth Series. fol. 1899.
Catalogue of Cuneiform Tablets, Vol. V. 8vo. 1899.
Handbook to the Coins of Great Britain and Ireland. By H. A. Grueber. 8vo. 1899.
Catalogue of Seals, Vol. VI. 8vo. 1900.
Descriptive List of Syriac and Karshuni MSS. acquired since 1893. By G. Margoliouth. 8vo. 1899.
Index to the Charters and Rolls in Dpt. MSS. Edited by H. J. Ellis and F. B. Bickley. Vol. I. 8vo. 1900.

Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti. Serie Quinta: Rendiconti. 1° Semestre, Vol. IX. Fasc. 10, 11. 8vo. 1900.
American Academy of Arts and Sciences—Proceedings, Vol. XXXV. Nos. 17-19. 8vo. 1900.
Astronomical Society, Royal—Memoirs, Vols LII. LIII. 4to. 1899.
Bankers, Institute of—Journal, Vol. XXI. Part 6. 8vo. 1900.
Boston Public Library—Bulletin for June, 1900. 8vo.
Boston Society of Medical Sciences—Journal, Vol. IV. No. 9. 8vo. 1900.
British Architects, Royal Institute of—Journal, Third Series, Vol. VII. Nos. 15, 16. 4to. 1900.
British Astronomical Association—Journal, Vol. X. No. 8. 8vo. 1900.
Cambridge Philosophical Society—Transactions, Vol. XIX. Part I. 4to. 1900.
Cambridge University Library—Report of the Library Syndicate for 1899. 4to. 1900.
Camera Club—Journal for June, 1900. 8vo.
Chemical Industry, Society of—Journal, Vol. XIX. No. 5. 8vo. 1900.
Chemical Society—Journal for June, 1900. 8vo.
Proceedings, No. 225. 8vo. 1900.
Chicago, Field Columbian Museum—Annual Report, 1898-99. 8vo. 1899.
Chicago, John Crerar Library—Fifth Annual Report. 8vo. 1900.
Cornwall Polytechnic Society, Royal—Annual Report, 1899. 8vo. 1900.
Cutter, E. Esq. and Cutter, J. A. Esq. (the Authors)—On Galvanism: Food Primer; Food Causation of Cataract; Cancer; Diabetes and Locomotor Ataxia. 8vo. 1900.
Editors—Aeronautical Journal for April, 1900. 8vo.
American Journal of Science for June, 1900. 8vo.
Analyst for June, 1900. 8vo.
Anthony's Photographic Bulletin for June, 1900. 8vo.
Athenæum for June, 1900. 4to.
Author for June, 1900. 8vo.
Bimetallist for June, 1900. 8vo.
Brewers' Journal for June, 1900. 8vo.
Chemical News for June, 1900. 4to.
Chemist and Druggist for June, 1900. 8vo.
Electrical Engineer for June, 1900. fol.
Electrical Review for June, 1900. 8vo.
Electricity for June, 1900. 8vo.
Engineer for June, 1900. fol.
Engineering for June, 1900. fol.
Homœopathic Review for June, 1900. 8vo.
Horological Journal for May, 1900. 8vo.
Industries and Iron for June, 1900. fol.
Invention for June, 1900.
Journal of the British Dental Association for June, 1900. 8vo.
Journal of Physical Chemistry for April, 1900. 8vo.
Journal of State Medicine for June, 1900. 8vo.
Law Journal for June, 1900. 8vo.
Lightning for June, 1900. 8vo.
London Technical Education Gazette for June, 1900. 8vo.
Machinery Market for June, 1900. 8vo.
Motor Car Journal for June, 1900. 8vo.
Nature for June, 1900. 4to.
New Church Magazine for June, 1900. 8vo.
Nuovo Cimento for April, 1900. 8vo.
Photographic News for June, 1900. 8vo.
Physical Review for May-June, 1900. 8vo.
Popular Science Monthly for June, 1900. 8vo.
Public Health Engineer for June, 1900. 8vo.

Editors—continued.
Science Abstracts, Vol. III. Part 6. 8vo. 1900.
Science Siftings for June, 1900.
Telephone Magazine for June, 1900. 8vo.
Travel for June, 1900. 8vo.
Tropical Agriculturist for June, 1900.
Zoophilist for June, 1900. 4to.
Florence, Biblioteca Nazionale Centrale—Bolletino, No. 347. 8vo. 1900.
Franklin Institute—Journal for June, 1900. 8vo.
Geographical Society, Royal—Geographical Journal for June, 1900. 8vo.
Geological Society—Quarterly Journal, No. 222. 8vo. 1900.
Geological Literature during 1899. 8vo. 1900.
Imperial Institute—Imperial Institute Journal for June. 1900.
Johns Hopkins University—University Studies, Series XVII. Nos. 6–12; Series XVIII. Nos. 1–4. 8vo. 1899–1900.
University Circular, No. 143. 4to. 1900.
American Chemical Journal for June. 1900. 8vo.
American Journal of Philology, Vol. XXI. Part 1. 8vo. 1900.
Kew Observatory—Report for 1899. 8vo. 1900.
Leighton, John, Esq. M.R.I.—Ex-Libris Journal for June, 1900. 8vo.
Leipzig, Fürstlich Jablonowskische Gesellschaft—Preisschriften, No. XXXV. 8vo. 1900.
London County Council Technical Education Board—Annual Report, 1899–1900. 8vo. 1900.
Madras Government Museum—Bulletin, Vol. III. No. 1. 8vo. 1900.
Madrid, R. Academia de Ciencias—Anuario, 1900. 8vo.
Mexico, Sociedad Cientifica "Antonio Alzate"—Memorias y Revista, Tomo XIV. Nos. 3, 4. 8vo. 1899.
Microscopical Society, Royal—Journal, 1900, Part 3. 8vo.
Nares, Vice-Admiral Sir G. S. K.C.B. F.R.S. (the Conservator)—Report on the Navigation of the River Mersey, 1899. 8vo. 1900.
Navy League—Navy League Journal for June, 1900. 8vo.
New York Academy of Sciences—Memoirs, Vol. II. Part 1. 4to. 1899.
Nova Scotian Institute of Science—Proceedings and Transactions, Vol. X. Part 1. 8vo. 1899.
Odontological Society—Transactions, Vol. XXXII No. 7. 8vo. 1900.
Pharmaceutical Society of Great Britain—Journal for June, 1900. 8vo.
Philadelphia, Academy of Natural Sciences—Proceedings, 1899, Part 3. 8vo. 1899.
Photographic Society, Royal—Photographic Journal for May, 1900. 8vo.
Rome, Ministry of Public Works—Relazione sull' andamento dei servizi. 8vo. 1900.
Rosenthal, Jacques, Esq. (the Author)—Incunabula Typographica. 8vo. 1900.
Royal Society of London—Philosophical Transactions, Vol. CXCIV. A, No. 257; Vol. CXCIII. B. Nos. 185, 186. 4to. 1900.
Proceedings, No. 430. 8vo. 1900.
Saxon Society of Sciences, Royal—
Mathematisch-Physische Classe—
Berichte, 1900, No. 2. 8vo.
Philologisch-Historische Classe—
Berichte, 1900, No. 3. 8vo.
Scottish Meteorological Society—Journal, Third Series, Nos. 15, 16. 8vo. 1900.
Selborne Society—Nature Notes for June 1900. 8vo.
Sidgreaves, Rev. W. (S. J.) (the Director)—Stonyhurst Results, 1899. 8vo. 1900.
Tommasina, T. Esq. (the Author)—Sur l'auto-décohération du charbon. 8vo. 1900.
United Service Institution, Royal—Journal for June, 1900. 8vo.
United States Department of Agriculture—Biological Bulletin, No. 12. 8vo. 1900
North American Fauna, No. 17. 8vo. 1900.
Monthly Weather Review for Feb.–March. 1900. 8vo.

United States Patent Office—Official Gazette, Vol. XC. No. 13; Vol. XCI. Nos. 9–12. 8vo. 1900.
University of London—Calendar, 1900–1901. 8vo. 1900.
Ursini-Scuderi, Professor G. (the Author)—Musicometro. 4to. 1900.
Diagrammi Musicometrici. 4to. 1900.
Verein zur Berförderung des Gewerbfleisses in Preussen—Verhandlungen, 1900, Heft 5. 8vo.
Wilson, W. E. Esq. F.R.S. M.R.I. (the Author)—Astronomical and Physical Researches made at the Observatory, Daramona, Ireland. 4to. 1900.
Yorkshire Archæological Society—Yorkshire Archæological Journal, Part 60. 8vo. 1900.
Zoological Society of London—Proceedings, 1900, Part I. 8vo.

GENERAL MONTHLY MEETING,

Monday, November 5, 1900.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

William Henry Maw, Esq. M. Inst. C.E.
Mrs. Robert Middleton.
William Henry Perkin, Esq. Ph.D. LL.D. F.R.S.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Dr. Frank McClean, F.R.S. for his donation of £50 to the Fund for the Promotion of Experimental Research at Low Temperatures, and to Dr. Rudolph Messel for his present of a Bronze Bust of Sir Humphry Davy.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—
Geological Survey of India—
Memoirs, Vol. XXIX.; Vol. XXX. Part 1. 8vo. 1899–1900.
Palæontologia Indica, Series XV. Himalayan Fossils, Vol. III. Part 1. fol. 1899.
General Report, 1899–1900. 8vo.
The Lords of the Admiralty—Report of Her Majesty's Astronomer at the Cape of Good Hope for 1899. 4to. 1900.
Report of the Astronomer Royal to the Board of Visitors, 1900. 4to.
The Secretary of State for India—Archæological Survey of India: Lists of Antiquarian Remains in His Highness the Nizam's Territories. By H. Cousens. 4to. 1900.
Archæological Survey of India: New Series, Vol. XXIX. 8vo. 1899.
The French Government—Lettres de Catherine de Médicis, publiées par M. le Cte. Baguenault de Duchesse. Tome VII. 1579–1581. 4to. 1899

The Trustees of the British Museum—Catalogue of Cretaceous Bryozoa, Vol. I. 8vo. 1899.
Catalogue of Lepidoptera. Phalænæ, and Plates. Vol. II. 1900. 8vo.
Monograph of Christmas Island (Indian Ocean). By C. W. Andrews. 8vo. 1900.
Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. 1° Semestre, Vol. IX. Fasc. 12; 2° Semestre, Vol. IX. Fasc. 1–7. Classe di Scienze Morali, Storiche, etc. Vol. IX. Fasc. 3–5. 8vo. 1900.
Agricultural Society, Royal—Journal, Vol. XI. Parts 2, 3. 8vo 1900.
American Academy of Arts and Sciences—Proceedings, Vol. XXXV. Nos. 19–27; Vol. XXXVI. Nos. 1–4. 8vo. 1900.
American Association—Proceedings, 48th Meeting (Columbus), 1899. 8vo. 1899.
American Philosophical Society—Proceedings, Vol. XXXIX. Nos. 161, 162. 8vo. 1900.
Anderton, Basil, Esq.—Catalogue of Works on the Fine Arts in the Newcastle-upon-Tyne Public Libraries. 8vo. 1900.
Angstrom, Professor Dr. Knut (the Author)—Intensité de la Radiation Solaire à différentes altitudes. Recherches faites à Ténériffe, 1895–96. 4to. 1900.
Asiatic Society of Bengal—Proceedings, 1900, Nos. 2–8. 8vo.
Journal, Vol. LXVIII. Part 2, No. 4; Vol. LXIX. Part 1, No. 1, Part 2, No. 1. 8vo. 1900.
Asiatic Society, Royal—Journal for July and Oct. 1900. 8vo.
Asiatic Society, Royal (Bombay Branch)—Journal, Vol. XX. No. 55. 8vo. 1899.
Astronomical Society, Royal—Monthly Notices, Vol. LX. Nos. 8, 9. 8vo. 1900.
Bankers, Institute of—Journal, Vol. XXI. Part 7. 8vo. 1900.
Bashforth, Francis, Esq. (the Author)—Experiments made with the Bashforth Chronograph. 8vo. 1900.
Basle, Naturforschende Gesellschaft—Verhandlungen, Band XII. Heft 3. 8vo. 1900.
Berlin, Royal Prussian Academy of Sciences—Sitzungsberichte, 1900, Nos. 23–38. 8vo. 1900.
Bickerton, Professor A. W. (the Author)—Cosmic Evolution. 8vo. 1900.
Boston Public Library—Monthly Bulletin, Vol. V. Nos. 7–10. 8vo. 1900.
Forty-eighth Annual Report, 1899–1900. 8vo.
Boston Society of Medical Sciences—Journal, Vol. IV. No. 10. 8vo. 1900.
British Architects, Royal Institute of—Journal, 3rd Series, Vol VII. Nos. 19, 20. 4to. 1900.
British Astronomical Association—Memoirs, Vol. VIII. Part 4; Vol. IX. Part 2. 8vo. 1900.
Journal, Vol X. Nos. 9, 10. 8vo. 1900.
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GENERAL MONTHLY MEETING,

Monday, December 3rd, 1900.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

John Aungier, Esq.
Edwyn Barclay, Esq.
Rawlinson Tennant Baylis, Esq.
Henry Percy Boulnois, Esq.
Mrs. Ernest Hills,
Richard Taylor, Esq.

were elected Members of the Royal Institution.

The Managers reported that at their Meeting held that day they had elected Dr. Allan Macfadyen Fullerian Professor of Physiology for three years (the appointment dating from January 14, 1901).

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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Victoria Institute—Journal, Vol. XXXII. 8vo. 1900.
Yerkes Observatory, U.S.A.—Bulletin, Nos. 13-15. 8vo. 1900.

WEEKLY EVENING MEETING.

Friday, April 6, 1900.

Sir Frederick Bramwell, Bart., D.C.L. LL.D. F.R.S., Honorary Secretary and Vice-President, in the Chair.

Professor Dewar, M.A. LL.D. D.Sc. F.R.S. *M.R.I.*

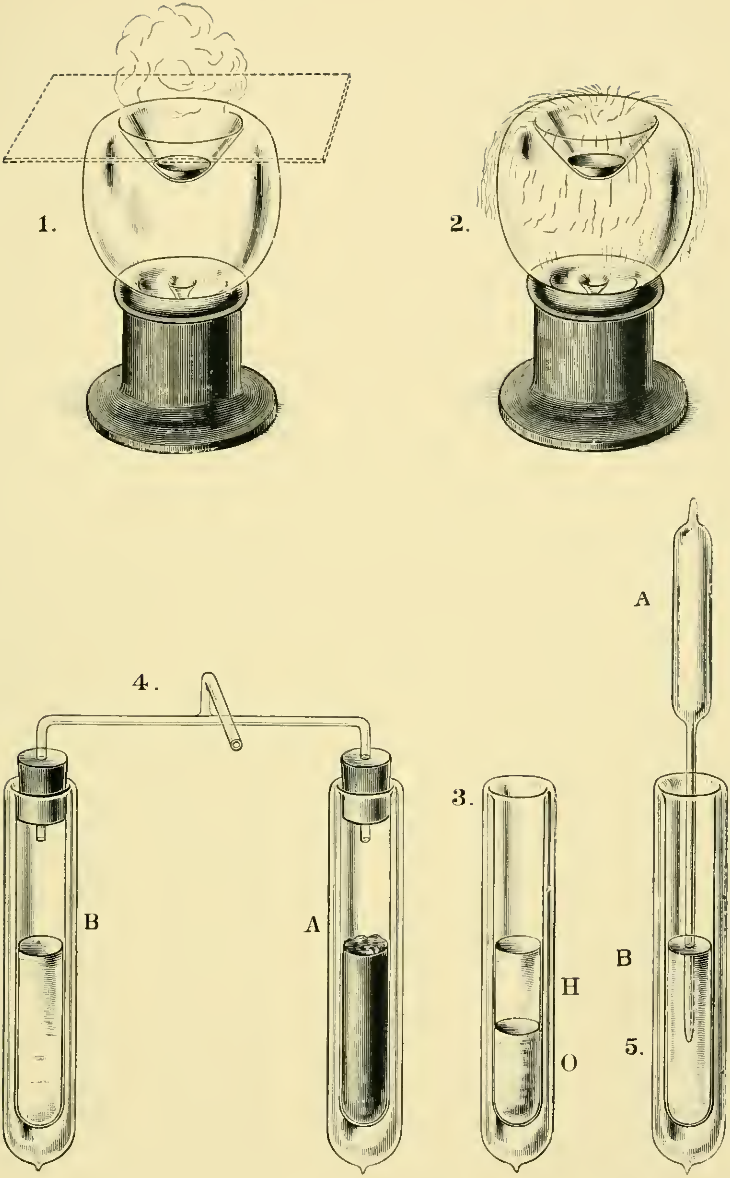
Solid Hydrogen.

Before proceeding to discuss the immediate subject of this lecture, it will be advisable to contrast experimentally some of the properties of hydrogen, nitrogen and oxygen in the liquid condition. The two vacuum cups (Figs. 1 and 2) are charged half full respectively with liquid hydrogen and liquid air. When the cup containing the liquid air is placed in front of the electric lamp, the image thrown on the screen reveals the continual overflow of a dense vapour round the outer walls of the vessel. The saturated vapour coming from the steady ebullition of liquid air is three times denser than the free air of the room, and the result is it falls through that air just as if it were a dense gas like carbonic acid or ether vapour. To observe this phenomenon, the vacuum cup must be shallow, otherwise the vapour gets heated up before reaching the mouth of the vessel, and no difference of density in the air coming off is observed. We will now project the image of the cup containing liquid hydrogen, covered loosely in this case with a glass plate, upon the screen; here, no heavy vapour escaping round the sides is visible. The vapour of the boiling liquid hydrogen has a density nearly equal to the air of the room, but as it gets very rapidly heated up by the glass cover the gas that is escaping is seen to rise in air like any light gas. On now removing the glass plate, a very different phenomenon is observed, which contrasts markedly with the behaviour of the liquid air in the former vessel. The cup and the air above is filled with a dense surging snowstorm of solid air; the air coming in contact with the excessively cold hydrogen vapour is suddenly solidified, and a part of it falls into the liquid hydrogen, causing more rapid evaporation, thereby intensifying the cloud condensation. After the mist has disappeared and all the liquid hydrogen gone, the cup contains a white deposit of solid air. This shortly melts, and on allowing the nitrogen to boil off, the presence of oxygen can be shown by the ignition of a red-hot splinter of wood. Such effects are easily understood when we remember that the boiling-point

of hydrogen is proportionally as much below the boiling-point of air as the latter is below the ordinary temperature of this room.

In order to observe the individual behaviour of the constituents of the air at temperatures below their ordinary boiling-points it is advantageous to place liquid nitrogen and oxygen in separate vacuum vessels, so connected that they may be simultaneously exhausted, as is represented in Fig. 4. On starting the air-pump both liquids enter into rapid ebullition. As the exhaustion gets higher the temperature of each liquid gets lower and lower, and if the melting-point is finally reached in either liquid it must shortly begin to solidify. This condition is quickly brought about in the case of the vessel A containing the liquid nitrogen, which passes rapidly into the condition of a dense white snow; but no amount of time spent in maintaining a good exhaustion (5 to 10 mm. pressure) has any effect in changing the liquid condition of the oxygen in B. Oxygen in fact remains liquid at temperatures where nitrogen is solid. The snow of solid air produced by the evaporation of liquid hydrogen, in the previous experiment, might thus be made up of solid nitrogen and a liquid rain of oxygen. To show that the temperature of boiling hydrogen solidifies oxygen, some of the latter liquid is placed in a vacuum test-tube O (Fig. 3), and liquid hydrogen H is poured on its surface, when the liquid oxygen is quickly transformed into a clear blue solid ice. Both oxygen and nitrogen, and we shall see later, hydrogen can be changed into the condition of transparent ice as well as into the snowy state. A closed vessel filled with any gas at atmospheric pressure, of such a form that a portion of the surface in the shape of a narrow quill tube, can be cooled in boiling liquid hydrogen like B, Fig. 5, shows condensation of the gas to the solid state; the only exceptions being helium and hydrogen itself. Here are two vessels of the same shape as A, B, Fig. 5. The first contains helium showing no condensation when the part B is cooled; the second is filled with hydrogen, which equally shows no change of state under the conditions of the experiment. It is easy, however, to make the hydrogen vessel show liquefaction. For this purpose the experiment with the hydrogen is repeated, only before doing so the part A is heated to about 300° C. over a Bunsen burner, in order to increase the pressure of gas in the interior to above two atmospheres. Now liquefaction is seen to take place with great facility. No change is produced by similarly increasing the pressure in the helium vessel.

The extraordinary command liquid hydrogen gives us over the transition of state in matter may be best illustrated by the use of a new kind of cryophorus. Wollaston's celebrated instrument operates by forcing the evaporation of water in a closed vessel by condensing its vapour in a part of the receiver at a distance from the fluid, thereby causing a lowering of temperature in the latter until freezing takes place. Hence the name cryophorus or cold-bearer. Instead of using water we may now show that the same principle may be applied to the solidification of nitrogen at a distance, instead of water. The sole difference in this case is that the



FIGS. 1, 2, 3, 4, 5.

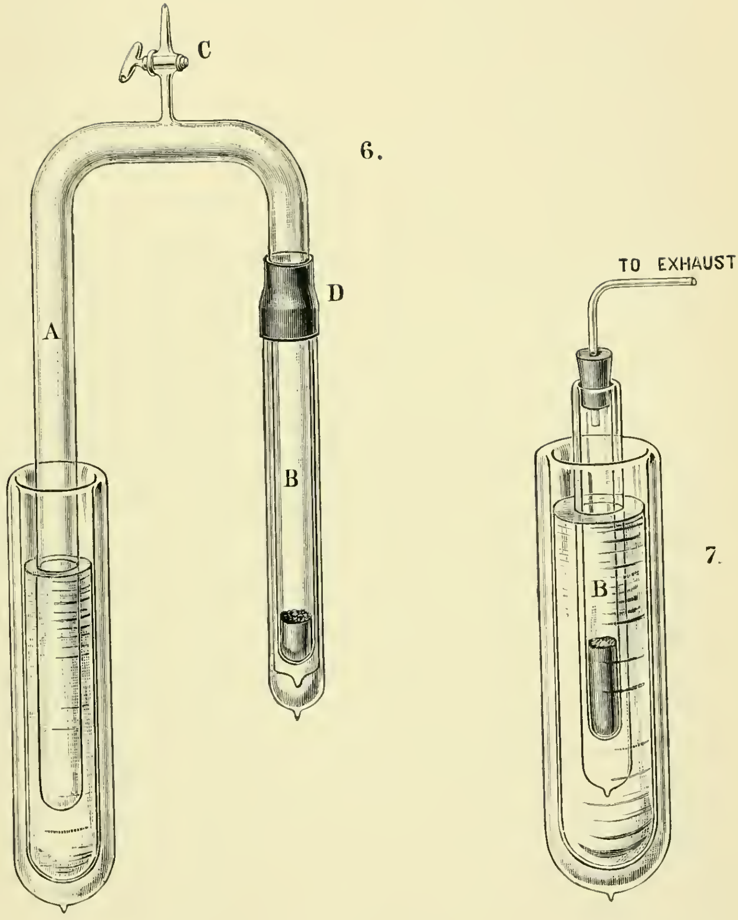
liquid nitrogen must be isolated from the influx of heat by being placed in a vacuum vessel, and the condensation of its vapour must be effected by the use of liquid hydrogen.

No boiling-out operation is necessary with the cryophorus we are about to use. The apparatus is shown in Fig. 6. The vacuum tube B contains liquid nitrogen. It is fitted on by an indiarubber joint to a wide piece of glass tubing doubly bent at right angles, A D; and in order to allow the gas from the boiling liquid to escape before the experiment begins an aperture C is left which can be closed with a stop-cock. On closing C, and inserting a part of the tube A into a vessel containing liquid hydrogen, the gas within is condensed, and thereby the pressure of the vapour in the interior of the vessel is reduced, forcing the liquid nitrogen in the other part of the apparatus to boil with great violence. In a few minutes the temperature of the nitrogen is so much reduced that it passes into the solid state. Many other liquid gases might be used to replace the nitrogen in this experiment. In making a selection, however, it is necessary to take only those bodies that possess a reasonably high tension of vapour at the melting point. The process would not succeed easily with a substance like oxygen, that has no measurable tension of vapour in the solid condition.

In the autumn of 1898, after the production of liquid hydrogen was possible on a small scale, its solidification was attempted by boiling under reduced pressure. At this time, to make the isolation of the hydrogen as effective as possible, the liquid was placed in a small vacuum test-tube, placed in a larger vessel of the same kind. Excess of hydrogen partly filled the annular space between the two vacuum vessels. On diminishing the pressure by exhaustion the evaporation was mainly thrown on the liquid hydrogen in the annular space between the tubes. In this arrangement the outside surface of the smaller tube was kept at the same temperature as the inside, so that the liquid hydrogen for the time was effectually guarded from influx of heat. With such a combination the liquid hydrogen was evaporated under diminished pressure, yet no solidification took place. Seeing experiments of this kind required a large supply of the liquid, other problems were attacked, and further attempts in the direction of producing the solid for the time abandoned. During the course of the present year many varieties of electric resistance thermometers have been under observation, and with some of these the reduction of temperature brought about by exhaustion was investigated. Thermometers constructed of platinum and platinum-rhodium (alloy) were only lowered $1\frac{1}{2}°$ C. by exhaustion of the liquid hydrogen, and they all gave a boiling-point of $-245°$ C., whereas the reduction in temperature by evaporation *in vacuo* ought to be $5°$ C., and the true boiling-point from $-252°$ C. to $-253°$ C. In the course of these experiments it was noted that almost invariably a slight leak of air occurred which became apparent by its being frozen into an air-snow in the interior of the vessel, where it met the cold vapour of hydrogen. When conducting wires covered with silk have to pass

through indiarubber corks it is very difficult at these excessively low temperatures to prevent leaks, when corks get as hard as a stone, and cements crack in all directions. The effect of this slight air leak on the liquid hydrogen when the pressure got reduced below 60 mm., was very remarkable, as it suddenly solidified into a white froth-like mass like frozen foam. My first notion was that this body might be a sponge of solid air containing liquid hydrogen. The ordinary solid air obtained by evaporation *in vacuo* is a magma of solid nitrogen containing liquid oxygen. The fact, however, that this white solid froth evaporated completely at the low pressure without leaving any substantial amount of solid air led to the conclusion that the body after all must be solid hydrogen. This surmise was confirmed by observing that if the pressure, and therefore the temperature, of the hydrogen was allowed to rise, the solid melted when the pressure reached about 55 mm. The failure of the early experiment must then have been due to supercooling of the liquid, which presumably is prevented by contact with metallic wires and traces of solid air. On the other hand, it is possible the pressure under which the ebullition took place might never have been low enough to reach the solid state.

For the lecture demonstration of solid hydrogen the apparatus may be most conveniently arranged as is shown in Fig. 7. The small vacuum tube B, after being filled with liquid hydrogen, is immersed in a larger vessel of the same kind filled with liquid air. By this arrangement the rate of the liquid hydrogen evaporation is so much diminished that it does not exceed that of liquid air in the same vessel when used in the ordinary way. On gradually applying exhaustion to the liquid hydrogen it is forced from its effective heat isolation to pass to a lower temperature, and when the exhaustion reaches 50 mm. the mass suddenly begins to solidify into a froth-like material. In order to ascertain the appearance of the hydrogen, made by cooling the liquid produced in a hermetically closed vessel the following experiment was arranged. A flask of about a litre capacity, to which a long glass tube was sealed, A B, Fig. 5, was filled with pure dry hydrogen and sealed off. The lower portion B of this tube was calibrated. It was surrounded with liquid hydrogen placed in a vacuum vessel arranged for exhaustion. As soon as the pressure of the boiling hydrogen got well reduced below that of the atmosphere, perfectly clear liquid hydrogen began to collect in the tube B, and could be observed accumulating until the liquid hydrogen surrounding the outside of the tube suddenly passed into a solid white foam-like mass, almost filling the whole space. As it was not possible to see the condition of the hydrogen in the interior of the tube B when it was covered with a large quantity of this solid, the whole apparatus was turned upside down in order to see whether any liquid would run down from B into the flask A. Liquid did not flow down the tube, so the liquid hydrogen with which the tube was partly filled must have solidified. By placing a strong light on the side of the vacuum test-tube opposite the eye, and maintaining the exhaustion at about 25 mm., gradually



Figs. 6, 7.

the hydrogen froth became less opaque, and the solid hydrogen in the tube B was seen to be a transparent ice, but the surface looked frothy. This fact prevented the solid density from being determined, but the maximum fluid density has been approximately ascertained. This was found to be 0·086, the liquid at its boiling-point having the density 0·07. The solid hydrogen melts when the pressure of the saturated vapour reaches about 55 mm. In order to determine the temperature of solidification two constant volume hydrogen thermometers were used. One at 0° C. contained hydrogen under a pressure of 269·8 mm., and the other under a pressure of 127 mm. The mean temperature of the solid was found to be 16° absolute under a pressure of 35 mm. All the attempts made to get an accurate electric resistance thermometer for such low temperature observations have been so far unsatisfactory. Now that pure helium is definitely proved to be more volatile than hydrogen, this body, after passing through a spiral glass tube immersed in solid hydrogen to separate all other gases, must be compared with the hydrogen thermometer. Taking the boiling-point as 21° absolute under 760 mm., and the similar value under 35 mm. is 16° absolute, then the following approximate formula for the vapour tension of liquid hydrogen below one atmosphere is derived :—

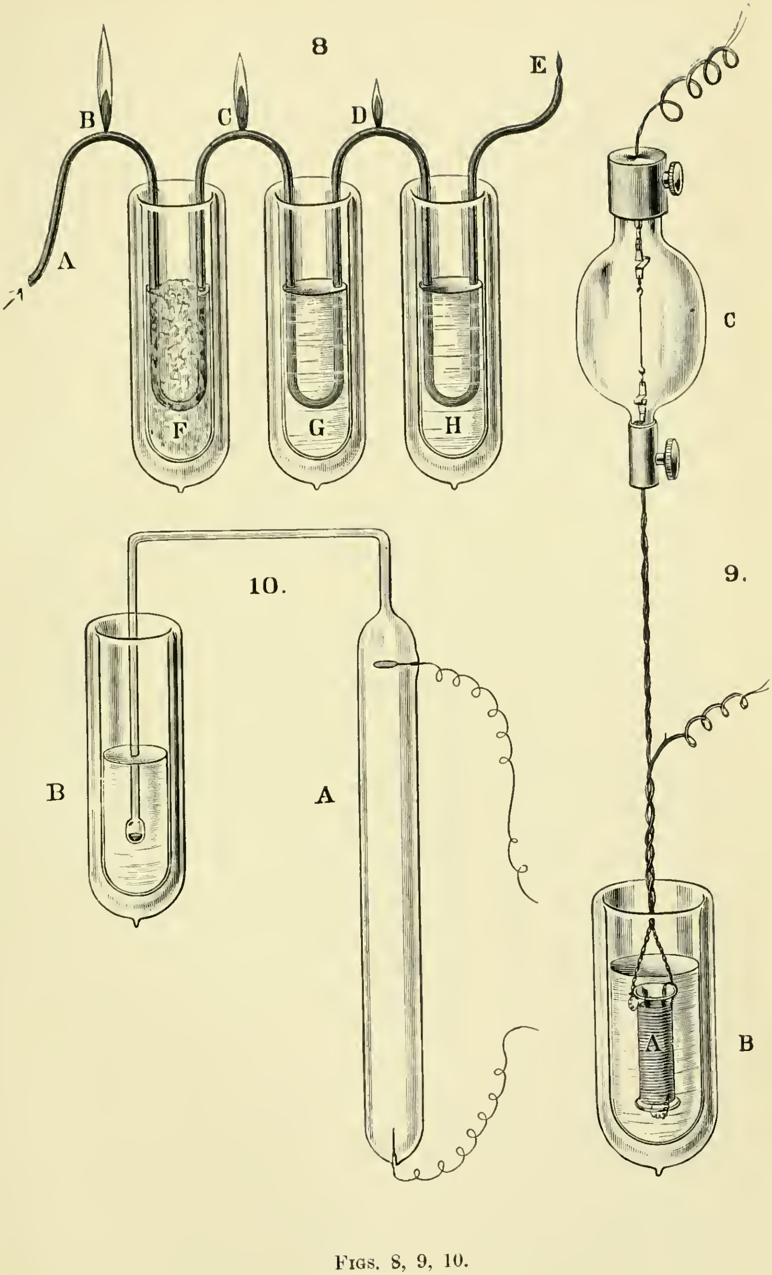
$$\log p = 6 \cdot 7341 - 83 \cdot 28 \, / \, T \text{ mm.},$$

where T is the absolute temperature, and p the pressure in mm. This formula gives for 55 mm. a temperature of 16·7° absolute. The melting-point of hydrogen must therefore be about 16° or 17° absolute. It has to be noted that the pressure in the constant volume hydrogen thermometer, used to determine the temperature of solid hydrogen boiling under 35 mm., had been so far reduced that the measurements were made under from one-half to one-fourth the saturation pressure for the temperature. When the same thermometers were used to determine the boiling-point of hydrogen at atmospheric pressure, the internal gas pressure was only reduced to one-thirteenth the saturation pressure for the temperatures. The absolute accuracy of the boiling-points under diminished pressure must be examined in some future paper. The practical limit of temperature we can command by the evaporation of solid hydrogen is from 14° to 15° absolute. In passing it may be noted that the critical temperature of hydrogen being 30° to 32° absolute, the melting-point is about half the critical temperature. The melting-point of nitrogen is also about half its critical temperature. The foam-like appearance of the solid when produced in an ordinary vacuum vessel is due to the small density of the liquid, and the fact that rapid ebullition is substantially taking place in the whole mass of liquid. The last doubt as to the possibility of solid hydrogen having a metallic character has been removed, and for the future, hydrogen must be classed among the non-metallic elements.

All solid bodies by themselves make very unsatisfactory cooling agents unless we can use them to cool some liquid. Now, with solid hydrogen we can cool no liquid other than hydrogen, so that, for

effective cooling, we must use the liquid just above its freezing-point, which is about 16°. It will, however, take a long time to exhaust the wide field of investigation which the use of liquid hydrogen opens up; so we may proceed to illustrate some of its further applications. In former lectures the relation of electrical resistance to temperature has been discussed, and it was experimentally demonstrated that the curves of resistance of the pure metals all pointed to this quality disappearing or becoming exceeding small at the absolute zero. This fact has been confirmed, even with the most highly conducting metals, down to the lowest temperature we can command. The experiment illustrated in Fig. 9 shows to an audience the diminution of resistance of pure copper wire when cooled in liquid hydrogen, in contrast to liquid air. An incandescent lamp C has been placed in circuit with a fine coil of copper wire A, immersed in liquid air, the resistances being so adjusted that the filament in C is just visible when the current passes under these conditions. Now, on removing the coil from the liquid-air vessel and placing it in another similar vessel filled with liquid hydrogen, a great increase in the brilliancy of the lamp is observed. As a matter of fact, the sample of copper has its resistance in liquid air reduced to about one-twentieth of what it is at the temperature of melting ice, whereas in liquid hydrogen the resistance is reduced to one-hundredth of the same amount. In other words, the resistance in liquid hydrogen is only about one-fifth of what it is in liquid air. The interesting point, however, is that theoretically we should infer, from experiments made at higher temperatures, that at a temperature of $-223°$ C. the copper should have no resistance, or it should have become a perfect conductor. As this is not the case, even at the temperature of $-253°$, we must infer that the curve co-relating resistance and temperature tends to become asymptotic at the lowest temperatures.

Liquid hydrogen is a most useful agent for the production of high vacua and for the separation of gases from air that may be more volatile than oxygen or nitrogen. An experiment illustrating the production of a high vacuum is shown in Fig. 10 where A is the large electric discharging tube to which has been attached a narrow glass tube twice bent at right angles, and terminating in a bulb at the end for immersion in the liquid hydrogen. The rapidity with which the vacuum is attained is shown by the rate at which the striation in the tube changes and the phosphorescent state supervenes. Another rough illustration of the application of cold to effect the separation of a complex mixture of gases is shown in Fig. 8. Coal-gas is passed in succession through the U-tubes F, G and H made of ordinary gas-pipe, having small holes at B, C, D and E, in order that a flame may be produced before and after each vessel is passed. Each of the U-tubes is placed in a vacuum vessel, and the first cooling substance the gas, in its transit meets is solid carbonic acid in F, then liquid air in G, and finally liquid hydrogen in H. At the temperature of the carbonic acid bath, all the easily condensable hydro-



Figs. 8, 9, 10.

carbons separate, and consequently the flame C is less luminous than B. The liquid air bath condenses the ethylene and a large part of the marsh gas, and allows the carbonic oxide and the hydrogen to pass through so that flame D is less luminous than C. Finally, after the liquid hydrogen bath, nothing escapes condensation but free hydrogen, the carbonic oxide and any marsh gas being solidified; the result is, the flame E is almost invisible.

A really practical application of liquid hydrogen is the purification of helium obtained from the gases emitted by the mineral springs of Bath. Although the helium only amounts to one-thousandth part by volume—the nine hundred and ninety-nine being chiefly nitrogen—yet the low temperature method of separation can be successfully applied.

Now that we know definitely the approximate values of some of the more important physical constants of liquid hydrogen, it is interesting to look back at the values that have been deduced—say for such a constant as the density—by various workers using entirely different methods. The following table gives some of the more important values of the density of hydrogen under the different conditions in which it enters into organic and inorganic bodies.

Density of Hydrogen in Different Conditions.

Kopp	Organic bodies	0·18
Amagat	Limit of gaseous compression	0·12
Wroblewski	Van der Waals' equation	0·027 (critical density)
Van der Waals	Superior limit of density	0·82
Graham	Palladium alloy	2·0
Dewar	Palladium alloy	0·63
Dewar	Liquid hydrogen at B.P.	0·07

My density at the boiling-point agrees substantially with that which can be deduced from Wroblewski's form of the Van der Waals' equation. The deduced densities of Kopp for organic bodies and Amagat for gaseous compression are both about the same value, and may be taken as a mean to be twice the observed density of hydrogen in the liquid state. The conclusions of Graham and myself, touching the density of the hydrogen in the so-called alloy of palladium, must be regarded as altogether exceptional. Even my value would exceed the density of the stuff constituting the real gas molecule, according to the theory of Van der Waals. In order to harmonise the palladium hydrogen results with those deduced from the study of organic bodies, we must assume that, during the formation of the so-called hydrogenium, a condensation of the palladium sufficient to increase its density by one-fifth must take place. This is by no means an unreasonable hypothesis. The mode of determining the density of hydrogen at its melting-point has been previously described and found to be 0·086. In the same way the approximate values for the densities of nitrogen and oxygen at their melting-points have been found, their respective values being 1·07 and 1·27. The following table shows the comparison between my results and those given by Amagat for high gaseous compressions :—

Densities.

—	Liquid Melting-point.	Gas. 3000 Atmospheres.	Limiting Value, 4000 Atmospheres.
Hydrogen	0·086	0·097	0·12
Nitrogen	1·1	0·833	0·12
Oxygen	1·27	1·127	1·25

It will be noted that the density of gaseous hydrogen at 3000 atmospheres is actually greater than the maximum density of the liquid state; but neither in the case of nitrogen nor oxygen does the density at the same pressure reach the fluid density. Amagat's limiting value for oxygen under 4000 atmospheres would, however, be almost identical with mine.

During the course of my inquiries sufficient data have been accumulated to construct Waterston Formulæ giving the approximate densities of liquid hydrogen, nitrogen and oxygen in each case through a wide range of temperature. The equation for each substance is given in the following table:—

Liquid Atomic Volumes.

$$\text{Hydrogen} = 23{\cdot}3 - 8{\cdot}64 \log (32^\circ - t)$$
$$\text{Nitrogen} = 30{\cdot}0 - 11{\cdot}00 \log (127^\circ - t)$$
$$\text{Oxygen} = 32{\cdot}6 - 10{\cdot}22 \log (155^\circ - t)$$

		Absolute Zero.	Observed at Melting-point.
1.	Atomic volume of hydrogen	= 10·3	11·7
2.	" "		
3.	" nitrogen	= 12·8	13·1
4.	" oxygen	= 10·20	12·6

From these formulæ we find the respective hypothetical atomic volumes of hydrogen, nitrogen and oxygen at the absolute zero to be 10·3; 12·8 and 10·2. My observed minimum fluid volumes were 11·7; 13 and 12·6. The coefficients of expansion of the liquids, taken in the same order at their respective boiling-points are 0·024; 0·0056 and 0·0046. Thus liquid hydrogen has a coefficient of expansion five times greater than that of liquid oxygen. Further inquiry will enable the constants in these equations to be determined with greater accuracy. In the meantime, however, they give us general ideas of the order of magnitude of the quantities involved.

I have to thank Mr. Robert Lennox for efficient aid in the arrangement and execution of the difficult experiments you have witnessed. Mr. Heath has also heartily assisted in the preparations.

[J. D.]

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Royal Institution of Great Britain.

GENERAL MONTHLY MEETING,

Monday, February 4, 1901.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. F.R.S., President, in the Chair.

John Boldero, Esq.
John Y. Buchanan, Esq. M.A. F.R.S.
Augustus Littleton, Esq.
John Macfadyen, Esq.
Arthur William Reed, Esq. B.A.
Major John Middleton Rogers,
Dyson Weston, Esq.
Thomas Tellyer Whipham, M.D.

were elected Members of the Royal Institution.

The special thanks of the Members were returned to Sir Frederick Abel, Bart., K.C.B., for his donation of £50, and to Professor Dewar for his donation of £50, to the Fund for the Promotion of Experimental Research at Low Temperatures.

The following Address to the King was read and approved, and authorised to be signed by His Grace THE DUKE OF NORTHUMBERLAND, K.G., the President, on behalf of the Members :—

TO THE KING'S MOST EXCELLENT MAJESTY.

The humble Address of the Members of the Royal Institution of Great Britain.

MOST GRACIOUS SOVEREIGN,

We, your Majesty's most loyal and dutiful subjects, the Members of the Royal Institution of Great Britain, established for the promotion of Science, Literature and the Arts, respectfully crave leave to approach your Sacred Person with expressions of our heartfelt sorrow at the loss sustained by your Majesty, the Royal Family, the Empire, and the World at large, in the death of your Majesty's revered Mother, our late most gracious and beloved Sovereign and Patron of this Institution: and also to testify our unfeigned congratulations upon your Majesty's Accession to the Throne, as well as our sincere attachment to your Royal Person.

That your Majesty may long live in Prosperity and Happiness over a free and affectionate people is the sincere wish and earnest prayer of

Your Majesty's Loyal and Dutiful Subjects,
The Members of the Royal Institution of Great Britain.

The President, in moving a Vote of Condolence on the Death of

Her late Majesty, remarked that Her Majesty had been for the whole of her reign the Patron of this Institution. She had shown her keen appreciation of the value of scientific study and research by the honours she had conferred from time to time on those who followed them. There was one signal act of kindness which should be recorded in connection with this Institution, namely that she gave to Professor Faraday rooms in Hampton Court Palace, and that when he found himself unable to meet the expense of necessary repairs she took it upon herself. Her husband and sons had been frequent attendants at our Lectures, but perhaps the highest debt of gratitude which scientific men owed to our late Sovereign was that she had by her wisdom and skill secured them through a prolonged reign the utmost freedom and tolerance of thought and action, combined with complete order and stability of government, a condition of things most favourable to the pursuit of Science. He was sure that the Members of the Royal Institution shared to the fullest extent the regard which the whole of her people, from the lowest to the highest, had evinced to her memory.

The following Resolution, passed by the Managers, was read:—

Resolved, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution in the decease of Mr. BASIL WOODD SMITH, F.R.A.S. F.S.A.

He became a Member of the Royal Institution in 1853, and was elected a Visitor in 1876, and a Manager in 1889. He always took a warm interest in the welfare of the Institution, and for many years as Manager and Vice-President he rendered valuable assistance in the transaction of the business affairs and concerns of the Institution.

The Managers further desire to offer to the family the expression of the most sincere sympathy with them in their bereavement.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

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WEEKLY EVENING MEETING,

Friday, February 8, 1901.

Sir William Crookes, F.R.S., Honorary Secretary and Vice-President, in the Chair.

Professor G. H. Bryan, Sc.D. F.R.S.

History and Progress of Aerial Locomotion.

The early history of artificial flight—or, more generally, of attempts to solve the problem of *directed* locomotion through the air—takes us back to the Egyptian figures of men with wings, closely resembling those of a modern gliding machine, and the legend of Dædalus, who, as the inventor of sailing ships, was accredited with having attached wings to himself. In succeeding ages we have numerous records—some of purely fantastic and visionary ideas—of flying-machines, such as the design of the French eighteenth century novelist, Rétif de la Bretonne, and that of the Portuguese Lourenco; others, of machines constructed, but which proved unsuccessful, such as those of Besnier and the Marquis de Bacqueville; and last, but not least, several descriptions of early attempts at gliding from a height, such as the experiments of Dante of Perugia, and of the Turk described by Busbequius, which so closely resemble modern gliding experiments as to be worthy of credence. Montgolfier's discovery of the balloon, by rendering it possible to ascend in the air, diverted attention to another method by which to seek the solution of the problem of directed aerial locomotion, namely, by adopting the principle of the fish rather than that of the bird, and experimenting with machines which, by the addition of a gas-bag, are made lighter than the air they displace. Now, the difficulty of propelling a balloon through the air is due to the fact that, in order to rise in the air, the balloon and its load must be lighter than the volume of air they displace; and since air is, in round numbers, one-thousandth of the density of water, it follows that if a balloon and a ship are to carry the same total weight, the balloon must displace one thousand times the volume of air that the ship does of water. The large size of balloons renders it very difficult to propel them through the air at the speed necessary for travelling in the face of a wind such as is commonly blowing in fair weather. There can be no doubt that Count Zeppelin's experiments do actually furnish a solution of the problem of directed locomotion through the air; the relative velocity attained, viz. 17 miles

an hour, being sufficient to enable the machine to be driven against the amount of wind encountered on a fairly calm day, and the machine having been steered at will and brought back to the starting-point. The only previous record of a similar kind is that of Messrs. Renard and Krebs, who, in 1885, performed a journey in their balloon 'La France,' and returned to the starting-point again. But the speed of that balloon has been variously described by writers as four and fourteen miles an hour, and, unless the latter figure is the correct one, it is certain that the balloon could never hold its own against the light winds blowing even in ordinary calm weather. The recent experiments of Santos Dumont have excited considerable interest; but we have not, so far, seen any records of performances by this machine, beyond the maintenance, for half a minute, of a relative speed of seven miles an hour in the face of a wind blowing at four miles an hour—giving for this brief interval a forward velocity of three miles an hour.

The chief advantages of the Zeppelin balloon are (1) its division into seventeen compartments, and (2) the distribution of the load at two points instead of at the centre. The former feature has not only the advantage of minimising the danger arising from escape of gas—in particular as the result of bullet shots in times of war—but, what is more important, it prevents the gas from "wobbling about" (for no other phrase is so expressive) in the interior, an action which would cause the envelope to flap, absorbing energy, and greatly increasing the resistance of the air. The agreement between the speed actually obtained and that previously calculated from theory was very close indeed. From what has been said above, the balloon, which carried a load of about ten tons, must have displaced a volume of air somewhere about equal to the volume of water displaced by a ship of 10,000 tons, and it is not difficult from the illustrations to realise that such was the case. In ignorance of this simple property, many prospectuses are issued of companies for promoting air-ships, quite impracticable in reality; and a diagram was exhibited on the screen of a kind of aerial tramcar, filled with passengers, supported by three cigar-shaped balloons smaller than the tramcar itself. Another example of want of knowledge on the part of promoters of air-ship syndicates was exhibited on the screen, in the form of a design of an actual patent specification, drawn up not many years ago, for seats on such "aerial tramcars," containing cavities filled with gas to help raise the machine. Remembering that a cubic foot of air only weighs about an ounce, the lifting power would only amount to roughly about an ounce per cubic foot of cavity, and the diminution of weight would be far less than that obtained by making the seats of light bamboo. To obtain an appreciable effect, the cavities must be filled with what does not exist, namely, "something which is lighter than nothing, and has a tendency upwards."

Passing from the mechanically-propelled balloon to the question

of the flying machine proper, the study of artificial flight by means of wings, aeroplanes, or aerocurves and propellers, involves the investigation of (1) the laws of aerial resistance of plane and curved surfaces and screws; (2) the lightest possible motors and sources of energy; (3) the conditions of balance and stability of a body in free air, supported by wings or aerocurves. The first of these three directions of investigation having been dealt with by Lord Rayleigh, in his lecture last year, we shall devote our chief attention to the second and third. We may however notice, in passing, Langley's experiments on the resistance of planes and curved surfaces, which confirmed Duchemin's law, and showed that the greater the horizontal velocity of a surface moving through the air the less is the horse-power necessary to support a given weight by it; Phillips's Wealdstone experiments, on the advantages of a number of narrow superposed planes (resembling a Venetian blind), over a single wide plane of equal area, and Fitzgerald's investigation of the flapping flight of aeroplanes.

With regard to the question of the motor, the experiments of Lilienthal, Pilcher and Chanute, agree in indicating that about two horse-power would be required to convert their downward glides into horizontal or slightly upward flights; and Chanute estimates the possible maximum weight of the motor required to drive a machine capable of lifting one man off the ground, at 4 lbs. per horse-power with vertical screws, 8 lbs. with flapping wings, and 14 lbs. with propeller-driven aerocurves. Da Pra, from theoretical considerations, allows 15 kilogrammes per horse-power as the possible weight of the motor in his design. Regarding the actual weight of motors, as long ago as 1868 Stringfellow gained a prize for a steam-engine and boiler weighing only 13 lbs., and giving rather over one horse-power. Maxim's gigantic machine had an engine weighing 8 lbs. per horse-power. Langley is reputed to have constructed a one horse-power engine weighing 7 lbs.; Hargreave one weighing 10 lbs. It should, however, be observed that the heavier the machine the greater is the ratio of horse-power to total weight necessary to render flight possible, and hence we see how it is that birds are able to fly although they themselves weigh 150 to 200, and their muscles alone 5 to 20 lbs. per horse-power. For models similar in form, but of different dimensions, the law commonly assumed is that the horse-power per lb. of weight is proportional to the square root of the linear dimensions, if the weight itself (as is usually the case) is proportional to the cube of the linear dimensions.

Thus, while the experimenters who attempted to fly, in the eighteenth and early part of the nineteenth century, by their own muscular power, were attempting an impossible task, owing to the fact that the rate at which a man is capable of working (say about $\frac{1}{3}$ horse-power) is far below the amount required for flight, the difficulties arising from the question of horse-power in proportion to weight can no longer be regarded as insuperable. But, even if these purely mechanical difficulties are overcome, we are far from having solved the problem of

flight. There still remain the problems of starting off the ground and landing safely again, which become more difficult as the size of the machine is increased. Still more intricate is the question of balance and stability. The very fluctuations of wind-velocity, from which sailing birds may not improbably derive their supply of energy, vastly increase the difficulty of artificial flight. Langley speaks of wind-velocities suddenly rising to forty miles an hour, and equally suddenly dropping to a dead calm. With such gusts to contend against, the danger of a machine suddenly capsizing is a very serious one. Mr. Wenham relates that, thirty years ago, a gliding model of his, which flew well from a housetop, was dropped by Glaisher in one of his balloon ascents, but, after gliding twelve yards, it tripped and whirled over and over till it reached the earth.

Our knowledge of the conditions of balance and stability is largely deduced from experiments in gliding from a height under gravity. In 1864, Captain Le Bris constructed an "artificial albatross," with which he is reported to have made successful glides up to 600 feet; but he broke his leg, and his funds became exhausted, so that the experiments had to be discontinued. In recent times, great advances in the art of gliding have been made by Lilienthal in Germany, Pilcher in England, and Chanute and Herring in America. The death of the two former, as the result of accidents with their machines, will no doubt deter others from working on the same lines. It is therefore desirable to notice in detail, the points of difference between the methods adopted by Chanute and Herring, and those of their less fortunate predecessors.

Both Lilienthal and Pilcher used broad wings; the first in his later experiments using a machine with two superposed surfaces, and the second always using single-surface machines. Both were dependent entirely on the movements of their body for counteracting the tendency to overturn, produced by gusts of wind; and, under such conditions, gliding was of necessity an art requiring considerable gymnastic agility.

Mr. Octave Chanute commenced his gliding experiments, in 1896, by constructing an "albatross machine," based on the design of Le Bris; but the large size of the machine necessitated its being flown from a properly constructed track, and glides could only be made when the wind was blowing in a particular direction. He next tried gliding with a machine of the Lilienthal type, but soon discontinued his experiments in this direction, as he regarded such a machine as dangerous and difficult of control; and he became convinced of the necessity of devising an automatic arrangement by which the apparatus should right itself when a gust of wind caused it to heel over.

His first attempt in this direction was the construction of a "ladder kite," formed with wings pivoted to a central frame, and held in place by rubber springs. The kite flew very steadily, and was used as the basis of a "multiple-winged machine," also with movable

parts. Seven different forms were tried, the last and most successful ones having a number of wings, sometimes as many as five pairs placed one above the other. In the early forms the movements of the body in balancing amounted to five inches, in the later ones they were reduced to two inches, and finally to one inch.

Mr. Chanute worked in conjunction with Mr. Herring, and the last, and in many ways most successful, machine experimented on was a two-surfaced machine invented by the latter. This, too, was provided with movable parts for automatically regulating the balance, and was taken, in 1897, to Dune Park, where the sand-hills formed a safe experimenting ground. Many glides were made of from 150 to 300 feet, in winds varying up to 31 miles an hour (nearly $1\frac{1}{2}$ times the greatest wind-velocity in which Lilienthal experimented), and no accidents occurred, although on three occasions the machine was struck by gusts of wind from above. The longitudinal balance was perfect, no movement being required to right the machine when wind-fluctuations occurred in the line of motion, and in a side wind the movements of the body were much reduced by the automatic arrangement. The best glide was one of 927 feet, performed in 48 seconds by "quartering," i.e. sailing in a direction parallel to the side of a hill up which the wind was blowing. Several of Mr. Chanute's friends tried the machine, and found it easy to glide with.

These experiments of Messrs. Chanute and Herring bring us nearer to a solution of the problem of dynamic flight, i.e. directed motion through air without the assistance of balloons. It has, no doubt, been possible for Lilienthal and Pilcher to balance themselves in the air by their own agility, but directly we add a motor to a gliding machine the increased weight makes the effort far more difficult. It is only by reducing the exertion of balancing to a minimum, that we can hope to gain the mastery of a machine carrying its own motive power. On the other hand, the conditions of balance and stability of a propeller-driven machine may differ essentially from those of a mere gliding machine. In the latter, the only propelling force, due to gravity, acts in a direction fixed in space, while the propelling force due to a motor-driven screw acts in a direction fixed relatively to the machine.

Now, all our preconceived notions lead us to think that the addition of a propelling force of this character, so far from increasing the difficulty of maintaining equilibrium in the air, may materially improve the stability of the machine, and Mr. Herring states that this view has been borne out by experiments which he has made with models. While automatic balance and stability are the first factors to be secured in attempts to solve the problem of flight, it is important that these should not be investigated on gravity-propelled machines alone, but that the effects of a motor should be experimentally determined as soon as such experiments can be carried on in safety.

It is the transition from the gliding machine to the mechanically-propelled machine, carrying its own store of energy, which forms the chief gap to be bridged at the present time, in the attempt to conquer the air; and, as this gap bids fair to be narrowed, in the course of time, by reducing the difficulty of balancing large machines on the one hand, and by reducing the weight of motors on the other, we may hope that the near future may witness an experimental demonstration of the possibility of artificial flight.

[G. H. B.]

WEEKLY EVENING MEETING,

Friday, February 15, 1901.

His Grace The Duke of Northumberland, K.G. D.C.L. F.R.S., President, in the Chair.

The Right Rev. Monsignor Gerald Molloy, D.D. D.Sc.

Electric Waves.

Dr. Molloy said he had chosen the subject of electric waves, because he thought it represented one of the most important scientific developments of the closing years of the nineteenth century. He proposed, as far as might be within the limits of an hour, to give some general conception of the nature of these waves, to sketch very briefly the history of their discovery, and to show by a few simple experiments how their existence might be demonstrated, and their properties investigated.

He began by calling attention to the essential characteristics of wave motion, which he illustrated by a reference to waves of water, waves of sound, waves of light and radiant heat. He then said that the main purpose of his lecture was to bring home to them that electrical energy was transmitted through space by a motion of this kind; and that the medium through which this motion was conveyed was the ether, the same medium that served for the transmission of light and radiant heat.

Electric waves are most conveniently produced by a spark discharge. It was shown long ago by Professor William Thomson, now Lord Kelvin, that such a discharge, under certain conditions, was an oscillating phenomenon; that is, that the electric charge swings to and fro, like a pendulum, several times before it comes to rest. Each such oscillation would presumably involve some disturbance of the ether; these disturbances would be transmitted outwards and away in all directions; and following one another at regular intervals through space, would constitute a series of electric waves.

These ideas, which had been present, more or less vaguely, to the minds of scientific men for some time, were put into the form of a definite theory, supported by mathematical reasoning, by Professor Clerk Maxwell, about the year 1864. According to this theory, electrical energy is transmitted through space by vibrations or waves of ether; these vibrations travel with the same velocity as light; they are, in fact, vibrations of the same kind as light, differing only in the matter of wave-length; and if we could but increase the rate of vibration, thereby diminishing the wave-length, they would be-

come at first rays of dark heat, like those emitted by a hot metal plate, and then rays of light.

The truth of Maxwell's theory was experimentally demonstrated by Professor Heinrich Hertz, of Germany, in the year 1888, by a series of researches almost unrivalled for their brilliancy and thoroughness. His experiments, however, were only suitable to the laboratory, and various modifications were necessary in order to present the results in a sensible form before an audience.

The apparatus he had provided for this purpose might be said briefly to consist of two parts. At one end of the table was an arrangement intended to produce the electric waves. It consisted of an induction coil, and a pair of discharging rods ending in two brass knobs. This part he would call the oscillator. At the other end of the table was an arrangement intended to detect and reveal the presence of the electric waves. This part, which he called the resonator, was somewhat more complicated. First, there was a little piece of apparatus called the coherer. The coherer, as they knew, was a glass tube holding some metallic filings, which in their normal condition were practically a non-conductor of electricity, but which, when struck by electric waves, became a fairly good conductor. It was here mounted in the circuit of a small battery and an electric bell. In its normal condition, it opposed the passing of the current, and the bell was silent; when the electric waves arrived, it became a conductor—the current passed and the bell was rung. Thus the ringing of the bell would be a signal that the waves were there. A gentle tap on the coherer would restore it to its former condition, and it would be ready for a new experiment.

The lecturer now produced a spark in the oscillator, and the bell at once began to ring, proving that the electric waves had gone out from the oscillator and travelled through space to the resonator. A tap on the coherer reduced the bell to silence; and an assistant now proceeded to carry the resonator to various parts of the theatre. In every position it responded instantaneously to the spark of the oscillator; thus showing that electric waves, like waves of light and radiant heat, go out in all directions from the source of disturbance.

In the next experiment, the lecturer used two parabolic brass reflectors, which had been made many years ago for experiments on radiant heat. In the focus of one, he mounted a very small oscillator, which produced waves of some six or eight inches wave-length; while in the focus of the other he mounted a coherer in circuit with a small battery and bell. He then repeated with the electric waves produced in the oscillator, the well known experiment of the conjugate mirrors, and thus proved that the electric waves are subject to the same laws of reflection as waves of radiant heat and light.

With the same apparatus, he now tested the transparency and opacity of various bodies to electric waves—plate glass, sheet iron, indiarubber, wood. In particular, he called attention to the curious behaviour of plate glass with respect to waves of ether: it is very

transparent to electric waves; it is almost quite opaque to the much shorter waves of dark heat; it is again transparent to the still shorter waves of visible light; and it is again opaque to the shorter ultra-violet rays, and intensely opaque to the shortest waves of all, the X-rays.

The phenomenon of polarisation was next considered. From the way in which the waves are produced, we should expect to find that they are polarised at their source. It was shown how this might be tested by means of a frame over which a series of parallel copper wires were stretched. If the frame were held in the path of the waves, with the wires parallel to the spark, the grid would be opaque; if it were held with the wires perpendicular to the spark, the grid would be transparent. The experiment was tried, and the result fell out as expected. The grid behaved with respect to the electric waves, just as the analyser in a polariscope behaves with respect to a beam of plane polarised light.

The lecturer then called attention to the great variety of ether waves with which we are now acquainted, and having pointed out the position which each group occupies in the scale, said in conclusion: "Thus we come to realise that the various forms of energy to which, in common language, we give the names of electricity, magnetism, heat, light, chemical action, are all transmitted through space in the form of waves or vibrations of the ether; and that these vibrations are all essentially of the same kind, being distinguished from one another only by their wave-length. They produce widely different effects, when they strike upon our different senses; but considered in themselves they are only, so to say, notes of different pitch in the great scale of radiant energy which I have imperfectly attempted to sketch. This large and comprehensive view of radiant energy is one of the most notable results achieved by the great scientific men of the century that has just passed away. And the work that remains to be done by the coming generation, the great scientific men of the twentieth century, is to explore more thoroughly the properties of these ethereal waves, to fill up the gaps that still exist in the scale, and perhaps to reveal to the world the intrinsic constitution of the ether itself, that mystery of mysteries which underlies all those outward phenomena of nature."

WEEKLY EVENING MEETING,

Friday, February 22, 1901.

Sir Andrew Noble, K.C.B. F.R.S., Vice-President, in the Chair.

Sir W. Roberts-Austen, K.C.B. D.C.L. F.R.S. *M.R.I.*

Metals as Fuel.

A careful metallurgist,* writing in the eighteenth century, claimed that "every matter which is combustible either wholly or in part, is called fuel, the pabulum of fire." The word is, however, usually restricted to substances which may be burnt by means of atmospheric air with sufficient rapidity to evolve heat capable of being applied to economic purposes. The latter definition covers certain metals, though it was doubtless framed to include only carbon and associations of carbon and hydrogen, such as coal. The omission from the definition of the reference to atmospheric air would enable the list of metals which might be used as fuel to be widely extended.

It has long been known that metals will burn, and it would be easy to show that the history of inorganic chemistry is epitomised and enshrined in a mass of litharge, which is simply burnt lead. Successive generations of chemists, from Geber in the eighth century to Lavoisier in the eighteenth, studied litharge carefully before the latter proved partly by its aid the identity of respiration, calcination and combustion. Into this history I need not enter, but it may be pointed out that Sir Isaac Newton † had a clear idea as to the possibility of burning metals. "Is not fire," he asks, "a body heated so hot as to emit light copiously?" . . . "for what else is red-hot iron than fire?" and he significantly adds, "metals in fusion do not flame for want of copious fume." He was, moreover, aware that a mixture of lead and tin "suitably heated" does emit "fume and flame," and, in fact, a mass of one part tin and four parts lead, which looks metallic, will, if it be kindled, continue to burn like an inferior variety of peat, leaving an ash-like product which may be used as an enamel.

I propose to show that metals may be burnt for the sake of the

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* C. E. Gellert, 'Metallurgic Chemistry,' translated by I. S. (London, 1776), p. 74.

† 'Optic,' pp. 316–319, quoted by Shaw in his edition of the works of Boyle, vol. ii. p. 400.

heat and light they produce, just as ordinary fuels are burnt, except that in burning ordinary fuels combustion is often effected in two distinct steps or stages, in the first of which carbonic oxide is formed, and in the second carbonic acid, the products in both cases being gaseous. When metals are burnt, the products of combustion are solid, or condense to solids, and they therefore present a marked contrast to ordinary fuels which, as has just been stated, yield on combustion gaseous products. As I shall have but little to say about the light which attends the combustion of metals, I may as well dismiss the subject by reference to a familiar application of the burning of metals for the purpose of illumination. It is easy to fire electrically a portion of what is known as a "magnesium star," and a "fire-ball" of magnesium attached to a parachute is beautifully packed in this shell, for the loan of which I am indebted to the authorities of the Royal Arsenal, Woolwich, and when the shell explodes the stars burn and illuminate the enemy's position in the darkness of night, so that guns may be laid to place projectiles in the enemy's lines.

Before proceeding further, I want to use the electric furnace as affording a basis of comparison with the method of producing high temperatures by the combustion of metals, which I shall proceed to show subsequently. A current of 100 amperes at 200 volts is passed by carbon poles into the furnace in which pig-iron is being melted; directly the last piece of iron has become fluid, the temperature of the fused pool must be about 1300° C. The fluid mass is reflected on the screen merely to give some indication as to the appearance of such a mass at 1300° C., and not to afford a test of the capabilities of the electric furnace. Later on I hope to show that a far higher temperature can be produced by very simple means in a receptacle of about the same capacity as the laboratory part of the furnace.

Henceforth in the course of this lecture metals will be burnt for the sake of the heat which is the result of their combustion. From this point of view metallurgists have long used metals as fuel, often without due recognition of the fact, but case after case could be cited in which conducting definite metallurgical operations is made possible by burning portions of the metal or metals under treatment. Time will perhaps be saved if I place in sharp contrast the use of ordinary fuel and metallic fuel, even though it takes us rather far back, for I do not want it to be thought that the use of metals as fuel is new, although their adoption for this purpose has recently been greatly stimulated. Here is a mass of very ordinary iron ore picked up on a heath in Surrey, which skirts the site of what was once the ancient forest of Anderida. The pre-historic dweller on the heath who used the beautiful flint arrowheads, which are found near the iron ore, merely burnt the wood of the forest to warm himself or to cook his food. But the Britons whom Cæsar found in Andreaswold smelted iron with the wood of the forest trees, from which they prepared charcoal, and smelting iron was actively conducted in Queen Elizabeth's

reign, and even survived into the last century in the district I am contemplating. But in smelting iron, carbon became associated with it and played a subtle part, rendering the iron precious for certain purposes and useless for others. Iron had therefore to be "decarburised" with a view to its conversion into steel, and in doing this metallurgists for centuries truly burnt some of the iron itself, using it actually as fuel. I will only add that the use of metals as fuel assumed magnificent proportions in the hands of Bessemer, as may be illustrated by an experiment. A few pounds of a compound of iron, carbon, silicon and manganese is melted in the wind furnace, which is simply used because it affords a convenient method of melting the mass, which is turned into a small Bessemer converter. A stream of oxygen is directed into the fluid mass. Air would do, but with so small a mass the free nitrogen would cool it too rapidly. In a few seconds the carbon in the fluid will be burnt away: nevertheless the mass gradually becomes hotter and hotter, a shower of sparks rises, and a brillant pyrotechnic display is the result. The metalloid silicon is now burning, and then brown fumes of iron and manganese pass freely off; these metals are truly burning and are maintaining the heat of the bath, and the presence of their fumes shows that it is time to stop the operation. The temperature is somewhere near 2000° C., but according to some recent investigations of Professor Noel Hartley * a temperature of more than 2000° C. is attained in the converter. Bessemer gave the world in 1856 cheap steel; we therefore owe to him the inestimable benefits that are the results of that gift, and I ask you to bear in mind that his great service to the industry of which we as a nation are so justly proud rested on the possibility of using metalloids and metals as fuel. I have already promised that in the course of the lecture I will show some experiments in which the temperature will be a thousand degrees higher than in the one you have just seen. In the Bessemer process the products of combustion are both gaseous and solid, and in a very ordinary case the heat engendered by the carbon of the bath which evolves gases is only half that which results from the combustion of the silicon, iron and manganese which yield solid products. As regards the "open-hearth process" in the phase of it which is known as the "pig and ore" process, oxygen is presented and heat is produced under similar conditions to those we shall consider subsequently in the case of the action of aluminium on ferric oxide.

The following table, which contains the relative calorific powers of different metals and metalloids as compared with carbon, indicates the advantage which certain metals possess over carbon for use as fuel. The question at once presents itself—At what temperature will such metals as can be used for fuel begin to abstract oxygen from the air? The answer is—It depends on the method by which the metals are pre-

* Phil. Trans. vol. cxcvi. series A, p. 479, 1901.

HEAT EVOLVED BY BURNING ONE GRAMME OF THE FOLLOWING ELEMENTS.

Element.	Product of combustion.	Calories.
Aluminium	Al_2O_3	7250
Magnesium	MgO	6000
Nickel	NiO	2200
Manganese	MnO_2	2110
Iron	Fe_2O_3	1790
"	Fe_3O_4	1580
"	FeO	1190
Cobalt	CoO	1090
Copper	CuO	600
Lead	PbO	240
Barium	BaO	90
Chromium	Cr_2O_3	60
Silver	Ag_2O	30
Carbon	CO_2	8080
"	CO	2417
Silicon	SiO_2	7830

pared. If they are in a chemically active state, as lead is which has been prepared from tartrate of lead, they will, in many cases, take fire in air and burn at the ordinary temperature. Such lead burns readily when shaken in air. If this mass of uranium, for which I am indebted to M. Moissan, be filed in air, the detached particles will ignite. Metallic iron which has been reduced by hydrogen from its oxide at a temperature below 700° C. will also take fire and burn in air at the ordinary temperature, a point of extraordinary interest in relation to the allotropy of iron.* Metals in this chemically active state are said to be "pyrophoric."

So far as I am aware, metals in this chemically active state have not been used as fuels. Neither am I aware that any use has been made of the allotropy of metals as enabling them to be used as fuel, but Professor Graham once told me that pyrophoric iron had been suggested for warming ladies' muffs, the intention being to place the iron in a small receptacle and to admit air gradually as warmth was needed. Sir Henry Trueman Wood also remembers the suggestion, but tells me that he can find no record of it in the 'Journal' of the Society of Arts. I may just mention that the burning of metallic antimony plays a very important part in roasting silver ores, and the behaviour of the metal is so pecular while burning that I must pause to show it you. [A melted globule of antimony, if thrown on to a tray of paper, darts about and cannons from the sides, leaving a track of dark oxide on the paper.]

The metal I am going to employ as fuel is aluminium, the oxygen for its combustion being supplied by metallic oxides, which readily part with their oxygen to aluminium if it be raised to certain definite

* Osmond and Cartaud, Ann. des Mines, vol. xviii. 1895, p. 113.

temperatures. This question of the transference of oxygen from one metal to another, which results in the liberation of the metal attacked, is of special interest to us at the Royal Institution, for it undoubtedly originated within these walls, and is due to Sir Humphry Davy. He discovered potassium in 1807, and in 1809 attempted to remove the oxygen from alumina by heating it with metallic potassium. He says,* "if I had succeeded in isolating the metal I should have called it *alumium*." His success was imperfect, but he certainly did obtain, by the intervention of metallic potassium, an alloy of aluminium and iron. It remained for Wöhler to prepare pure metallic aluminium from its chloride in 1827, and for Henri Saint Claire Deville, who began to work in 1854, to establish the metallurgy of aluminium on an industrial scale. As regards the reduction of metals from their chlorides, Wöhler † obtained crystalline compounds of chromium and aluminium, and Michel ‡ compounds of aluminium with manganese, iron, nickel, tungsten, molybdenum and titanium. Levy § obtained an alloy of titanium and aluminium, Beketoff ‖ an alloy of barium with aluminium from the chloride of barium mixed with baryta. Dr. Goldschmidt ¶ has given references to these authorities in a recent valuable paper. In 1856, Charles and Alexandre Tissier ** observed the fact which is the starting-point of the experiments I have to show you. They found that aluminium decomposes the oxides of lead and of copper, much heat being evolved by the reaction.

They do not appear to have used aluminium in a finely divided state, and therefore failed to reduce certain metals from their oxides which are now known to be perfectly easy to reduce. It was not until comparatively recently that the use of aluminium for separating other metals from their oxides assumed serious proportions. Claude Vautin showed on June 13, 1894, at a soirée of the Royal Society, a few metals, and among them carbon-free chromium and manganese, which he had prepared, and as he undoubtedly gave the impulse that started much of the subsequent work in this direction, it may be well to give the description which was appended to the specimens he showed. It runs as follows:—

Specimens of Metallic Chromium, Manganese, Tungsten, Iron, etc. free from Carbon; also fused Alumina, obtained during the reduction of the metallic samples.

"The specimens of metallic chromium, manganese, etc. have been reduced from their oxides by means of metallic aluminium. The

* Phil. Trans. part i. 1810, p. 60.
† Ann. der Chemie, vol. cvi. p. 118.
‡ Ibid. vol. cxv. p. 102; ibid. vol. cxiii. p. 248.
§ Comptes rendus, vol. cvi. p. 66.
‖ Ann. der Chemie, vol. cx. p. 374.
¶ Ibid. vol. ccci. p. 19.
** Comptes rendus, vol. xliii. 1856, p. 1187.

oxide of the metal to be reduced is intimately mixed with finely divided aluminium, and heated in magnesia-lined crucibles. The heat produced by the oxidation of aluminium during the operation is sufficient to fuse alumina, a specimen of which is exhibited."

The subject is, however, in a sense your own, for, as far as I know, the lecture on "The Rarer Metals and their Alloys," * which I delivered here in 1895, was the first occasion on which the reducing action of aluminium was demonstrated on a comparatively large scale, and covered an extended series of metallic oxides. Since that time great progress has been made, the most noteworthy advance being in the direction of the use of aluminium for the sake of the heat afforded by its combustion as a true fuel, the oxygen being derived, not from the air, but from a metallic oxide. In order that I may be clear, let me repeat that when coal is burnt the oxygen is derived from the air.

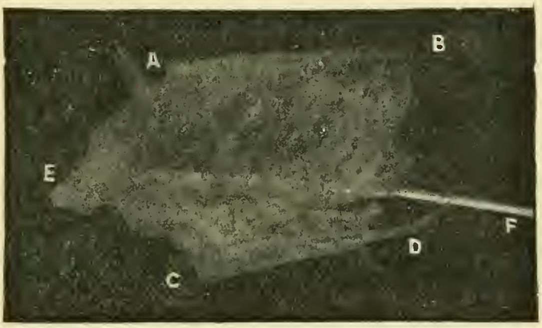

FIG. 1.—The oxidation in air of an amalgamated wire of aluminium, E F. The films of alumina, A B and C D, are those which first formed on the wire.

When aluminium is used as a fuel the oxygen is derived from a metallic oxide, the metals change places: the aluminium is oxidised, and the other metal set free from its oxide. This part of the subject must be carefully approached, and the question at once arises as to what extent the aluminium must be heated before it will begin to abstract oxygen from air or from an oxide. It is well known that the metal aluminium will not oxidise sensibly in the air at the ordinary temperature, but the presence of a little mercury enables it to oxidise readily. Le Bon † has shown how minute the quantity of mercury may be. This wire of aluminium to which a thermo-couple is attached will, if a mere trace of mercury be rubbed on its surface, become rapidly heated by oxidation, the temperature rising to 102° C., while

* R. Inst. Proc. vol. xiv. p. 497. 'Nature,' vol. lii. pp. 14 and 39.

† Comptes rendus, vol. cxxxi. p. 707.

at the same time a fungoid-like growth of alumina forms on its surface (see Fig. 1). Aluminium foil will burn readily in oxygen if its combustion be started by a glowing fragment of charcoal. The temperature at which aluminium will abstract oxygen from a metallic oxide will depend on the oxide submitted to its action. Three cases may be taken: (1) Lead oxide and granulated aluminium may be ignited by a match, as may also silver oxide (Ag_2O), for it parts with its oxygen very readily. (2) Chromium oxide (Cr_2O_3) and granulated aluminium burns slowly and requires rather a high temperature to start the reaction. Oxide of iron (Fe_2O_3) and granulated aluminium also requires the presence of a readily reducible oxide to start the reaction. On the other hand (3) a mixture of sodium peroxide, carbide of calcium and granulated aluminium may be started by a drop of water by the mere inflammation of the acetylene. In all these cases, or in any other case, the products are solid, for if any of the reduced metal is volatilised it soon condenses, and may be collected, usually in an oxidised form.

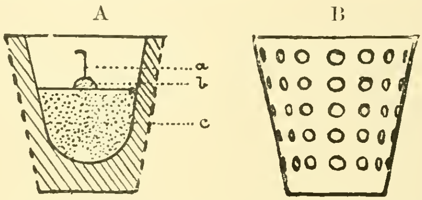

Fig. 2.—Crucible in which the reduction of metallic oxides is effected. A, diagrammatic section of the perforated sheet-iron crucible B, lined with magnesia; *c* is the mixture of aluminium and the metallic oxide to be reduced to metal; *a* is a piece of magnesium ribbon placed in a mixture *b*, of aluminium and some readily reducible oxide.

In using aluminium as fuel the object, of course, is to produce intense heat, and, returning to this mass of iron ore from the Surrey heath, it may at once be stated that an oxide of iron, ferric oxide, is the most convenient oxide to use, partly because it is inexpensive.

Many of my audience already know that the recent investigations having for their object the use of aluminium as a source of heat have been conducted by Dr. Hans Goldschmidt, of Essen, and it is through his labours that metallurgy enters upon an entirely new phase. It would be difficult to offer him fuller or more unstinted praise than that. You will, I trust, soon realise how much this branch of metallurgical industry is indebted to him. In its simplest form his process consists in igniting a mixture of oxide of iron, ferric oxide and finely divided aluminium. To this mixture the name of "thermit" has been given, and several varieties of it, adapted to various kinds of work, are used by Dr. Goldschmidt at the works of the Allgemeine Thermit-Gesellschaft at Essen-Ruhr.

The mixture is placed inside a crucible (Fig. 2) and is ignited by

a small piece of magnesium wire, which serves as a kind of wick if it is placed in a little heap of calcium sulphate and aluminium. Such a mass will now be lighted, and you see intense heat is produced. [When the operation was conducted in accordance with the above indications, the theatre was brilliantly illuminated by the intense light produced. A mass of metallic chromium weighing about 100 lbs. reduced to the metallic state as above described, was exhibited.] The aluminium abstracts oxygen from the oxide of iron, and a sufficiently intense heat is produced, not only to melt the iron which is liberated from its oxygen, but to melt up the slag and, further, to leave a considerable surplus of heat, which is available for doing other work. No known pyrometer will enable the heat to be measured. I believe it to be about 3000° C. The aluminium plays the part of a fuel, and this table shows the advantage aluminium possesses as compared with carbon for the particular work required of it.*

THE REDUCTION OF Fe_2O_3 TO IRON BY ALUMINIUM AND BY CARBON.

—	Aluminium.	Carbon.
Compound produced	Al_2O_3	CO
Amount of reducing agent required to produce 1 kilo of iron	0·484 kilo	0·321 kilo
Amount of heat produced by oxidation of the reducing agent	3456 calories	770 calories
Heat required to reduce the Fe_2O_3	1796 „	1796 „
Heat required for fusion of the slag	518 „	
Heat required for fusion of the iron	362 „	
Total heat required	2706 „	1796 „
Residual heat available	750 „	−1026 „

On the aluminium side some 750 calories (units of heat) are available to do work (3456 − 2706 = 750 calories). On the carbon side there is a deficiency of no less than −1026 calories. As regards the crucibles, they may be made of alumina, the solid product which is the result of the combustion of aluminium. They may also be made of magnesia or mended with magnesia. I shall have more to say about the solid product of the combustion subsequently. The practical application of the process is as follows. The ignited and molten mass in the crucible is so intensely hot that it may be made to unite surfaces of steel that require to be joined, such as the ends

* These data are from a paper by Prof. Kupelwieser, of Leoben, 'Oesterreichische Zeitschrift für Berg- und Hüttenwesen,' vol. xlvii. 1899, pp. 145–149.

of lengths of rails. It may be objected that the fluid contents of the crucible would set as a whole round the metallic junction and give trouble, but this is not the case, for a layer of fluid alumina appears both to coat the rod, tube or rail which has to be welded,

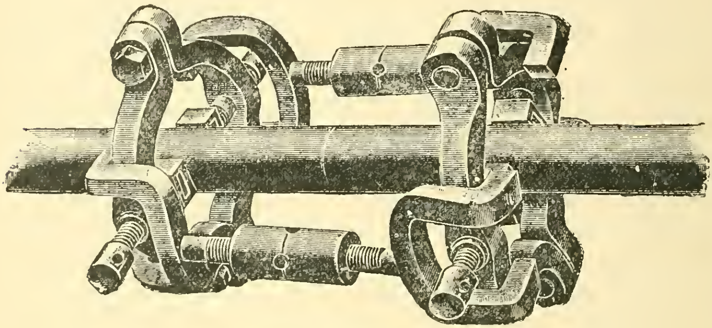

Fig. 3.—The clamps used for welding tubes up to four inches in diameter.

and to set in a mass which can be readily detached after the work is done. The casings (Figs. 4 and 5) are protected in the same way. The diagrams (Figs. 3, 4, 5) need but little comment, as they sufficiently indicate the method adopted in the cases they represent.

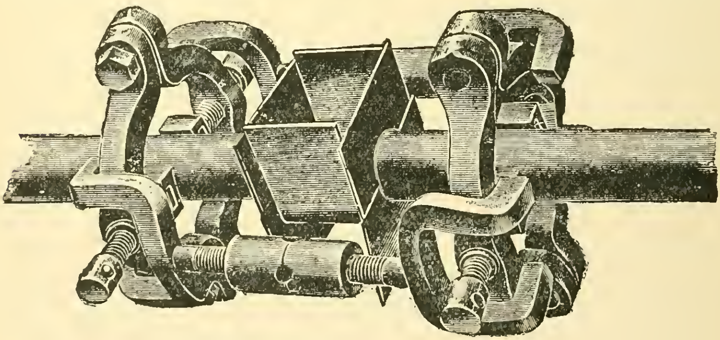

Fig. 4.—Tubes clamped together with a casing of thin iron round the junction to be welded.

[These figures were used to illustrate a paper by Mr. E. F. Lange.* I was indebted to him for the loan of small appliances of a similar kind to enable me to demonstrate to the audience the welding of

* Journal of the Iron and Steel Institute, 1900, No. ii. p. 192.

steel tubes, and the operation was shown on as large a scale as safety would permit.] The welding of three miles of electrical tramway rails was successfully effected in Brunswick in May 1900.

As regards the comparison of the use of aluminium as fuel with the electric arc, M. Camille Matignon,* in a very interesting discourse recently delivered in Paris, has instituted a comparison between the Goldschmidt process and electric furnace. Quoting Moissan,† he shows that in reducing titanic acid by carbon in the electric furnace having a "laboratory space" of 800 cubic centimetres, 300 horse-power absolute were employed, producing per second 190,500 calories by burning 1·08 kilograms of aluminium. On the other hand, by burning 3·2 kilograms of ferric oxide during one minute in a crucible of about the same capacity as the laboratory of the electric furnace, the rate of evolution of heat is equivalent to 375 horse-power absolute;

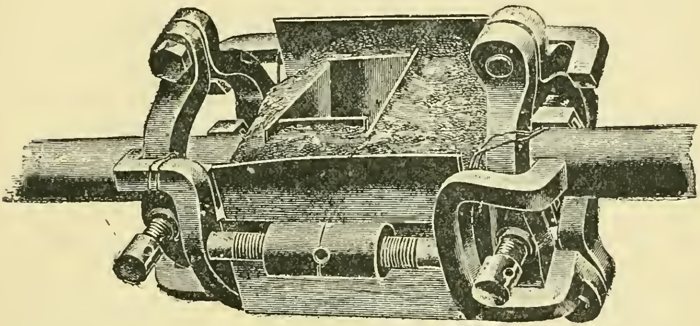

FIG. 5.—Casing packed round with moulding sand in readiness for the welding operation.

the latter process does not, however, work continuously, but could readily be made to do so. It should be pointed out that an impure variety of aluminium can be used, and that if the heat needed to effect a given operation is but moderate, the aluminium may be diluted by the presence of an inert substance.

The photomicrograph (Fig. 6) is from a little test piece of wrought iron (Fig. 7) which was cut in two. The carefully faced surfaces were then clamped together, and I united them into an excellent weld, without any previous experience in conducting such an operation. No line of demarcation can be seen, and the crystals pass over the line A B, which I know by measurement to be that of the actual weld.

The very hot molten iron may be used in a somewhat different way for repairing defective castings. In this case the slag is carefully

* 'Moniteur Scientifique Quesneville,' 4 S. vol. xiv. part i. pp. 357 et seq.

† 'Le Four électrique,' p. 19.

poured off the fluid iron in the crucible and the iron is then poured into the defective part in the casting which it is required to mend, a guiding rim of some refractory material being provided. By mixing other metallic oxides with the iron oxide, the metals they contain are

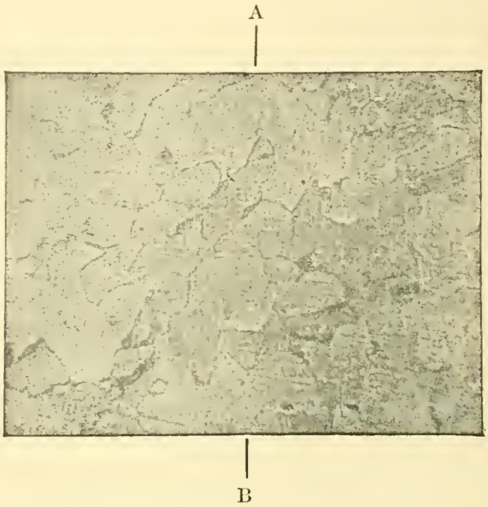

Fig. 6.—Section of the welded test piece (Fig. 7), showing crystals passing across the line of weld, A B. Magnification 140 diameters.

reduced and alloy themselves with the iron, and the composition of the defective casting can thus be matched. In connection with the repairs of fractured or defective steel castings, the possibility of producing directly steel of a suitable degree of carburisation is important.

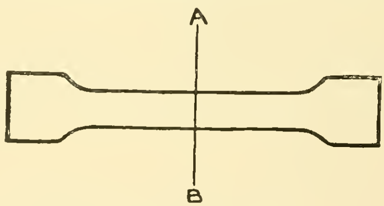

Fig. 7.—Test piece of wrought iron welded at A B. See Fig. 6 for micro-section.

This may readily be effected by mixing fragments of cast iron with the "thermit." Thus 70 to 90 grams of cast iron mixed with 1000 grams of thermit give a very fine grained and workable steel. One useful application of the process is for locally softening hardened

armour plates in the positions where bolts and screws have to be inserted through holes drilled to admit them. This is effected by placing a little fluid "thermit" on the spot where the plate has to be drilled, and the heat softens the hardened surface. It should also be remembered that, with reference to the repairs of defective parts of machinery, a suitable admixture of metallic oxides with the ferric oxide, such as those of chromium, nickel or manganese, may be reduced together with the iron derived from the ferric oxide. Richly carburised iron may be added to the molten mass, and in this way any quality of steel may be produced.

This latter reference to metallic oxides reminds us of the original use for which the finely divided aluminium was employed, namely, as a reducing agent for the rarer metals, and not for the sake of the heat evolved by the reaction. This portion of the subject I dealt with at the Royal Institution six years ago, but there have been great advances since. It would have been tedious to have conducted the experiments before you, as the crucibles would have taken so long to cool; but in each of these crucibles which will now be broken open, I hope to find a small mass of metal, which, until now, has not left the spot in which it was reduced. [About a pound of nickel and a pound of cobalt were then removed from the respective crucibles in which they had been reduced.]

Manganese and chromium containing only small quantities of carbon are now produced on a large scale for industrial use. As regards the reduction of metals and alloys from their oxides by burning aluminium, the following are the most recent results that have been obtained.* The use of carbon-free chromium in connection with the metallurgy of steel is an exceedingly useful development of the methods we have considered. Hitherto, the addition of ferro-chrome to steel has evolved a loss of from 20 to 25 per cent. of the chromium while with pure chromium the loss is slight. Moreover, the addition of ferro-chrome incidentally raises the percentage of carbon, and steel containing, for instance, 2·5 per cent. of chromium should not have more than from 0·15 to 0·20 per cent. of carbon, and this can only be attained by the use of pure chromium. In the manufacture, also, of tool steel, the percentage of chromium may reach from 6 to 10 per cent. and even higher, a result which is only rendered possible by the use of pure chromium. In the same way, in connection with the metallurgy of copper, the possibility of providing carbon-free manganese is important, as is also the preparation of cupro-manganese free from iron. Alloys of manganese with zinc and with tin are likely to prove of value. Many uses have been found for the alloy containing 80 per cent. of zinc and 20 per cent. of manganese, while it is anticipated that the alloy containing 50 per cent. of tin and 50 per cent. of manganese will also prove to be important. Use has also been found for an alloy of 70 per cent. manganese and 30 per cent. chro-

* 'Stahl und Eisen,' March 24, 1901.

mium. Ferro-titanium, with 20 to 25 per cent. of titanium, and alloys of titanium and manganese containing from 30 to 35 per cent. of titanium, have also been produced. Titanium, moreover, absorbs nitrogen, and ferro-titanium is found to be very useful in producing sound steel castings. I, quite independently of Dr. Goldschmidt, succeeded in the preparation of alloys of iron with from 3 to 25 per cent. of boron, the alloy containing 3 per cent. of boron proving to be beautifully crystallised. Dr. Goldschmidt states that definite results have not been obtained in attempts to utilise it. I am still investigating this most interesting subject. Dr. Goldschmidt has obtained ferro-vanadium, the best results being obtained with steel containing 0·5 per cent. of vanadium. He has also prepared an alloy of lead and barium containing 30 per cent. of barium, which affords an example of the possibility of forming alloys of metals with those of the alkaline earths by this process.

It only remains for me to direct your attention to the nature of the solid product of the combustion of aluminium, which is alumina often of a high degree of purity, and in a specially interesting form. The alumina from the reduction of oxide of chromium, when it is allowed to cool, forms large ruby-tinted crystalline masses, closely resembling the natural ruby. I have now to show you on the screen some rubies and sapphires produced as an incident of this beautiful process. The blue sapphire mass is, however, only translucent, not transparent. The ruby crystals are often very beautiful, as these slides show. Rubies placed in a vacuum tube and subjected to the bombardment of an electric discharge are, as Sir William Crookes has taught us, beautifully phosphorescent. I have here in this tube some thin crystalline plates of artificial ruby; they become beautifully phosphorescent when the current from the induction coil is passed through the tube, and by the kindness of Sir William Crookes I can show you some true rubies treated in a similar way. The behaviour of the artificial rubies in the vacuum tube is not quite as brilliant as that of the natural ones, but hitherto no special attention has been devoted to their preparation; they are simply thin plates broken from a large crystalline mass of slag such as that on the table. I may add that this variety of corundum produced by the burning of aluminium is very hard, and may be used, not only for the same purposes as ordinary corundum, but for lining the crucibles in which the operations are conducted, so that the product of combustion takes its place in conducting the process. My warmest thanks are due to Dr. Goldschmidt for lending me the beautiful specimens on the table, and to Mr. W. H. Merrett for his aid in conducting the experiments.

I have set before you the considerations respecting the use of metals as fuel simply as they appear to flow. I trust that the adoption of the title of this lecture has been justified by the evidence given as to the possibility of using metals as fuel in the strictest sense of the word. It is well to be accurate on this point, because we are told that the first known appearance of the word "fuel" in the English lan-

guage occurs in a poem * and seems to have been a misinterpretation of the old French word *fouaille*, and was adopted in the belief that sustenance for the body and food for the flames are synonymous. Widening our view of metals by grouping them with fuels will be acceptable, because fire and flame powerfully appeal to our thoughts. We "kindle" enthusiasm, and add "fuel" to the fire of ambition—we constantly use fire, flame and fuel as similes, and any prospect of extending their use to us as such by enlisting metals in the service will be welcome. An early Italian metallurgist, Vanoccio Biringuccio, might not have thought so, for I find that, writing in the sixteenth century, he quaintly devotes the last chapter of a work on metallurgy to "Fires which burn and leave no ashes." † In this chapter he appeals to envy, hatred, malice and other products of a kindled imagination, and traces their analogies to fuel and flame, but he speedily takes leave of his readers in alarm at the prospect such a treatment of the subject presents.

The burning of aluminium as fuel gives us sapphires and rubies in the place of ashes, and metallic fuel is burnt, not by the air above but by the oxygen derived from the earth beneath, as it occurs in the red and yellow oxides to which our rocks and cliffs owe their colour and their beauty.

[W. R.-A.]

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* 'Cœur de Lion,' 15th Century.

† 'De la Pirotechnia,' 1540, p. 167. (Venice.) 'Del fuocho che consuma et non fa cenere.'

WEEKLY EVENING MEETING,

Friday, March 1, 1901.

Sir Frederick Bramwell, Bart., D.C.L. LL.D. F.R.S. M. Inst. C.E., Vice-President, in the Chair.

Henry Hardinge Cunynghame, Esq., C.B. M.A. *M.R.I.*

Enamels.

Before presenting to you this evening some observations upon the art of enamelling upon metal, I desire to make it understood that I do not pretend to give an account of any newly-discovered facts. Almost all that I shall say and show was known three centuries ago. Any improvements are matters of detail. But during the eighteenth century, and the early part of the nineteenth, the art became debased and discontinued, and as no adequate written accounts of it existed the processes had to a large extent to be re-discovered. This, however, has been so completely done, that it may be said with confidence that there is no pigment known to the mediæval craftsmen which we do not now possess, and that we can imitate and surpass all their colours. Where modern art fails, is in the bold and subtle arrangements which ancient work displays, and in the delicate tints which many of the old workmen knew so well how to use. I wish I could say that our modern artistic powers in art, generally, were on a par with our scientific knowledge.

It will be quite impossible in the time at my disposal to do more than give a very brief sketch of the history of enamelling. After this I shall show how the enamel is manufactured, and, lastly, how it is placed upon copper and gold jewellery—avoiding details, so as to endeavour to give a view of the subject as a whole.

In the course of my remarks I shall mention a few facts of chemistry, concerning which many of my audience know more than I can pretend to do; but since there are some present whose knowledge of the subject is rather on its artistic side, I shall be pardoned if I say some things which would be out of place if I were dealing purely with the scientific side of the subject.

The word "enamel" has been used in many senses. We speak of enamelling a bicycle, which really only means to give it a coat of black made of bituminous substances and then to expose it in an oven for a few hours to a moderate heat. Enamel-paint is only common oil-paint mixed up with some hard varnish, and the enamel which ladies used to employ upon their faces consisted of certain preparations of gelatine and gums.

All these uses of the word enamel are, strictly speaking, incorrect. True enamel consists of glass, melted on to the surface of metals or of pottery by means of intense heat. But in all these cases the substance is the same—it is glass.

Glass consists of silica united with soda or potash, either simply or with the oxides of various earths, such as lime, magnesia, or alumina; and the colouring matter is supplied by the addition of small quantities of iron, copper, cobalt, manganese, chromium and other metals.

The glazes upon china, porcelain, earthenware, bricks, tiles, iron saucepans, and on copper, are all of the same character, simply consisting of various sorts of white or coloured glass, and this glaze may be made, by means of heat, to adhere like a varnish upon the surface of any substance, which will not melt when raised to the temperature necessary to fuse the glass. But when ordinary glass, composed of silica and soda or potash, is melted and run on to china, it cracks, or as it is termed in old English phrase, "crazes." A crazed plate is unfit for domestic use, for the hot grease is apt to penetrate through the cracks under the ware, and to produce stains, and make the crockery offensive and insanitary. In order to prevent "crazing," it is necessary to mix some alumina, or clay, or some lime, or magnesia, with the glass. But these substances render the glass so hard to melt, that in order to lower its fusibility some lead or borax must be added. Of these, lead is the best, for it makes the glaze tough, elastic, and to flow freely. Borax makes the glaze more apt to crack, and liable to be affected by moisture.

The use of lead of course involves the danger of lead-poisoning. In the present state of the pottery trade, it is too much to ask that lead should be totally disused, but it is possible to prevent the workmen from handling it in a raw state, and thus its danger may be very greatly diminished.

Enamelling was known to the Greeks, probably also to the Egyptians and to the Scandinavians. But it seems first to have been brought to perfection by the Byzantines, and chiefly, though not exclusively, in Christian art.

In its later forms Greek art reflected the feelings and the degeneracy of a dying world. Those who desire to experience it will find it in literature in the Milesian tales, or the Golden Ass of Apuleius. In sculpture it is reflected in the small tinted statuettes to be found in most museums. The severity of earlier times has given place to a cloying sweetness, which, though charming, is instinctively felt to be wanting in manly vigour.

With Christianity art took a severer form. The early Christian Church had before her eyes the spectacle of a world which had perished through the immoderate use of art. Weary of the babbling of the schools of rhetoric, men turned to the silence of the cloister. The desert was a necessary protest against the luxury of later Roman times; celibacy was a reaction against universal sexual profligacy.

And the Church, therefore, seems to have resolved to curb the arts, and for the future to retain them in the strictest leading strings.

The languishing embraces of graceful Greek gods were exchanged for the severe and even repellent types of Byzantine mosaics. But this very restraint, salutary as it was while the morals of Europe were being reformed, was not without disadvantages; for where literature or art is in complete subjection to a system of morals, religion, or politics, it is certain to be cramped down and kept in bondage by artificial rules which destroy its vitality. Therefore, though the fire of Christianity purged classical art of most of its dross, it destroyed a good deal that was valuable in the process.

The rejection of ornate ritual and church decoration is partly due to this feeling. But it is also true that an art which is intended to appeal, not to Anchorites or Puritans, but to ordinary men, must satisfy not only the moral but also the æsthetic sentiments. Therefore the perfection of sacred art is seen when the highest beauty has been reached without the sacrifice of pure and noble religious feeling. The work of art must, so to speak, have a noble soul enshrined in a beautiful body.

The method adopted by the Byzantines was to mark upon a plate of metal the outlines of the desired pattern, to solder over those outlines little cloisons, or walls of metal about $\frac{1}{100}$th of an inch thick and $\frac{1}{50}$th of an inch deep, and to melt glass into the spaces.

The chief examples of this style that remain are at St. Mark's, Venice, St. Ambrogio at Milan, and at Limburg, but small specimens are to be found in most museums.

The figures in Byzantine enamels resemble the figures in Byzantine illumination and mosaic. They are undoubtedly derived from classical sources. But the sense of artistic effect is always subordinated to the religious aim of the work. The attitudes are stereotyped, and they are sometimes rather more like religious hieroglyphics than like pictures. But the charm of them is in the pure healthy colour, and the strong devotional feeling they display. On account of this, we can overlook goggle eyes, and uncouth drapery, which would be unpardonable in a modern artist acquainted with the work of the great European schools.

It will be remembered that it was the ungrateful hand of Venice, which had benefited so much by Byzantine art and trade, that struck the first and heaviest blow at the Byzantine Empire. When it fell, the art of glass-making passed to Venice, and by Venice was transmitted to Europe. For years the Venetians kept the secret of making coloured glass. If one of their workmen betrayed them, they had him tracked down and murdered, even in the most distant country.

But, in spite of all their care, the secrets which they had learned from the East leaked out, and as the power of Venice declined, the chief seat of the enamel trade was transferred to Limoges, just at the

time when that splendid outburst of artistic feeling took place which made French cathedrals the wonder and envy of the world.

Most of the early Limoges work was champlevé, that is to say, instead of the plate being prepared by putting walls on to a thin sheet of metal, the hollows were carved out of a thick sheet.

The metal employed as a foundation was bronze, that is to say, copper mixed with about ten per cent. of tin, or else brass (copper and zinc). The hollows were cut out to the depth of about one-twentieth of an inch and left rough at the bottom, so as to give a better hold to the enamel, which was opaque, and made by staining opaque-white enamel with the oxides of various metals, so as to produce two or three shades of blue and green, yellow and black.

The faces in Limoges champlevé were generally left unenamelled, but chased up and filled in with black, like church brasses. Sometimes the heads were modelled in relief, and fastened on with rivets. Each colour was usually put in a separate compartment, but sometimes two colours will be found in juxtaposition in one compartment. The face of the enamel was ground and polished, and the metalwork was frequently gilded by the mercury process, which only requires a low heat.

Limoges champlevé was rough and bold, but often slovenly. On a large scale, as when it was employed to fill up the brasses on tombs, it had a fine effect. Traces of this work are yet to be seen on the battered tombs of the kings at Westminster Abbey; but some fine enamelled brasses, in a good state of preservation, exist in France.

The colours used for champlevé were usually opaque, like coloured sealing wax.

When transparent enamel was introduced the style of champlevé changed, and it became what is known as basse-taille.

There are some specimens of this work at South Kensington, and a small one is also in the Wallace Collection. There is a set of six scenes from the life of Christ in the Louvre. Each plate is circular, and about two and a half inches in height. They were no doubt used as ornaments for a reliquary. The drawing is rather archaic, being in the style of the Flemish masters, who had so great an influence upon French art; but in execution, colour, finish, and artistic feeling, nothing better can be desired.

With the exception of one or two dents on the edges, they are as fresh as though they were not twenty years old. Indeed, as one looks at them, it is impossible to realise that they were done at least five hundred years ago. It is hardly too much to say that these six modest little plates, which a casual visitor at the Louvre might pass by without notice, are, from an artistic point of view, better than anything that has been put out by all the porcelain and enamel factories of Europe for the last two hundred years.

With basse-taille, mediæval art may be said to close; in fact, Renaissance has already begun.

The nature of Gothic work will never be understood, unless we remember that it was chiefly used for the instruction of persons who could not read, and who learned their Bible stories and the lives of the saints from the painted walls and panes of some cathedral. The requirements of art were subordinated to utility. The art, like the literature of the Gothic period, was highly symbolic. As an example of this symbolism, it may be mentioned that there was a mediæval tradition that the Creed was the joint work of the Twelve Apostles, each of whom contributed a sentence. Each sentence was supposed to be based on a corresponding sentence from one of the twelve greater prophets, so that each apostle had his particular prototype. In Chartres Cathedral the Apostles may be seen in order, each actually sitting on the shoulders of his own prophet, and with the corresponding sentences written above and below.

But towards the end of the fifteenth century the public, though still under the domination of Gothic traditions, had imbibed a considerable amount of Renaissance taste and feeling, which manifested itself in a desire for a more free and natural style of drawing. There therefore arose a demand for fresh work in enamel, more in accordance with the new style of art that was gradually gaining ground. In response to this demand, a totally new mode of enamelling was invented, derived partly from the jewellers' work of the day, partly from the art of glass window painting. It is not clear where this invention was made, but it seems to have been introduced simultaneously in Italy, in Germany, and in France.

The new method consisted in covering thin plates of metal with layers of coloured enamel, no longer melted into the recesses of cloisonné or champlevé, but made to flow over the whole plate or parts of it, and in gradations of thickness, whereby gradation of tint was obtained. The town of Limoges again took the lead in this manufacture, and retained its supremacy so effectually that this sort of work is known as "Limoges enamel." It seems certain that the members of the Penicaud family who practised the art of glass painting were the first to execute this new work in Limoges. They took as their models the coloured pictures which adorned the breviaries of the day, and which were in the Flemish style, with French influence.

"Nardon" (that is to say "Leonard") Penicaud was apparently the eldest of the family. His method was—having covered a thin plate of copper, or bronze, with a layer of opaque-white enamel—to paint with a brush or pen, in dense black, upon the surface so obtained, a picture in strong outline, so as to resemble one of the coarse, strongly cut woodcuts of the period. This was fired, to melt the surface of the enamel and fix the black outlines, much in the same way as a black print upon a piece of china. To do this, the charcoal furnaces then in use by glass painters were no doubt employed.

The next step was to cover the drawing with layers of transparent enamel, much as one would colour a woodcut with paint. Nardon

used five transparent colours, namely, two shades of saffre (cobalt), turquoise made from æsustum (oxide of copper), grass-green from crocus martis (iron), and violet from "peridot," or that form of manganese ore which is found near the town of Perigueux, in the vicinity of Limoges.

There appears to have been no transparent red among the colours used by Nardon, except small buttons of ruby placed upon little spots of gold-leaf, to represent jewels, and which were freely sprinkled over the picture.

For flesh, a reddish tinge of violet was used. This was done by painting the faces with a strong tint of violet, made from manganese and iron, and then, after this was fired, working over it with opaque-white in gradations, so as to let the violet ground show through in the thinner parts. This work was called "grisaille," being the name then employed for white-shaded work in painted windows. A thin glaze of violet was sometimes then put over the grisaille.

Metallic gold was lavishly used by Nardon Penicaud, though, owing to the imperfect way in which it was fired, a great deal of it has been rubbed off in the specimens of his work which we now possess; probably he used borax with it—a fatal practice. Not only was this gold painted in on the high lights of the drapery, but it was dotted in stars over the sky, or in curling ornaments or tongues of fire over the backgrounds.

He used red oxide of iron, that is to say, ordinary rouge (crocus martis), to represent blood, and sometimes upon the lips, but he did not employ this rouge otherwise. The principal tone of the colouring is cobalt and turquoise. The effect is most rich and harmonious, though the peculiar purple tone given to the flesh by the violet underground somewhat mars it.

There is an indescribable charm about Nardon's best work, due to the fresh and ingenuous expression of the faces, and the skill with which his simple colours are contrasted and united. The gold also gives delicacy and richness, and, as the designs are usually taken from the very best work of the great Flemish masters, the composition is generally excellent.

During this period, that is to say, the end of the fifteenth century, there seems also to have been executed some of the work known as plique-à-jour. This is done by forming a number of cloisons without any foundation, so as to resemble a sort of grating, into the interstices of which enamel was melted. The effect is that of filigree work, filled up with variously coloured transparent glass. Very few pieces have survived. In the life of Benvenuto Cellini, he relates that Francis I. showed him a specimen, and that he imitated it. There is no difficulty in this work, the method of which will be described hereafter. It is done to a limited extent at present in Russia, but genuine old specimens are exceedingly rare. Work in this style is executed in Geneva and in Sweden.

The progress of enamelling which has been above described carries

us well into the sixteenth century. But a great change was at hand. Under the influence of the revival of classicalism, and in the hands of Carpaccio, the Bellinis, Botticelli and Leonardo da Vinci, painting assumed the most exquisite forms. The Florentine architects infused the spirit of classical ornament into Lombard and Gothic architecture, so as to produce a new style, of which the scuola of St. John the Baptist at Venice or the tombs of the Scaligers at Verona are examples. In the hands of Ghiberti, Donatello, and Lucca Della Robbia, sculpture and modelling rose to new forms of beauty, while the genius of Michael Angelo crowned the work by his majestic creations.

If the influence of the classical movement had stopped here, it would have been productive of unmixed good, and might have resulted in a new and beautiful style, which would have gone on developing for centuries. Unfortunately, however, the taste for classical learning passed due bounds, and took such a hold upon art as to destroy its vital energies. Classical history or fable almost monopolised the subjects of pictures; classical costume became universal. Madonnas were made to resemble goddesses. The dignified angels with prismatic wings of the fifteenth century gave place to infant Cupids. If the nude was to be represented, artists could think of nothing better than the Judgment of Paris, or Perseus and Andromeda, or the chaste Lucretia, who was frequently represented as having prepared for death by divesting herself of all her clothing. The Judgment of Solomon, and the History of Susanna, afforded subjects for courts of justice; the Queen of Sheba coming to Solomon flattered the vanity of a pope or a prince—all, however, being in classical costume.

In desperate endeavour to give some modern interest to this hack-work, it became usual to put the heads of the patrons of arts on the bodies of Æneas, Julius Cæsar, or Alexander; and where modern battles were painted, the combatants were represented either naked or in the dress of Roman soldiers.

Thus began the reign of pedantry, in which art was admired, not because it gave pleasure or profit to the spectator, but because it afforded an opportunity for the display of classical learning. For what interest could the ordinary mind take in such scenes? Who cares about the Birth of Venus, or the Rape of Europa, except as a means of displaying beautiful nude figures? How wearisome, even when done by the greatest artist, appears the well-worn "Fame" upon a tomb, with a trumpet and a wreath of laurels, or "Patience" upon one side of a monument smiling at "Grief" upon the other. Such art as this was deficient in soul. It had ceased to draw its inspiration from the social or religious life and feelings of the people. It appealed no longer to the masses, but only to a group of illuminati. Its roots were severed from ordinary human pleasures and aspirations, and its death became only a question of time. It died hard, poisoning by its corruption the art of all Europe, and ending with death-heads and skeletons on tombs, grinning satyrs, stone clouds, empty

niches, pot-bellied Pompadour furniture, and scrolls of fame with nothing inscribed upon them.

Unfortunately for France, the Renaissance, instead of being allowed to exercise a gradual influence, was introduced suddenly, and in its worst form by Francis I. If he had only patronised Leonardo da Vinci and Benvenuto Cellini, nothing but good would have resulted. As it was, architecture in his time kept clear of the worst Renaissance influence ; but, unfortunately for painting, on his release from captivity, he persuaded Rosso and Primaticcio, two pupils of the school of Giulio Romano, to superintend the decoration of Fontainebleau. Both, especially Rosso, were men of more than mere talent ; but they brought with them the whole paraphernalia of faded classic allegory. In consequence, Dido and Æneas, Diana and Actæon, Venus, Hebe, and the kindred tribe of hackneyed goddesses, sprawled upon clouds, over wall and ceiling, in France for three hundred years. In 1764, we find Cochin representing the Goddess of Medicine interfering to prevent the Fates from severing the thread of life of Madame de Pompadour. They are all upon clouds with Music, Painting and Sculpture, the arts which Madame de Pompadour patronised and did so much to degrade, scattered about in the smoke as if shot out of a volcano. Or again, we have Louis XV. as Apollo, dispelling the mists of infidelity and ignorance. The movement only ended with the introduction of the modern romantic schools.

The most remarkable of the enamellers who adopted the new style was Leonard Limousin, that is to say, "Leonard the Limousin," possibly so called to distinguish him from Leonard Penicaud, or from Leonard Tirny, a distinguished engraver of that epoch. Leonard possessed real talent, and in order to retain his services Francis I. gave him the post of one of his valets, and assigned him a small salary. He worked in every conceivable way. Sometimes he put transparent ground on the copper, sometimes opaque. He rarely used a black ground, except for portraits, and the black outlines in his enamels are usually painted with a brush. Like his predecessors, he had a limited number of colours. In the high lights, instead of putting white grisaille work under the colour, he put it upon the top, which gives a chalky appearance. One is bound to admire many points in his drawing, and, considered simply as an artist, he was the greatest enameller of his day. But his work is often very bad in colour, and frequently exhibits the absurdities and trivialities of the Italian school without its merits. Perhaps one of the reasons why his enamels appear tedious is the constant recurrence of muscular Roman centurions in yellow and blue, with helmets, cuirasses, tunics, bare legs, and sandals. He also caught some of the worst tricks of posing and posturing, and impossible undulations of the body, which distinguish the Italian decline.

There is, however, one department in which he easily occupies by far the highest position—namely, portraiture. His portraits were painted from drawings or pictures of excellent character. These he

sometimes copied on a background of black enamel. The face is put in with thick white grisaille, over a coat of black, and painted up with fine stippled opaque-red colour, made from rouge. The clothes are generally shining black, touched with gold, or else with unglazed black. The back-ground is almost invariably of cobalt-blue, laid over a coat of white. The white enamel used to represent the skin is of the colour and appearance of egg-shell, with a low polish. The general tone is whiter than is natural. The eyes are usually scratched out of a dark back-ground, but the mouth and other lines and wrinkles are painted upon the white. The clothes are often shaded in lines scratched through to a dark under-ground. The delicate white appearance of the skin gives an air of great distinction to the portraits, and harmonises with the black and blue of the dress and back-ground, which fuller colouring would have failed to do. Sometimes a high colour is given to the cheeks with oxide of iron, the tint being dabbed on with a fitch, instead of being stippled. The hair of women is almost invariably auburn, inclining to red, which, however, is accounted for by the fact that the Court ladies of the period wore wigs of this colour over their natural hair.

These portraits are suggestive in the highest degree, and this is a great merit. For with enamels it is impossible to attempt actual imitation of nature. The subject can only be suggested, not reproduced. And therefore, in an art in which so much must of necessity be left to the imagination, we most highly admire the skill of the man who employs the means at his disposal, not in a futile attempt to reproduce nature, but in an effort to give rise to ideas.

The salon art of the eighteenth century is not altogether devoid of artistic merit. Considerable skill is shown in the design, and the drawing is often very delicate; but it was pretty art, not great art. It was endurable when executed by artists, but it became wretched when it fell into the hands of hack-workmen, and when printed designs were substituted for hand-work. This led to its abandonment. The factories at Chelsea, Battersea and Bow were removed from London, and enamel upon metal ceased to be practised. And it was no great pity, for the noble art had now been completely degraded, and there remained only one more depth to which it could sink, which was attained when the mode of applying it to iron was discovered, and the walls of every railway station covered with detestable enamelled advertisements.

Meantime, however, there had been springing up a new application of the art in the shape of miniatures, painted upon enamel and fired. This was only a development of the portraits of Leonard Limousin, but in style it resembled miniature-painting. A splendid collection of such work is to be seen at South Kensington Museum. It was a perfectly legitimate branch of art. A large portrait is more satisfactory than a miniature, but a lover cannot take it into battle on his breast; and a desire for small portable portraits of our friends has given rise to a beautiful development of enamelling. The man to

whom this is chiefly due is Petitot, a jeweller of Geneva, who migrated to Paris, where he executed a great number of most beautiful miniatures.

I now pass to the mode of making the enamel. I do not propose to describe the various recipes that exist. I will only say that the simplest method is the best. For whereas in order that a glaze may not craze upon china or earthenware, it must be most carefully compounded to suit the substance upon which it is placed; any glass that is easily fusible may be melted on to, and will adhere to copper.

For cheapness in glazing earthenware, the raw ingredients of glass are mixed together, sometimes with a little fritting or half melting, and are put on in this raw state. It would be better, of course, to use a well-made glass. For enamelling on metals which cannot be exposed to very great heat, it is necessary to compound the glass beforehand.

As in art work the expense of the material is no object, the enameller will begin with good flint glass. This, as you know, is what is used for the lenses of optical instruments and for cut table-decanters, in which the high refractive power due to the lead gives lustre and the colours of the rainbow. Sham diamonds are also made of very heavily leaded glass.

We therefore mix powdered flint glass with oxides of various metals and melt it. Two or three hours' heat is enough to incorporate the mass. It is then poured out into cakes upon a surface of greased iron, and marked while hot with the maker's monogram.

There is one colour that is so interesting that I must specially call your attention to it. If to a glass, about $\frac{1}{1000}$ of its weight of chloride of gold is added, together with some red-lead and an oxide of uranium, tin, iron, or some electro-negative metal, then, when melted and poured, it remains white.

But if it is then re-heated, it suddenly becomes crimson. The reason of this curious change is not known, but the subject was investigated by Faraday.

We now come to the method of putting the enamel upon the metal. To do this, it must be pounded finely and washed. For if the grains are too fine, bubbles of air are entangled in the mass and the transparency is spoiled. Therefore, by elutriation, the finer particles are removed, and only those left which are about $\frac{1}{100}$ of an inch in diameter, like sand.

The plate being cleaned, the enamel is put on wet, smoothed and patted down, dried over a hot plate, and then put into a muffle-furnace at a temperature of about 1200° Fahr.—a nice clear red. There are a number of details which have to be observed as to the thorough drying of the plate and the method of manipulation, with which I will not weary you. I have dealt with them at length in a little work which I have published.

If the copper plate is thin, then, if the enamel is only put on one side, the unequal contraction of the glass and copper causes it to fly

off. To prevent this, the enamel is put on both sides, being made to adhere to the under side with a little very weak gum. In this condition the enamel will stand.

The plate is rarely perfect the first time, it usually wants patching.

The next process is to put on paillons. The idea of putting on pieces of gold-leaf, and covering them with a coating of glass, was very old, and the process had long been used by the makers of glass goblets, and in the manufacture of mosaics. It is very simple; pure gold-leaf is taken, about 25 times as thick as that used for gilding picture frames. It is stuck down upon the enamelled surface with gum, having previously been pricked all over with very fine needles, to allow the gases, formed by the burning of the gum, to escape. This is fused well down upon the surface. Then over it a drawing may be made with a paint composed of black enamel. Some glass, finely ground up with black oxide of iridium, is the best. This also is fired.

Then, over all, a coating of coloured transparent enamel is put, so that the gold paillons shine through it.

Next, the grisaille must be painted. Grisaille consists of glass, made of an opaque white with oxide of tin. It is ground up very fine, with paraffin oil, and applied like a paint. Being semitransparent, the high lights are thickest. Four or five paintings are necessary to get proper gradations. Then over the grisaille are put tints of enamel.

In this stage the work is apt to appear dull and confused. It is difficult to lay the colours on with precision, and therefore, to give point and emphasis, it is desirable to touch up the work on the surface with gold. Common shell-gold, as sold for miniature painting, does very well, if pure. Unfortunately, it is rarely to be obtained. When fired the gold may be burnished.

The process I have described is in its essence the same for all sorts of enamelling, but I may fitly conclude with a few remarks upon the making of jewellery.

The origin of the wearing of jewellery is very probably to be sought in savage countries, where it was used as a method of indicating rank and displaying wealth. Precious stones originally seem to have been worn as amulets, or as antidotes to poison.

Jewellery is still chiefly employed as a mark of social distinction and wealth, and hence is only made of the precious metals.

Unfortunately, the feeling which dominates our minds, in purchasing jewellery, is too often a desire to get something that will make a good show for the money, and impress our friends. When a wedding present has to be bought people say, "Let us give them something that looks as if it had cost a good deal." It has always been so, and always will be so to the end of time. But this general desire for showy work is very destructive of art. It is the cause why jewellery is stamped out by the thousand at Birmingham.

The present work is deplorable. Can anything be more hideous than the designs which are contained in the Christmas advertisements of Bond Street shops?

The setting of diamonds, also, has no other aim than to display the value of the stones.

Jewellery work in the sixteenth century had higher aims. The objects were made interesting. This will be abundantly clear to any one who will pay a visit to the collection of jewellery at the British Museum recently bequeathed by Baron Rothschild.

Jewellery should be executed in fine gold only, or, if strength is required, then in pure gold backed by stronger metals. The simplest way to execute it is to make a model in wax or clay, and then mould the gold upon a form of fusible metal made from the model. To prevent the lead from adhering to the gold, it should be well covered with black-lead, or better still, an electrotype mould can be prepared. A little tiny furnace is all that is needful, and the necessary tools can all stand upon a tea-tray.

I observe, with regret, that every year the amusements of our country seem more and more to be tending in a non-constructive, if not in actually a destructive direction. The base mechanic is tolerated with a slight touch of pity, if not of disdain. From the Universities down to the Board Schools, knowledge appears to be valued in proportion as it is divorced from practical utility. Let us try to counteract this unhappy tendency by promoting, to the utmost of our power, a knowledge and practice of useful arts, such as enamelling and jewellery, which may be practised by our own firesides.

[H. H. C.]

GENERAL MONTHLY MEETING,

Monday, March 4, 1901.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Francis Henry Anderson, M.D.
Alfred Baldwin, Esq. M.P.
Sir William J. Bell, LL.D. D.L. J.P.
Robert Macfarlane Cocks, Esq.
William Duppa Crotch, Esq. M.A.
Rosser Samuel Dean, Esq.
Lady Farrer,
Major-General Viscount Frankfort de Montmorency, K.C.B.
Gustavus Hartridge, Esq. F.R.C.S.
Captain Thomas Bridges Heathorn, R.A.
Lady Hope,
R. H. Household, Esq.
Louis Stromeyer Little, Esq.
Frederick Louis Lucas, Esq. M.A.
Mrs. Makins,
Miss Edith M. Marindin,
Francis Owen, Esq.
Charles Schiff, Esq.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Mr. Hugh Leonard, F.S.A. for his donation of £100, and to Sir William J. Farrer for his donation of £50, to the Fund for the Promotion of Experimental Research at Low Temperatures.

The Special Thanks of the Members were returned to Mr. Hugh Spottiswoode, and to the Directors of the *Sphere*, for their present of the original of the double-page engraving of Sir Robert Ball lecturing to Children at the Royal Institution on "Great Chapters from the Book of Nature," 1900.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

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WEEKLY EVENING MEETING,

Friday, March 8, 1901.

Alexander Siemens, Esq., M. Inst. C. E., Vice-President, in the Chair.

W. A. Shenstone, Esq., F.R.S.

Vitrified Quartz.

Although the great improvements introduced into the art of glass making by Abbe and Schott have led to such marked advances in microscopy, thermometry, and in other departments during the last quarter of a century, glass remains unsuitable for many of the purposes to which we put it, and there is still a real need for some plastic material more infusible, more insoluble, more fully transparent, more elastic and more stable under changes of temperature than glass.

Such a substance exists in the form of vitrified quartz, or vitrified silica, as I shall prefer to call it.

Vitrified silica was made first in 1839 * by M. Gaudin, who spun threads of it by hand and noticed their flexibility, made it into small very hard pellets by dropping fused quartz into cold water, and observed that it was inactive to polarised light.† It was rediscovered in 1869 by M. A. Gautier,‡ who made capillary tubes and spirals of silica, and exhibited them at the Paris Exhibition in 1878, but who failed to obtain larger objects even with the aid of the electric furnace. Finally, it was discovered yet once again in 1889 by Professor C. V. Boys, who used the torsion of "quartz fibres" for measuring small forces, produced fine tubes and small bulbs from vitrified silica, and who was the first to fully recognise the great value of this remarkable substance.

As all who are here to-night are not chemists, I may remind you that quartz or rock crystal has for some time past been used by spectacle makers and in the construction of optical instruments, and that it is a form of oxide of silicon,§ a compound which is very familiar in the forms of sand and flint. It is occasionally found in magnificent masses, and our chief source of supply is Brazil, where it occurs in large fragments like those on the table before us.

Quartz itself exhibits many of the desirable qualities enumerated

* Comptes Rendus, viii. 678, 711.

† A more recent observation made by Professor S. P. Thompson confirms this.

‡ Comptes Rendus, cxxx. 816.

§ Silicon was discovered by Berzelius in 1823.

above. It is hard, transparent to the ultra-violet rays, difficult to melt, a good insulator, and insoluble in most solvents, but it bears sudden changes of temperature very badly, and therefore it is not easy to manipulate it at high temperatures. When it has been vitrified by heat, however, it becomes much more tractable, and in the vitreous state (vitrified silica) it is not very troublesome to deal with.

It is about this "vitrified silica," how to prepare it and fashion it into apparatus when plastic, and about its properties and uses, that I am to address you to-night.

The first obstacle met by those who wish to make vitrified silica is caused by the tendency of quartz to splinter. It will not bear contact with a flame. As you see, when a piece of quartz is thrust into a flame it cracks and falls to pieces, and the fragments again break up when similarly treated. Consequently it was very difficult for the pioneer workers in silica to soften it in the flame. It is true that if the quartz be broken small and heated to redness in a crucible it becomes more easy to manage, but even then it gives much trouble, and I should not like to say how much my first silica tube, which had a capacity of about 5 cubic centimetres, had cost me for oxygen and labour when it was finished.

Fortunately, we have found that we can make non-splintering silica by heating quartz in small fragments to about 1000° C., and throwing them quickly into cold water. As you see, it then becomes white and enamel-like, and after the treatment has been repeated, the product, though still in masses, may be thrust suddenly into the hottest part of an oxy-hydrogen flame without causing it to splinter to the slightest extent. The preparation of this non-splintering silica constitutes the first stage of the process I am about to show you.

Another difficulty is connected with the oxy-gas burner. Vitrified silica only becomes sufficiently plastic for our purpose when it is heated above the melting point of platinum, and it cannot be heated sufficiently in all parts of an oxy-gas flame. What we want is not so much a very large flame as one which presents a very hot spot (this is situated just beyond the inner blue cone of the flame). After trying all sorts of burners I have concluded that the "mixed gas" jets give the best results, and the injector burner of Mr. Jackson of Manchester is decidedly the best I have met with.

The first step in the process of converting the white enamel-like non-splintering silica into tubes and other vessels consists in heating the ends of two small fragments of the material held in platinum forceps, pressing them together whilst hot till they adhere, adding a third lump to these, then a fourth, and so on until a rough rod has been made, and then reheating sections of this rod and drawing it out into finer rods about 1 mm. in diameter. In doing this care must be taken to heat each fresh mass of material slowly and from below upward in order that there may be as few bubbles as possible in the vitrified product.

A few of the fine rods of silica are next bound round a stout

platinum wire, or twisted into a spiral whilst soft (Boys' and Dufour's method), and heated in the flame till the sides of the rods adhere. The uncouth tube thus produced is reheated, drawn out, and closed at one end; a bulb is blown on the closed end in the usual manner, and this, when again drawn out, gives a fine and fairly regular tube which can be lengthened by adding silica to one end of it, blowing a new bulb from this and drawing it out as before.

The enlargement of the small bulbs was, at first, rather difficult. My earliest attempt consisted in adding a small lump of silica to one end of a bulb, softening this in the flame and expanding it by blowing. It is not impossible to succeed in this way, though the vessels so produced are apt to be uncouth in appearance. But the process is unsatisfactory, owing to the fact that often the thinner parts of the bulb which immediately surround the mass to be expanded become hotter and softer than the latter. When this happens the bulb bursts, and as it can only be repaired by the addition of fresh lumps of silica, the process is apt to be very tedious and expensive. After many failures, it occurred to me that I might develop the bulbs by applying thin rings of silica as shown in Fig. 1, heating them until they begin to spread, and then expanding them by blowing. This method gives satisfactory results. By it we can produce long tubes and other apparatus like those exhibited to-night, if not at a very quick rate or very low cost, yet with certainty and very much more quickly than before.

When once a tube of silica has been made, it can be worked in the flame as easily, though not as inexpensively, as glass. Such a tube can readily be thickened by adding fresh rings of silica, and it can be drawn out to various degrees of fineness and sealed hermetically, whilst all kinds of joints can be made easily. In one respect silica is easier to work than glass; it never breaks when thrust into the flame and finished apparatus needs no annealing.

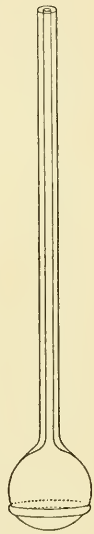


Fig 1.

One precaution must be taken. The eyes must be protected by black spectacles, and the glass of which these are made must be very dark; so dark that white hot silica does not look very bright when viewed through it.

I have spoken of silica as being easy to work, but I do not mean you to understand that it is easy to do what you see Mr. Lacell doing to-night. It is not easy to perform any operation of this sort with his wonderful precision, and especially it is not easy to work under the conditions enforced upon him now, for he can see nothing of the effects he produces and must adapt his manipulation to my remarks, although he can hear the latter only very imperfectly.

The Properties and Applications of Vitrified Silica.

Vitrified silica is harder than felspar, but less hard than chalcedony. When cut with a file it can be broken like glass. Its conducting power for heat is about equal to that of glass. Mr. Boys has shown that, even in an atmosphere saturated with moisture it is a very good insulator. Its density* (2·21) is decidedly less than that of quartz (2·66), and very nearly as low as that of ordinary amorphous silica. Its optical properties have not yet been fully studied, but its approximate index of refraction has been determined by Professor S. P. Thompson, by means of a small prism cut for the purpose by Mr. Hilger; it is decidedly less than that of quartz.

The melting-point of silica is not known, and it is plastic over a considerable range of temperature. When a platinum wire embedded in a thick tube of silica is heated from outside by means of an oxy-gas flame the platinum melts and runs at a temperature at which the silica retains its shape.

The rate of expansion of vitreous silica has been studied by H. Le Chatelier,† and recently by Professor Callendar. The former finds its mean co-efficient of expansion between 0° and 1000° to be 0·0000007, but from the manner in which his material was prepared I think it probable that it was not quite pure. Professor Callendar has, within the last few days, examined the behaviour of a rod of pure vitrified silica prepared for him by my method. He finds its mean co-efficient of expansion to be 0·00000059, which is only $\frac{1}{17}$ as great as that of platinum, and much smaller than that of any similar substance that has hitherto been studied. He finds also that the expansion of vitrified silica is exceedingly regular up to 1000°, and that if not heated above 1000° the rod returns very exactly to its original length when cold. Beyond 1000° he found a slight permanent elongation, although the rod was under compression. Professor Callendar was able to carry his experiments up to 1500°, which is very satisfactory, for it shows that vitrified silica remains solid, or practically solid, at this very high temperature; this is an important observation, as less carefully conducted experiments made by others had led me to fear that the substance might be found to become slightly plastic even at as low a temperature as 1000°. Above 1000° the rate of expansion diminishes rapidly, changing to a contraction at about 1200°.‡ On cooling from 1500° to 1200° it expands.

Fine rods of silica and also quartz fibres are apt to become rather brittle after being heated to redness. But we have not at present detected this defect in the case of thick tubes or rods.

The transparency of vitrified silica to ultra-violet rays has been

* This was determined by my pupil, Mr. T. Pears, the silica used contained a few minute bubbles.

† Comptes Rendus, cxxx. 1703.

‡ H. Le Chatelier's curve, see Fig 5, shows a similar contraction, but commencing at a somewhat lower temperature.

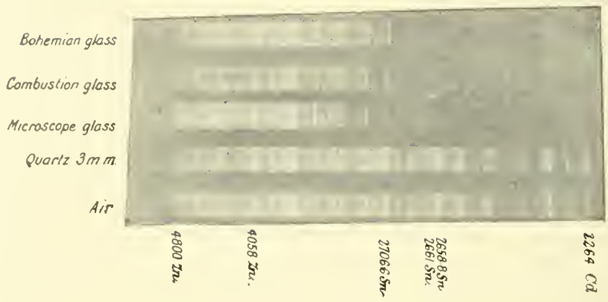

Bohemian glass
Combustion glass
Microscope glass
Quartz 3mm.
Air
4800 Zn.
4058 Zn.
2706.6 Sn
2658.8 Sn
2661 Sn.
2264 Cd

FIG. 2.

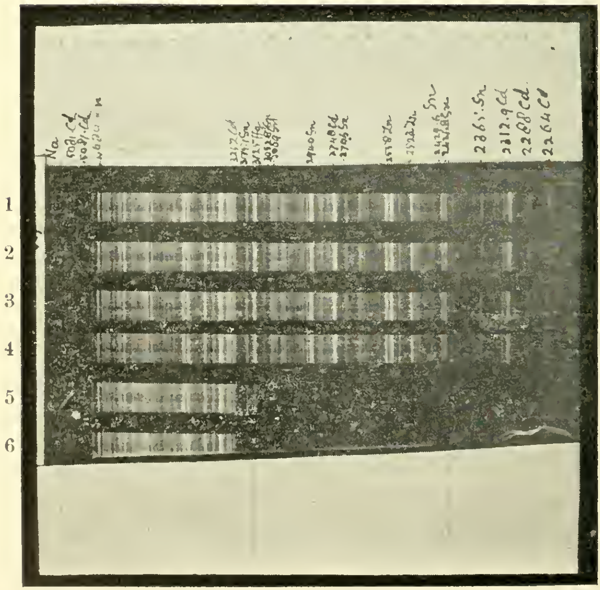

Na
2265 Sn
2312.9 Cd
2288 Cd
2264 Cd
1
2
3
4
5
6

FIG. 3.

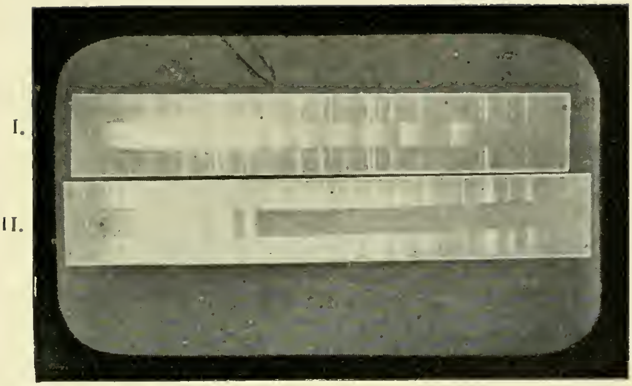
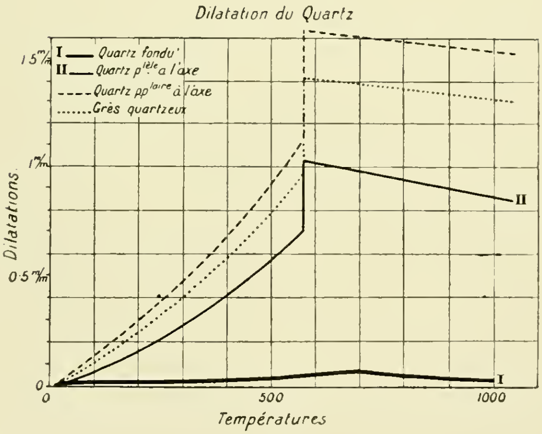


FIG. 4.



Dilatation du Quartz
I —— Quartz fondu
II —— Quartz p^lèle à l'axe
- - - Quartz pp^laire à l'axe
...... Grès quartzeux
1.5 m/m
1 m/m
0.5 m/m
0
Dilatations
0
500
1000
Températures
II
I

FIG. 5.

carefully examined by Dr. A. Wynter Blyth, to whom I am greatly indebted. His results may be gathered from the accompanying illustrations based upon his photographs.

Fig. 2 gives the results of photographing the ultra-violet end of the spectrum of the light produced by electric discharge between poles made of an alloy of mercury, zinc, tin, and cadmium, after it had passed through various kinds of glass, a sheet of quartz, and air respectively.

Fig. 3 gives a similar set of photographs in which the transparency of air (1) is compared with that of plates of quartz, 3 and 4 mm. thick (2 and 3), and with plates of vitrified silica (4), soda glass (5) and flint glass (6) of equal thickness, which were prepared for the purpose by Mr. Hilger. It will be seen that vitreous silica, like quartz, is as transparent to ultra-violet radiations as air itself.

The fourth figure compares the light emitted by vacuum tubes of vitrified silica (I.) and glass (II.) respectively. The bands shown were obtained from tubes into which traces of hydrocyanic acid had been admitted. In this case, as before, it will be seen that silica is very superior to glass and practically equal to air in transparency.

The most remarkable property of vitreous silica is its behaviour under sudden changes of temperature. We have seen already that tubes of it may be plunged suddenly into an oxy-gas flame without injury, and I have mentioned the fact that apparatus made of silica needs no annealing. But this is not all ; we may drop water on a white-hot silica rod or plunge a white-hot silica tube into cold water or even, by Professor Dewar's kind aid, into liquid air, without injuring it in any way whatever, indeed, experiments seem to show that the material gains very distinctly in regard to its elasticity after this treatment. I need hardly point out how convenient tubes of such a material will be to chemists or how many spoilt lecture experiments may be avoided in future, by those who possess a silica tube.

This last property of silica and the splintering of quartz find an explanation in the results obtained by H. Le Chatelier * and by Callendar. These observers, as already explained, have shown that the rate of expansion of silica is exceedingly low, and moreover that at temperatures much above 1000° it contracts when heated. Under these circumstances it follows, first, that the strains set up in silica when it is suddenly heated or cooled are comparatively small in amount, and secondly that if, for example, vitrified silica be cooled from 1500° to below 1000°, the strains set up at the earlier stages of the change will tend to neutralise those produced subsequently. These facts enabled Le Chatelier to predict the indifference of vitrified silica to sudden variations of temperature, but the actual phenomena had been observed and exhibited in this country previously.

The behaviour of quartz under changes of temperature is also very peculiar. This was studied by Le Chatelier.† From his curves which are given in Fig. 5, it may be seen that this form of

* Comptes Rendus, cviii. 1046, and cxxx. 1703.

† Ibid. cviii. 1046.

silica expands quite regularly, and much more rapidly than vitreous silica up to 570°, but that at this temperature a sudden and considerable expansion takes place which is followed by a steady contraction on further heating.

One of the most important fields in which vitrified silica is likely to be useful is that of thermometry.

Owing to its small co-efficient of expansion, the degrees of silica-mercury thermometers will be of greater length in proportion to the volumes of their bulbs than those of glass instruments. Owing to its high melting-point it should be possible to employ it with advantage for the measuring of high temperatures by replacing the mercury by tin or other metals, as has been done by M. Dufour.* And whilst the great elasticity of vitrified silica suggests that the fixed points of silica-mercury thermometers will be much more stable than those of glass-mercury instruments, the impunity with which it may be suddenly cooled from high temperatures promises obvious advantages.

Again, the high melting-point of silica should make it very valuable for use in platinum thermometers, and I exhibit such a thermometer to-night which has been fitted up for Professor R. T. Glazebrook. But as the applications of vitreous silica in thermometry are still under investigation, I will not dwell on this part of the subject, except to add that as glass reservoirs for air thermometers have proved disappointing, I am not without hopes that the new material may prove helpful in that department also.

We have not yet had time to examine the behaviour of silica with solvents, but, if it acts like other forms of the same compound, it may be expected to replace platinum for some purposes, as, for example, for condensers for the preparation of pure water, and vessels of silica probably would be much more suitable for use in exact experiments on the freezing-points and boiling-points of dilute aqueous solutions, than the glass tubes now often used for such work; but, of course, silica vessels will be very susceptible to the action of alkalis. Finally, silica may be expected to prove more suitable than glass for use in researches on pure gases owing to the qualities of its surface, and for use in experiments concerning the behaviour of gases at high temperatures. We have already one small application of silica to research in this latter field to put upon record.

It is well known that nitrogen and oxygen enter into combination under the influence of electric discharge, and Sir William Crookes † has shown that oxides of nitrogen are present in considerable quantities in the flames which accompany the discharges of large induction coils; but although various observers have reported indications of the presence of nitrous fumes in the neighbourhood of flames, the forming of an oxide of nitrogen from nitrogen and oxygen alone, and without the intervention of electricity, has not, so far as I am

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* Comptes Rendus, cxxx. 775. † Chem. News, lxv. 301.

aware, been unmistakably established. Therefore, it is interesting to record the fact, first observed by Mr. Lacell, that nitric peroxide is produced by heating a mixture of oxygen and nitrogen above the melting-point of platinum in tubes of silica. It is easy to obtain a gas showing a distinctly yellow colour and exhibiting the reactions of nitric peroxide in this way.

Of course, vitreous silica is not entirely without defects. Unfortunately it becomes slightly permeable to hydrogen as platinum does, though to a less extent, at about 1000°.* It is attacked when hot by basic oxides. It may be heated to about 960°, in contact with copper oxide without injury, but at higher temperatures it is attacked. It may be heated more strongly with ferric oxide, but quicklime attacks it at a bright red heat. It is evident that caution must be exercised when it is in contact with basic oxides or alkaline solutions. When one first fashions vessels of silica before the flame the vessels exhibit to a greater or less extent a phenomenon resembling the devitrification of glass. They become covered with a white opaque crust. This is easily removed by reheating, provided that the tube is kept scrupulously free from dust and dirt. If this precaution be not taken the appearance of the vessel may be spoilt permanently. The earlier observers attributed this phenomenon to the volatility of silica. My impression is, that it is connected with the minute traces of alkali metals which are present in most Brazil pebble, and which are usually burnt off in the processes I have described.

What I have told you to-night has shown you that in several respects vitrified silica is as much superior to the best glass, as Jena glass is superior to more ordinary specimens, and that the progress made during the last few years will enable investigators to employ vitreous silica much more widely in the future than has been possible in the past. At the same time it is evident that the processes for producing it are still in their infancy, that there is much more to be done, and that further progress can only be made at considerable expense.

In concluding my remarks I wish to express the great obligation I am under to my friend Mr. Lacell. You will have discovered for yourselves that the chief burden has been upon his shoulders to-night, and that without the illumination provided by his precise and beautiful manipulations my discourse would have been but a dry affair. Also I must add that the cost of the work at its later stages has been partly defrayed by a subsidy from the Government Grant Fund of the Royal Society.

[W. A. S.]

* Villard, Comptes Rendus, cxxx. 1752.

WEEKLY EVENING MEETING,

Friday, March 15, 1901.

Sir William Crookes, F.R.S., Honorary Secretary and Vice-President, in the Chair.

Major Alfred St. Hill Gibbons, F.R.G.S.

Through the Heart of Africa from South to North.

It was not without some misgivings that I accepted the invitation of this Institution to read a paper on my recent experiences in Africa. I had already referred to the organisation and route of the expedition; described its prospective objects and their realisation; gone with some detail into the hydrography of the Zambesi and Nile districts with a view to impressing on those whose duty it is to direct and develop the more primitive dependencies of the Empire, what an effective and economical developing force these great river systems supply; and related some of those lighter incidents which serve the twofold purpose of illustrating native method and character, and of making it less difficult for the audience to keep awake.

It is true that 13,000 miles of route offers a considerable field, but it is none the less true that the records of the vast majority of days are scarcely worth the paper and ink expended on them by virtue of their incidental sameness or unimportant character.

When I first decided to take up African exploration I considered it advisable to select a single district and work it thoroughly. I was quite aware that long straight lines through the continent look much more imposing than twice their distance within a limited area, also that it is on such lines that the relative merits of explorers are determined, but I had no wish to waste my time in comparatively useless labour. In such journeys the traveller, passing rapidly as he does from one district to another, bases his views on his first and only impression, whereas the second or later impressions are not infrequently more in accordance with fact. It is thus—so it seems to me—that many of the apparent discrepancies which sometimes puzzle the reader of two books on the same country, are to be accounted for.

Marotseland was little known seven years ago, and excepting part of the Zambesi or Kabompo rivers, and the West Coast trade routes, was untrodden by explorers, so I selected this as my field of action. Besides this, Marotseland was recognised as falling within the British sphere, a further condition which, to me, supplied a *sine quâ non.*

First, I propose giving a general description of the character of the countries along my routes from the Cape to Egypt, so far as it

is apparent to an ordinary observer unsupported by the special knowledge of the botanist or geologist. Later, I will trace part of my journey through Marotseland to the Zambesi source, eastward along the Congo-Zambesi watershed, thence by lakes Mweru, Tanganika, Kivu, Albert Edward, and Victoria to the Nile and Egypt.

From where the Hex River Mountains, rising out of the rich cultivated plain which extends for some 100 miles from Capetown, culminate in the Great Karroo, to where the ground falls away within the last day's march to the Murchison Falls on the Victoria Nile, my journeys northwards have seldom taken me over ground of a lower altitude than 3000 feet above the sea-level, while probably 90 per cent. of my route has been through country ranging from 3500 feet to 5000 feet.

Can a dry land surface at such an altitude be unhealthy for Europeans? My impression is that, with regularity of living and ordinary care, it cannot; and I support my opinion by my own experience. Throughout my travels I have never been stopped a day by fever, nor have I lost a single porter from death. I have twice suffered from dysentery—once severely, and have only had one cold in Africa between 1890 and 1900, whereas in England I consider myself fortunate if I get off with four in a winter. With well-built houses and good diet I am convinced that the earth offers few healthier sites for European settlement than the higher parts of the plateau of Africa.

Immediately after the first rains the Karroo veldt becomes a veritable carpet in colouring. The latent richness of the soil responds with incredible promptitude to its absorption of water—the only factor necessary to invest it with first class wealth-producing properties. Flowers of various tints spring into being, and the arid monotony of what appeared only a few days earlier to be but a parched desert, studded here and there with abrupt barren kopjes rising like islands from the sea bed, has become a rich pasturage for herds of cattle and flocks of goats and sheep. Here and there at extended intervals a cluster of well-grown trees surrounds a homestead—a modest indication of undeveloped wealth and a prayer for enterprise from the dry veldt surface. Wells and dams will one day convert the Karroo and the grass plains beyond into one of the richest and most habitable places of the earth.

From the Orange River to Mafeking the Karroo gives place to modified undulations, growing grass in one place and stunted scrub in another, but trees larger than a well-grown gooseberry-bush are seldom encountered in the open veldt.

Two miles north of Mafeking a scattered savannah forest of thorny acacia is encountered, and although trees henceforward vary in character, forest land extends with one or two trifling exceptions as far as the borders of the north African desert. Undulations—sometimes of a light clay, sometimes of gravel—characterise the route through the east of the Bechuanaland Protectorate. These are

covered for the most part with acacia—*Ac. horrida*, *Ac. giraffa*, and others—or in districts with the "mopani," a tree whose leaf when viewed from a short distance is not unlike that of the beech in appearance, and whose dry dead leaves similarly cling to the tree when the foliage of the succeeding year bursts. Through the Kalahari desert the same class of vegetation thrives, though the mopani predominates. The soil here is of a light yellow sand, and to trek through this inhospitable country, teams must be strong and wagons lightly loaded—even then the struggling oxen, parched and half choked with dust, can with difficulty draw the wagon through the shifting sand at a greater rate than one mile an hour. After five weeks of such continuous labour it can be well imagined with what feelings the Zambesi with its 500 yards of deep clear water is approached by both man and beast.

This grand river, supplying as it does a natural boundary between South and Central Africa, forms in more ways than one a divisionary line. To the south the main rivers alone carry water during the dry season; to the north the smallest tributary has a running stream throughout the year. The yellow light sand of the Kalahari gives place to a heavy white sand in the Zambesi basin; thornless trees of varied and pleasant foliage replace the acacia and mopani; the natives differ in type and custom; and in several cases the river supplies a boundary to the habitat of fauna. Droughts are of frequent occurrence in South Africa, but in Marotseland the rainfall is remarkably stable, varying but little annually from 33 inches.

Throughout the Upper Zambesi basin the vegetation remains the same in general character; trees from 30 to 40 feet high offer good shade, and the thorny undergrowth of some countries is conspicuous by its absence.

The rivers have quite a character of their own. Clear streams wind between clean-cut banks through flat alluvial valleys 1 to 800 yards wide. These valleys are hemmed in by forest-clad undulations, which increase in size and altitude in proportion to their distance from the Zambesi River, the ground rising gradually to east and west till it attains an altitude of 4000 feet and over. As the Zambesi sources are approached, the soil has changed from white sand to red, light clay, though the vegetation remains much the same, with occasional local variations. Thus, on the watershed, at a height of 5000 feet above the sea-level, bracken is common, while I ate raspberries from bushes to all appearance similar to our English plant.

These red clay undulations—except where broken by the mountainous ranges of Tanganika and Kivu—extend to the neighbourhood of the Victoria Nile, when they give place to a yellow sandy clay, to be in its turn replaced by the desert sand of Egypt.

In the valley connecting Tanganika with Kivu, and thence northwards into Toro, the euphorbia and acacia predominate. In Unyoro and down the Nile to the borders of the desert the acacia holds as

prominent a position as it does in South Africa—and, in fact, the countries themselves bear a close resemblance one to the other. Nor is this resemblance confined to vegetation. I noticed many small birds in the North which had counterparts in South Africa. So, too, the giraffe, whose habitat in the South is limited by the Zambesi, once more appears on the Nile; so does the secretary-bird. The North and South African ostrich are identical, though a different species intervenes in East Africa. It has been assumed that the so-called white rhinoceros, *R. simus*, never existed north of the Zambesi, and in the early nineties naturalists and sportsmen lamented his total extinction, though it has since transpired that a very limited number still exist; yet I killed and brought home the skin and skull of one of these animals, which I chanced on near Lado, on the Nile. His measurements and outer appearance coincided in all respects with *R. simus*, though I understand he differs slightly in minor points, affecting teeth and skull bones.

In April, 1899, I was travelling along the well-defined bed of a river about 100 yards wide and 20 feet deep. A cursory examination showed that this bed was not habitually waterless; yet the fact that at the end of the wet season, when vleys and streams were full, its deepest depressions were perfectly dry, seemed to suggest that normal conditions did not apply in this case. In two days' time the mystery was solved, though in the process of solution I had perforce to wade knee-deep for three-and-a-half days.

It appears that at the end of the wet season the Okavango overflows its eastern bank in 19° S. latitude, and I was meeting the first rush of water. The flat country becomes inundated for miles until the many subsidiary streams thus formed converge into one and escape along the bed, already described, into the Kwando River. Thus the Okavango for some three or four months in the year is actually connected with the Zambesi system by this considerable stream of water, which is known among the natives as Mag'wekwana. It is conceivable that this river has at one time been a Zambesi affluent, and that, on the old bed being choked by drift and sand, the river has made a new one in its present course. However, be this as it may, the diversion of the Okavango through the Mag'wekwana into the Kwando would call for no greater engineering skill than the cutting of a new, or the re-cutting of the old channel, as the case may be, through the few miles which separate the Okavango from the well-defined portion of the Mag'wekwana bed. The Okavango, a large river some 300 yards wide in places, flows southward into the Kalahari under present conditions. Here the whole of that strong, deep stream disappears, and is wasted. It may be deemed worth while at no distant date to partially or entirely divert its course to the Kwando, and thus, by water communication, tap the extensive rubber-fields contiguous to the upper Kwito, whose first products are now finding an outlet through Portuguese territory. This overflow occurs in the northern confines of the Maiye country—a South

African tribe under the control of Sekome, chief of the Batawana. These people are very clever basket and mat workers, but what interested one most was the marriage custom in vogue amongst them. It is no more strictly correct to assert that wives are bought and sold in Africa than it is in England. Sometimes, no doubt, pecuniary and other considerations are not altogether absent in either case. Almost every large tribe in Africa has its own way of arranging betrothals and conducting marriage ceremonies. In the case of the Maiye, the young aspirant takes beads to the mother of the girl. Acceptation means consent. On the wedding-day the bride goes into the forest and hides. Sufficient time having been given to allow her to get thoroughly hidden, the bridegroom, having furnished himself with provisions for two for three days, sallies forth in pursuit. Then a game of "hide-and-seek" ensues. I do not know what happens if the seeker fails to find the hider, but I imagine less difficulty is experienced in bringing the game to a successful issue than might be supposed if only the vast extent of the African forest is considered. After a three days' honeymoon the pair return, settle near the woman's father, and the man becomes a member of his wife's family.

Journeying along the Okavango I passed over a dark yellow sand which grows thick undergrowth—in many places almost impenetrable. As the confluence of this river with the Kwito is reached the yellow sandy soil disappears, and with it the tangled undergrowth, and we enter the white sand country already described as characterising Marotseland. As I had noticed during my journey from the east-coast, so also here, 17° 30′ S. latitude roughly marks the boundary between South and Central African vegetation. In passing up the Kwito I was agreeably surprised with the country generally. The undulations become steeper and steeper, and by the time the 15th parallel is reached might almost be described as hills. These are intersected by the rivers and streams already described, whose alluvial plains are capable of feeding immense herds of cattle, the herbage being not only rich but dense.

The country from 17° 30′ to the neighbourhood of the Kwito sources and for some 200 miles eastwards is peopled by the Mambunda tribe. These are thick-set people and physically above the average. Were it not for that cruel disregard of each for his neighbour—even of his own blood in an adjoining village—added to greed, a second predominating characteristic of the African native, they might be both powerful and prosperous. As it is, the most trifling quarrel serves as an excuse for the raiding of one village by another, with the ulterior object of supplying women and children as articles of barter with the West Coast slave traders. I will give you one instance which came under my notice. I had rested at midday in the midst of a cluster of villages of which Kouwewe was one. In the evening I camped near Chachingi, a strongly stockaded village in the valley of the Lomba, a Kwando affluent. The chief Mine-

chachingi drew my attention to several fresh bullet-marks and twisted arrow-heads in his stockade. It appeared that a few months earlier a man of Kouwewe having possessed himself of an old muzzle-loader sallied forth in pursuit of game, but to his intense chagrin blazed away several rounds without result.

Living in a village, three days to the north, was a distinguished doctor, so to him our disappointed sportsman repaired, and poured out his trouble into sympathetic ears. The doctor assured his patient that he possessed all the necessary medicines to effect a cure. The fee was arranged, and so confident was the doctor in the efficacy of his medicine that payment was deferred until the cure should have been effected. The jubilant sportsman returned to his village, made a number of incisions in his wrist, and rubbed in the medicine as prescribed.

In due course, eager and confident, he shouldered his old blunderbuss once more. But his hopes were to be blighted, for—would you believe it?—not a bullet took effect. Angry and disappointed he vowed he would not pay the bill. A deputy called for the fee, but took back the message—"Your medicine is worthless, so you must do without the payment." A week later the doctor gathered together the warriors from his own and two neighbouring villages for an expedition against Kouwewe. They halted near Chachingi "en route," gazed up at the steep ascent beyond, and felt less and less inclined to toil uphill for a further half day. There is a way out of most difficulties, and these warriors found a way out of theirs—"Why should we toil up this long hill?—here is Chachingi in front of us, he will do just as well." So they set to without further parley and blazed away for three days. But Chachingi held his own, the doctor went home fee-less, and the patient on the hill suffered no inconvenience.

In travelling eastwards from the Kwito between the 14th and 15th parallels, I saw much of the native rubber industry.

Rubber, as is well known, is extracted from many plants in various parts of the world. In Africa alone there are three families which are subdivided into many species.

It is obtained from trees, vines and roots. By root rubber of course I do not refer to the inferior article obtained from the roots of the tree or vine at the expense of the life of the plant.

In the case of the first two the rubber is easily extracted. An incision is made in the trunk and from the wound thus inflicted, a milky fluid (which must not be confused with sap) exudes, and, falling into a vessel placed beneath to catch it, congeals.

In the case of the root rubber the process is much more elaborate. The rubber is contained in straight stems, the thickness of the wrist and less, which run horizontally within a few inches of the earth surface. These underground stems, which sometimes attain a length of 15 to 20 feet, are indicated by a modest little plant not more than 18 inches high, growing smooth glazed leaves 2 inches long with a

maximum width of half an inch. Regardless of size, these stems are torn up, bound together and carried into camp. It would appear that even the care with which the native collects the smallest root does not suffice to exterminate the plant, for three years after every sign of rubber has been obliterated from a district, the little plant is flourishing once more. Apparently the smallest root left in the ground is sufficient to renew the plant. Once in camp the stems are subjected to seven processes before the rubber is completely separated from the fibre and ready for export.

1. They are cut into lengths of about 18 inches and stacked till quite dry.
2. Soaked in water for ten days.
3. Hammered on a block with a wooden mallet until the fibres separate.
4. Dried in the sun.
5. Hammered once more until the rubber congeals.
6. Boiled and cleaned of fibre.
7. Washed in cold water.

It is then tied up in bundles of strips, and eventually finds its way to European markets.

Towards the end of June 1899 I was in Lialui, the capital of Marotseland, paying my third and last visit to Lewanika. I cannot pass without saying a few words about this exceptional native ruler and his people. About 250 years ago, the Aälui, the forefathers of the Marotse of to-day, invaded the broad, long plain known as the Burotse, and, finding rich pasture, settled there. Prior to this invasion the middle Kabompo had been their home; part of the tribe remaining behind still exist under the name of Balakwakwa.

There is little doubt but that they originally migrated from the far north, though Lewanika, from whom I gleaned most of the past history of his tribe, has but a hazy idea of what occurred prior to the Kabompo settlement. The language no doubt will give an important clue, but no white man as yet has had an opportunity of studying it, for the people usually converse in Sekololo, a Sesuto dialect, only using their original language when they wish to make a remark "aside." My friend, Major Coryndon, recently appointed first administrator of Marotseland, gives me an instance of a South African native, who had spent a few years in Mashonaland, understanding Serotse from his knowledge of the Mashona dialect. There are other ethnological facts pointing to a connection between these two tribes which have been construed into evidence that the Marotse are an offshoot of the so-called Mashona, and entered Burotse from the south.

The later evidence I have been able to collect rather points to the fact that the two tribes were in some way connected *prior* to the migratory move from the north, which I believe ethnologists are agreed took place in the case of the Mashonas and most other South African tribes. I may say the Marotse and Mashona are very

different in type; if, therefore, they have been connected one with the other, the relations have been either those of master to slave or, subsequently to separation, the two tribes have been subjected to fusion of alien blood from opposite directions. So much for the origin of the Marotse: their subsequent history is long enough and interesting enough to furnish material for another paper, so obviously there is no place for it here. For the rest, I can only say that the Marotse proper are the best type of African native, both physically and intellectually, with whom I have come in contact; while Lewanika is the most remarkable instance of the possible power of adaptation by a native African to the conditions of a more enlightened civilisation that has ever come under my notice, and I do not even except Khama.

I will illustrate my meaning by asking you to follow us to his house in response to an invitation to lunch. Passing through a well-kept and spacious courtyard, containing the dwellings of his wives, a shelter for the band, and one or two other buildings, we enter an inner court, in the centre of which stands a large, double-roofed house, neatly thatched, and with an 80-feet frontage. An attendant opens a heavy red-wood door, mediæval in character, and announces us. We are welcomed with an easy courtesy by a tall, broad-shouldered man, deep-chested and very black, but with a well-chiselled nose, lips no thicker than those of many Englishmen, cheek and upper lip shaved, hair and pointed beard neatly combed, with an ivory dagger ornament two inches above the right ear. He is clothed in a well-fitting tweed suit and a light flannelette shirt, his whole person scrupulously clean and neat. The reception-room, about 20 feet by 25 feet, contains some half-a-dozen European chairs and a mahogany table. Native matwork in varied pattern decorates the wall. He shows us to a seat, and himself sits in a large, straight-backed arm-chair. Arranged above the chair, in a curtained recess, is a framed portrait. This is not for the gaze of vulgar eyes; the curtain is drawn except on special occasions. It is a portrait of the most widely loved and respected sovereign the world has ever seen—the Queen Victoria. Lewanika had a great respect for our late Queen, and, like so many of her coloured subjects, admired her greatness with something of superstitious reverence. "The Queen must be very great and good," he once said, "for all white men, whether English or not, say only what is good about her."

But now to lunch. We have just returned from washing our hands, and have used toilet soap and a clean towel. The table is laid with knives, forks and spoons, as highly polished and well arranged, and cloth as spotless, as we are accustomed to in well-ordered houses at home, and we take the seats allotted to us. A row of household servants extends from the table to the door and beyond, each within reaching-distance of his neighbour. No standing is permitted in the royal presence, and the approach and departure are effected with bended knee and lowered head. A covered dish appears from with-

out and is handed along the line, each attendant saluting by clapping his hands before touching the royal crockery. Rising only so far as is necessary, the table waiter places it before the king, removes the cover, and Zambesi fried fish is handed round. Then comes roast wild fowl—goose, duck, teal, or other bird. The Marotse eat the flesh of no domestic bird or animal save beef. They know not why; it is a time-honoured custom. The bread is excellent, made of wheaten flour, prepared from grain grown in the king's gardens from imported corn. The third course is usually curded milk eaten with cream and sugar or preserved Californian fruit, to be followed by biscuits and a cup of tea.

You will probably ask, "How comes Lewanika, who until four years ago saw probably less of white men than so many other native rulers, to be so far in advance of his peers?" The answer is that two definite causes have contributed towards this most praiseworthy result—two causes which apart one from the other would have proved equally futile and useless. The first is to be found in the natural character of the man; the second, in outside influence affecting his thought and developing his instincts. Of ancient lineage, he and his ancestors have ruled not only their own people, but other contiguous tribes for many generations. That rule for a considerable period at least has been in advance of the usual monarchical despotism which has raised powerful black empires in Africa to live for a day to the detriment and insecurity of subject and alien alike, only to fall like a pack of cards when a weak ruler takes the reins of power. In Marotseland there exists a definite constitution—unwritten, but real. The eldest sister of each succeeding king shares the royal prerogative. No great state measure can be legalised without her assent, and as her power is co-terminal with her brother's kingship it is her interest to strengthen him on his throne. The king is advised by great officers of state, and any momentous proposal, such as the making of peace or war, is exhaustively discussed in open council.

The country is ruled on feudal principles: some districts are governed by hereditary chiefs, others by deputed governors selected from members of the royal family. These again are subdivided into sub-districts and so forth. Tribute is collected annually and forwarded to the king, and at his court feudatory chiefs from every quarter of his vast dominions of 225,000 square miles—Great Britain is 81,090—are to be seen paying their visits of homage to their sovereign.

Chiefs are tried for any offence with which they may be charged by a court of their peers. I have myself watched one such trial with considerable interest, and was much struck by the liberality of the proceedings. The presiding chief—the king's brother-in-law, with a first grade colleague on right and left, supported by some fifty chiefs of the second grade—briefly charged the prisoners, who submitted arguments in defence of their conduct. Then the junior

second grade chief argued the case on its merits from his point of view, as did each of his colleagues in order of juniority until the senior had spoken. Thus the juniors could not be influenced by the speeches of their seniors. But when the turn of the three big men came, the order was reversed and the senior spoke first, for they were high officers of state, and as such should possess the courage of their own opinions. The court was then adjourned, and the finding was submitted to the king, who passed judgment.

Heredity is the primary mould by which character and intellect are shaped : subsequent influences develop and modify the image so created. Lewanika on inheriting a chieftainship under what is, comparatively speaking, so liberal a constitution, would also in the natural course of events inherit such qualities as had through his forefathers been fostered in the ruler and the recipient of homage, that is a certain width of mind and dignity in manner—qualities which are not conspicuous in the average African. Beyond this, Lewanika has been more fortunate than most in a similar position. Sixteen years ago he acceded to the request of M. Coillard, a French Protestant missionary, to be allowed to establish a mission in Burotse. Since then he has come directly under the influence of this spirited gentleman and Christian man. I have met many missionaries and others during my travels in Africa, but never a more charming personality than my friend M. Coillard. He is one of those rare men who not merely by his principles, but by the way he gives those principles effect, commands the respect and confidence of all men. Though he has not succeeded in Christianising Lewanika, he has strongly influenced him for good, yet no amount of teaching could have made him what he is had he not been a natural gentleman.

Early in September I bade farewell to Captains Quicke and Hamilton, who had returned to Lialui on completion of their respective journeys along the Lungwebungu and Kwando rivers. To complete the map of Marotseland three journeys were still necessary, which would finally place us at points very far apart from one another. Quicke and Hamilton would be only some 700 miles from the west and east coast respectively, while I would be 600 miles from either of them. To "rendezvous" in these circumstances would entail considerable delay, so it was decided that we should take different routes. Quicke and Hamilton would leave Africa by the west and east respectively, myself by the north.

It is with the keenest pleasure that I now, for the fourth time, publicly express my sincere appreciation of the services of those gentlemen who threw in their lot with me in 1898, and so honourably kept their pledge till every object of the expedition had been fully realised. Mr. Weller during our ascent of the Zambesi by steam launch, from the sea to the Guay confluence, and Captains Quicke and Hamilton in Marotseland, rendered invaluable services in circumstances sometimes of considerable discouragement. By the death of Mr. Müller in the earlier stage of our progress, we not only

lost a popular and energetic colleague, but were cut off from our main supplies, which he was bringing forward when seized with dysentery. In consequence, during the last twelve months of our travels, we had to exist on somewhat primitive lines, while all necessary comforts were rotting at Zumbo. Yet in parting with my officers I was able to do so with the consciousness that our close associations had been unmarred by a single unpleasantness or difference, and that each had borne his share of work conscientiously and energetically.

My journey to Nanakandundu was made with a flotilla of native canoes. From here it was my intention to follow the Zambesi on foot, with a view to making the long-delayed discovery of the sources of this most useful and beautiful river. In anticipation of the possible difficulty of engaging porters to follow this route, I obtained five donkeys, through the good offices of Major Coryndon. While my small equipment was distributed among the canoes, the donkeys were driven along the banks.

At Nanakandundu, known locally as Nyakatoro, no boys could be induced to accompany me into the almost depopulated country through which I wished to travel. Many were willing to travel the trade route along the watershed to Katanga, so I had perforce to load up my five donkeys, and, with the four east-coast natives who had been with me throughout, commenced a somewhat tedious journey.

On the Congo-Zambesi watershed the first rains commence towards the end of September, while south of the Kabompo no appreciable amount of rain falls till Christmas. I have watched the river rise several feet at Sesheke long before the appearance of the first shower.

Thus, in early October, when the donkeys were set in motion, the wet season was already on us, and in some ways the journey was more difficult with these four-legged beasts of burden than it might have been with their two-legged cousins. The former, if considerately treated, always do their best; the latter sometimes do not. Still, where innumerable small tributaries flow through spongy, boggy swamps, the porter is perhaps preferable. At all events, after being perpetually employed for four or five weeks in bridging streams, corduroying bogs, and cutting away the banks of rivulets to render them fordable by the donkeys, I began to think so. But the crisis came when one day, while skirting the dense line of tangled forest which rose from the bed of a Zambesi tributary, the sudden cracking of branches and splashing of water told us that an elephant had been rudely disturbed, and had made away across the rivulet. We were but a couple of hundred yards from the source of the stream, so I hurried round with the intention of taking the spoor. One donkey —a habitual wanderer—I ordered to be tied to a small tree. Desirous of more scope, he tugged at his reim and shook the tree. In an instant a loud buzzing was heard. The boys bolted for all they were worth, and myriads of enraged bees almost obscured the unhappy

beast. With a supreme effort he broke his reim and galloped away, passing within a few feet of his companions. Simultaneously the others joined in the mad career as clouds of bees separated from the main army and attacked them.

I would not have deemed it possible for donkeys to move so freely and swiftly had I not witnessed this unfortunate occurrence. At first an attempt was made to head them off, but as they separated and broke away in different directions I realised that this incident might deprive me of my carrying power, and place me in a very unenviable position, for the country was uninhabited, and consequently porters were unobtainable. After two or three hours' chase one was recovered, and a fire lighted to drive away the few bees that still plagued the poor brute. Then sending two boys off in one direction, I took another with a third. After tramping a couple of miles an animal in the bush raised my hopes, but instead of a donkey an old bull buffalo lumbered across my front. Having shot him I returned to my temporary camp, with a view to moving it to the meat. To my relief all but one donkey had been brought in, so the situation was much improved. By the following afternoon five unhappy, swollen-headed quadrupeds moped about the camp. The poor beasts were simply one mass of stings, as was the surface of the reim attached to the first unfortunate. Henceforward they lost flesh daily, but although I gave up all idea of their taking my things into the Congolese station in Katanga, I hoped to so far lessen the distance as to place me in communication with the station by the time I came to a full stop. A fortnight later two were killed by lions, but by throwing away what was not absolutely necessary we still moved on, till when within 300 miles of Lukafu, the objective station, I caught up a scientific expedition under the Belgian Lieutenant Lemaire, who was bound for the same place. It transpired that—on his own initiative—the Governor-General of the State had, with courteous consideration, intimated to his officers the possibility of my passing through Katanga, and added instructions to receive me hospitably and forward my interests.

It is with great pleasure that I not only publicly acknowledge this act of consideration, but record the unvarying kindness and good fellowship that were extended to me not only by the officers of many nationalities serving under the auspices of the Congo State, but by the Portuguese and Germans, as well as my own fellow countrymen. During my prolonged wanderings I encountered foreigners from every European nation except Russia and Spain, and in every instance I was most kindly received. The main points of interest in my donkey journey were the discovery of the Zambesi's source one degree north and one degree west of the position assumed on existing maps, and the determination of the watershed thence eastwards, which, in conjunction with M. Lemaire—whose work agrees in all essential points with mine—I have proved to be very inaccurately shown on the standard maps. As, however, I have already described the source

and watershed before the Royal Geographical Society, I will not repeat here.

M. Lemaire insisted on my travelling with him to Lukafu, and relieved my poor donkeys by placing a dozen loadless porters at my disposition. The donkeys I subsequently gave to Captain Verdick, the Lukafu commandant, who secured me twelve excellent porters to carry my goods along the line of the lakes and down the Nile to Lado.

Lukafu station is beautifully situated on ground rising from the Lufira plain. A small mountain stream, from which it takes its name, winds through the station grounds. Though only 3100 feet above the sea level, Captain Verdick assured me that the station is very healthy; this, I imagine to be due partly to the presence of exceptionally good water, but mainly to two conditions of living which are much neglected by the majority of Europeans settled in Africa: excellent brick houses, and—a good cook!

Two miles to the east of Lukafu red cliffs rise abruptly to some 1400 feet and culminate in the Kundelungu plateau, which undulates to a height of 5000 feet above the sea level. This very choice plateau has a length of 200 miles, and stretches as far as the north-west of Lake Mweru. On crossing it I made an equally steep descent into a plain approximately of an equal altitude to that which I had left three days earlier. Travelling in an almost northerly direction I struck the south-western shore of Lake Mweru. That this lake at no very remote period covered a much larger area than it does to-day, with its 2500 square miles, is apparent from the nature of the ground in the south-west and north. Kilwa island, which is shown near its eastern shore on existing maps, is in reality within a couple of miles from the western shore. Leaving the lake, I passed through a grand mountainous district till I reached M'pala, on Tanganika, but I did so at an angle of 15 degrees east of north, instead of 30, as the existing maps have it. This, assuming the position of Mweru to be longitudinally correct, as I believe it to be, places M'pala some 22 miles west of its hitherto allotted position. The tale the telegraph line, recently laid to the south of the lake, has told, suggests that my correction is justified. At M'pala a palm tree stands nearly a quarter of a mile from the present lake shore, and, so far as I could judge, 25 feet above its level. According to native report, Livingstone rested under this tree and moored his boat to its trunk. Thus, since his visit, the lake would seem to have fallen some 10 inches per annum. I subsequently waded knee-deep over the sand barrier which extends across the Lukugu outlet, and I concluded that in existing circumstances the lowering of the lake surface will continue indefinitely, for immediately beyond the bar the Lukugu flows down a steep descent into a gorge many feet below the lake level.

From Tanganika to Kivu is a valley 20 miles wide at the south end, along which the connecting river—the Ruzizi—winds with a

stable average fall of 20 feet to the mile. High mountains confine this valley on either side until within a few miles of Kivu they close in and two narrow passes—through one of which the Ruzizi flows—take the place of the hitherto wide valley.

As the crow flies the two lakes are only 60 miles apart. Lake Kivu is an ideal and in many respects a remarkable lake. It is 60 miles long with a water surface 4900 feet above the sea level. For some two feet above the water the action of the waves washes the banks clear of vegetation.

Fragments of older formations cemented together by molten lava present the appearance of a clumsily made rockery at home. So steep are the banks that the swimmer can dive into deep clear water from the shore on most parts of the lake, and can do so without fear of falling into the open jaws of a hungry crocodile, for neither these reptiles nor hippopotami find a home in Kivu, probably owing to the non-existence of shallows, sand banks and water reeds. There are two large mountainous islands in the lake, and all around except on the northern shore, undulating hills rise to ranges of mountains on the east and west. The lava from the active volcanoes of Umfumbira or Kirunga is washed by the lake in the north, and from them a lava valley extends far towards Lake Albert Edward, to be finally replaced by a white brackish sand. The mountain range to the west of Kivu stretches northwards along the western shore of Albert Edward until it strikes the Semliki valley, which separates it from the Mountains of the Moon—the mighty Ruenzori range. The water of Albert Edward is brackish, and holds large herds of hippopotami; on the shallow eastern shores the pelican, too, is to be seen in large flocks, and in greater numbers than I have noticed elsewhere. From the time I quitted Kivu I had passed through first a hostile and then a famine-stricken country. However, I never found it necessary to fire a shot, though on one or two occasions I quite expected I should have to defend myself against hordes of armed savages who surrounded me. Fortunately they invariably receded as I approached, and as I travel quickly they had little time to mature hostile plans. I was not sorry to find myself and my small caravan at Fort George on the north of Albert Edward, where there is a small garrison of Soudanese troops under a native non-commissioned officer. The one noticeable point about Lake Albert Edward is that the eastern shore runs in a north-easterly and not in a northerly direction as in the published maps.

The Uganda Protectorate has been so prominently before the public the last few years that it is rather in advance of an explorer's sphere, so I will not waste time by describing a country which has been so voluminously treated during the last five years. One fact about Uganda is, however, worthy of record. For nearly two years I had been moving rapidly through Africa, and had nearly completed 8000 miles when I entered Toro, the south-western province of the Protectorate. Here, for the first time, I found myself among natives

who—confident in their security—moved about unarmed. Since leaving Marotseland, women and children had almost invariably run at the approach of the white man. Now, regardless of sex or age, the native encountered would step aside as he passed and give a respectful salute. Faults no doubt may be found with the policy of the late administration, but of the tact and moral integrity of the district commissioners and officers there can be no question when facts speak so eloquently in their favour.

The Nile river, as a water-way and cheap line of communication with Uganda, has undoubtedly a great future, but apart from its utility the upper river has little in its favour. Enervating heat in the dry season, swamps and countless mosquitoes in the wet, and an utter absence of picturesque scenery, excepting only the neighbourhood of the Dufile rapids, are the points which strike the traveller most. I was relieved of the necessity of travelling down the Nile valley from Lado to Khartum by the considerate offer of a passage in the Egyptian stern-wheeler "Kaibar," by Bimbashi Saunders, the Governor of Fashoda, who picked me up at Lado early in August last while on a tour of inspection; and five weeks later I was in England, and had bidden farewell to African exploration for ever.

WEEKLY EVENING MEETING,

Friday, March 22, 1901.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Horace T. Brown, Esq., LL.D. F.R.S. *M.R.I.*

Some Recent Work on Diffusion.

The subject of my lecture, though essentially of a physical nature, had its origin in what may be regarded as a no-man's land—a strip of neutral territory which can be claimed exclusively neither by the physicists nor the biologists.

An attempt to reconcile some apparently contradictory facts connected with the nutrition of plants has led, somewhat unexpectedly, to an extension of the laws of gaseous diffusion, so that we shall have to deal with one of those comparatively rare cases in which biology has been able to react to some extent on physics.

It has long been known that the primary source of the carbon of all plants is the carbonic acid existing in small quantities in ordinary atmospheric air, and that their green parts, more especially the leaves, are able to utilise the energy of sunlight in decomposing the carbonic acid and water, and building up from their elements a whole series of substances, such as sugars and starch, which contribute directly to the nutrition of the plant.

The immediate seat of this synthetic and assimilatory process is found in the minute green chlorophyll granules, which occur in great numbers within the cells of the leaf-tissue; and one of the first problems to be dealt with in the study of the process is to show in what manner the highly dilute carbonic acid of the air can gain entry into the leaf with sufficient rapidity to supply these assimilating centres with material for the needs of the plant.

In a typical leaf, such as is represented in section in the diagram, both sides are covered with a cuticle and epidermis, pierced at regular intervals on one or both sides with extremely minute openings, whose size is capable of being regulated according to the requirements of the plant. These are the *stomates*, which open out into a relatively large cavity within the leaf, and this cavity in turn communicates with the numerous and roomy air-spaces between the cells containing the green chlorophyll granules.

One of the most important functions of the stomates is undoubtedly to regulate the transpiration of water from the plant, but

the question of how far these minute openings play a part in the interchanges of gases between the interior of the leaf and the outer air, has been a subject of very lively controversy.

It is now about thirty years since the eminent French chemist, Boussingault, came to the conclusion that the carbonic acid of the air gains access to the leaf, not through the *stomates*, but through the continuous substance of the cuticle and epidermis, by a process of *osmosis* similar to that by which carbonic acid had been shown by Graham to pass through a thin film of india-rubber.

So convincing did Boussingault's experiments and arguments appear to his contemporaries that this view became an article of faith for something like a quarter of a century, until in fact, some five or six years ago, when Mr. F. Frost Blackman took up the subject and proceeded most inconsiderately to shatter all the most cherished statements of our text-books on this question.

I regret that time will not allow me to do more than state the general conclusions at which Mr. Blackman arrived, and which may be briefly summarised as follows :—

In the first place there is no appreciable passage of atmospheric carbonic acid through the surface of a leaf which is naturally devoid of stomates, such for instance as the upper surface of a normal leaf, which is quite imperforate ; neither is any entry of carbonic acid possible when the stomates have been artificially blocked, or made to close spontaneously.

In addition to this, if a leaf has stomates on both surfaces, the intake of carbonic acid by those surfaces bears a distinct relation to the distribution of the stomates.

We can, in fact, no longer doubt that when a leaf is respiring or assimilating, mere osmosis of carbonic acid through the substance of the cuticle and epidermis plays little or no part in the gaseous exchanges, and that, whatever the exact nature of the process may be, it must be carried on exclusively by the minute openings of the stomates.

Since anything like a mass movement of the air through these openings is out of the question, we must look to the phenomena of *diffusion* for the true explanation, and especially to that form of it which was first described by Graham as *free diffusion*, that is to say, the natural tendency possessed by gases or liquids to form a perfect mixture when they are in contact with each other and there is no partition of any kind between them.

This spontaneous mixing is quite independent of any currents or mass movements of any kind, and is brought about by the gradual interpenetration of the molecules of the one gas or liquid by the molecules of the other.

As an example of this kind of diffusion, I have here a cylinder which, a few weeks ago, was partly filled with a 5 per cent. gelatine solution. After the gelatine had set, the cylinder was filled up with a highly coloured solution of copper salt, which you now see has permeated the jelly to a certain depth. There has been no mixing

of the solutions in the ordinary sense of the word, for the gelatine is virtually a solid. The effect has been produced by the molecules of the coloured copper salt, by reason of their rapid movement in all directions, gradually penetrating into the spaces between the molecules of the gelatine layer. Given a sufficient length of time, and there would be an equal partition of the coloured substance between the two layers.

Diffusion takes place, as is well known, much more rapidly with gases than with liquids. Had our cylinder contained, for instance, carbonic acid in the lower half and air in the upper, a complete mixing would have taken place in a comparatively short time, even if all convection currents had been prevented.

The classical researches of Graham on the diffusion of gases through thin porous septa established the general law that the rate of diffusion of the different gases, under identical conditions, varies inversely as the square roots of their respective densities. Graham's results, however, only acquaint us with the *relative* velocities of diffusion, whereas for the particular problem which we have before us, we must know the *absolute* velocities of diffusion under strictly defined conditions.

It is mainly to the Viennese school of physicists, and especially to Professor Loschmidt, that we owe our present knowledge of the actual rate of penetration of one gas by another in free diffusion.

By observing the speed with which different pairs of gases spontaneously mix in a tube, Loschmidt was able to deduce certain *absolute values* expressing the velocity of their interpenetration.

Some of these results for different pairs of gases are given in the diagram, the last column representing the "constant of diffusivity," expressed in centimetre-gram-second units.

Let us consider the constant for carbonic acid and air, which at 0° C. is ·142. This means that when air and carbonic acid gas are freely diffusing into each other at this temperature, an amount of either gas corresponding to ·142 cubic centimetre will pass in one second of time, across an area of one square centimetre, when the partial pressure of the gas varies by one atmosphere in one centimetre of length.

Now when we come to apply these absolute values of diffusivity to the passage of the extremely dilute CO_2 of the air into the leaf stomates (whose dimensions can of course be determined), we find that free diffusion through these openings is apparently able to account for only a portion of the gas which we know must enter the leaf, unless we make some extremely improbable assumptions as to the very low point at which the partial pressure of the carbonic acid is maintained immediately under the apertures.

I shall not, however, trouble you with the calculations on which this statement is based, since I prefer to put the matter in a more concrete form, which has also the advantage of emphasising the extraordinary power which an assimilating leaf possesses of extracting carbonic acid from its surrounding air.

There are two methods by which we can determine the actual amount of atmospheric carbonic acid used up by an assimilating leaf —one a direct, the other an indirect method.

Part of the apparatus used in the direct method, is shown on the table.

The leaf, which may be still attached to the plant, is enclosed in a glazed case through which a measured current of air is drawn, of which the carbonic acid content is accurately known. When the air emerges from the case it passes through an absorption apparatus which retains the whole of the CO_2 left in the air after passing over the leaf. This absorbed carbonic acid is determined at the close of the experiment, and we then have all the data for estimating the carbonic acid abstracted from the air by the leaf. The area of the leaf being known, the CO_2 absorbed can be referred to a unit area of leaf and a unit time.

By the indirect method, which is due to Sachs, the actual increase in dry weight of a given area of an assimilating leaf is determined, and since this increase in weight is due to substances having a definite percentage of carbon, a simple calculation enables us to determine the equivalent amount of carbonic acid abstracted from the air.

By such methods as this it can be shown that an actively assimilating leaf such as that of the Catalpa tree, in full daylight, and under favourable conditions of temperature, can take in carbonic acid from the air at the rate of about one-tenth cubic centimetre per hour for each square centimetre of leaf.

Since there are only about three volumes of carbonic acid in 10,000 volumes of ordinary air, this must mean that in a single hour the under surface of the leaf will take in as much carbonic acid as is contained in a column of air about eight feet long, and having the same area of cross section as the leaf.

But this remarkable power of an assimilating leaf will be better appreciated if we compare it with a liquid surface of a strong solution of caustic alkali, which is known to have such a great avidity for carbonic acid.

We can investigate the absorptive power of such solutions for the carbonic acid of the air under fixed and controllable conditions by using a form of apparatus which I have on the table, and which can be examined at the close of the lecture. It is so arranged that an air current of known velocity can be drawn over the surface of the absorbing solution which has a known area.

When a very low velocity of the air current has been reached, the amount of absorption becomes constant at ordinary temperatures at about ·17 c.c. of carbonic acid, per square c.m. of surface per hour.

So we see that a leaf, assimilating under natural conditions, is taking in carbonic acid from the air more than half as fast as a surface of the same area would do if it were wetted with a constantly

renewed film of a strong solution of caustic alkali, submitted to a strong current of air.

This is, in itself, a somewhat remarkable conclusion, but what are we to say to a proposition which would limit the absorptive power of the leaf surface to the extremely small apertures of the stomates?

In a leaf such as we have been considering, the aggregate area of the openings of the stomates, when expanded to their widest, amounts to less than *one per cent.* of the total leaf surface, so that if the entry of the CO_2 takes place exclusively by these openings, we must conclude that it goes in more than fifty times faster than it would do if the mouth of each one of these minute openings were filled with a constantly renewed solution of strong caustic alkali.

Such facts make it difficult unreservedly to accept the view that the gaseous exchanges in leaves are really carried on exclusively by the stomates, which occupy such a small fraction of the leaf surface. On the other hand, the direct experimental evidence in favour of this view is overwhelming, so that we apparently find ourselves on the horns of a dilemma.

There appeared to be only one way out of the difficulty—that was to assume that the leaf knows more about the laws of free diffusion than we do, and has adapted itself to some physical principles which have hitherto escaped notice. This was found to be the case when the structure of the leaf was regarded as a piece of physical apparatus for promoting rapid diffusion.

I do not propose to take you through the various and tedious stages by which the true explanation was reached, but will attempt, as far as possible, to short-circuit the current of the argument.

In the first place, I wish to call your attention to a particular mode of free diffusion which, in gases, has been but little studied, but which has a very direct bearing upon diffusion in the living leaf, where one of the constituents of the diffusing gases, the carbonic acid, is very small in amount compared with the others.

Let us for a moment concentrate our attention on the air which is contained in this open glass cylinder, and endeavour to picture to our minds the jostling crowds of the perfectly elastic molecules of the various gases, flying hither and thither in all imaginable directions, and coming into frequent collision with each other and the sides of the containing vessel.

Now in this jostling throng there is a certain proportion of molecules of *carbonic acid*, which we will imagine for the moment are distinguished from the molecules of the other gases by some difference in colour—let us suppose them to be *green*.

Now, further, consider a plane surface in the contained air of the cylinder; from the dynamical theory of gases it follows that in any given interval of time, temperature and pressure remaining constant, the same average number of the "green" molecules will cross this imaginary plane in opposite directions, and since this will be true

for any plane surface, no matter where we take it within the cylinder, there can be no change in the average distribution of the "green" molecules throughout the cylinder—in other words, no change in any part of the cylinder in the composition of the air as regards its carbonic acid content.

But now let us imagine that the bottom of the cylinder is suddenly made capable of absorbing carbonic acid, say by the introduction, without any disturbance of the air, of a little solution of caustic soda or caustic potash. The "green" molecules which now strike the bottom of the cylinder at all imaginable angles of incidence, will not all rebound, as they originally did, but will be to a large extent trapped in their to-and-fro excursions, so that in the first very brief interval of time a very thin stratum of air, parallel to and immediately above the absorbing surface, will be partially freed from its "green" molecules.

Now consider the kind of exchange of "green" molecules which occurs in the next very brief interval of time between this partially depleted layer at the bottom and the one immediately above it. The rate of exchange across the imaginary plane dividing these two contiguous layers can no longer be equal and opposite, since the number of "green" molecules in the upper stratum is greater than that in the lower. A larger number of the "green" molecules must consequently pass in a given brief interval of time from the higher to the lower stratum than from the lower to the higher; in other words, the *balance of exchange* is in favour of the lower layer. This state of affairs will rapidly propagate itself upwards until the mouth of the cylinder is reached; and provided the air outside the cylinder is kept of the same composition, and the absorptive power of the bottom of the cylinder is also kept constant, the *uncompensated balances of exchange* between the imaginary layers may be regarded as constituting a steady flow or drift of the "green" molecules down the tube towards the absorbent surface.

Although within the column there is this constant flow of carbonic acid molecules in the general direction of the axis of the tube, the system as a whole may now be regarded as static so long as all the conditions remain unchanged. The flow is then strictly analogous to the flow of heat in a bar of metal which is kept with its two ends at a uniform difference of temperature; or to the flow of electricity in a conductor between two regions maintained at a constant difference of potential; and static diffusion admits of precisely the same simple mathematical treatment as these phenomena of conduction of heat and of electricity when we come to its quantitative study.

In such an imaginary experiment as we have been considering, it is clear that the amount of carbonic acid in the air of the cylinder must vary uniformly from a maximum at the top of the cylinder to a vanishing point at the bottom, so that if the CO_2 really had the green colour, which for purposes of argument we have attributed to it, the

colour of the air column would uniformly diminish from top to bottom.

This can be illustrated by the diffusion of a coloured copper salt down a gelatine column. If this column were cut off just where the colour ceases to be perceptible, and the cut end were immersed in water to carry off the diffusing salt as fast as it came through the column, then if the upper end of the column remained in contact with the coloured copper solution, we should ultimately get a constant steady flow of the salt down the column.

Under these conditions it can be readily shown both experimentally and theoretically, that the actual amount of substance diffusing down the column in a given time will, in the first place, be directly proportional to the difference in the concentration of the diffusing substance at the two ends of the column; it will also be directly proportional to the *area* of cross-section of the column; but inversely proportional to its length.

The fact which for the moment I wish you to bear in mind is that, all other things being the same, the amount of diffusion down a column of this kind *varies directly as the area of the cross-section of the column.*

This is roughly illustrated by these two cylindrical columns of gelatine, of different diameters, down which a coloured solution has been diffusing for equal times.

The salt has penetrated both columns to the same depth, and the gradation of colour is also the same—a proof that the rates of diffusion down the columns must be proportional to their areas of cross-section.

But now let us consider what will happen if, instead of varying the width of the column throughout its entire length, we only partially obstruct the cylinder somewhere in the line of flow, say by means of a thin diaphragm pierced with a single circular hole of less diameter than the bore of the tube.

We must resort to experiment to answer this question.

Suppose we take a series of exactly similar flasks, such as I have here, and produce a steady flow of atmospheric carbonic acid down their necks by partially filling each flask with a solution of caustic soda, the amount of carbonic acid entering the flasks being determined by subsequent titration of the soda solution. We can then study the effect produced by partially obstructing the mouths of the flasks with thin discs of metal or celluloid pierced with a single hole of definite size.

The results of a series of experiments of this kind are given in Table I., and you will see that under these conditions the amounts of carbonic acid diffusing down the cylindrical neck in a given time, are not proportional to the areas of the apertures, as might reasonably have been expected, but are directly proportional to their *diameters.*

TABLE I.—DIFFUSION OF ATMOSPHERIC CO_2 THROUGH SINGLE APERTURES OF VARYING SIZE.

Diam. of Aperture.	CO_2 Diffused Per Hour.	CO_2 Diffused per sq. cm. per Hour.	Ratio of Areas.	Ratio of Diameters.	Ratio of CO_2 Diffused.
mm.	c.c.	c.c.			
22·7	·2380	·0588	1·00	1·00	1·00
6·03	·0625	·2186	·07	·26	·26
3·23	·0398	·4855	·023	·14	·16
2·11	·0260	·8253	·008	·093	·10

This of course implies, that as we make the aperture smaller, the flow through a given unit of its area is proportionally increased—in other words, the acceleration of flow is *inversely proportional to the diameters of the apertures.*

This unexpected fact, which lies at the root of the whole question we are considering to-night, may be experimentally illustrated in a variety of ways.

We may, for instance, cause the aqueous vapour of the air to diffuse into a similar series of flasks, using in this case strong sulphuric acid as the absorbent, and determining the amount of diffusion of the water vapour, by weighing the flasks from time to time. You will see from the results of such an experiment that the diffusion rates again follow pretty closely the ratios of the diameters of the apertures, and are widely divergent from the ratios of areas. (See Table II.)

TABLE II.—DIFFUSION OF AQUEOUS VAPOUR THROUGH APERTURES OF VARYING SIZE.

Diameter of Aperture.	Ratio of Areas.	Ratio of Diameters.	Ratio of Diffusion for Equal Times.
m.m.			
2·117	1·0	1·0	1·0
3·233	2·3	1·52	1·55
5·840	7·6	2·75	2·54

This "diameter law" is also applicable to circular liquid surfaces, the amount of absorption or evaporation from such surfaces varying, under certain conditions, not in accordance with the areas of those surfaces as might have been expected, but with their diameters.

I have here a short burette-like tube with a wide rim of metal round the top. When this tube is completely filled by letting in a solution of caustic soda, we have a circular surface of the solution lying in the same plane as the rim. When this has been exposed to the air for a given time, the carbonic acid absorbed by the disc of liquid can be determined by drawing off and titrating.

If such absorptive discs of different dimensions are exposed to air which is in *slight movement*, we shall find that the carbonic acid absorbed is proportional to the *area* of the surface. The smaller, however, we make the discs, and the greater precautions we take to keep the air over them perfectly still, the nearer do the absorptions become proportional to the diameters. (See Table III.)

TABLE III.—ABSORPTION OF ATMOSPHERIC CO_2 BY CIRCULAR SURFACES OF SOLUTIONS OF CAUSTIC ALKALI.

Diameter of Surface.	Ratio of Areas.	Ratio of Diameters.	Mean Ratio of Areas and Diameters.	Ratio of CO_2 Absorbed.
mm.				
10·25	1·0	1·0	1·0	1·0
20·25	3·9	1·9	2·9	3·0
29·25	8·1	2·8	5·4	5·3
40·00	15·2	3·9	9·5	9·2
5·00	1·0	1·0	..	1·0
10·25	4·2	2·05	..	2·47

There is always, however, more difficulty in obtaining these results with plane absorbing surfaces, than by diffusion through a perforated diaphragm. The reason for this will be apparent later.

Before entering on an explanation of these facts, I wish you to note a very important conclusion to be drawn from them, and one which readily admits of experimental verification.

We have seen that when we partially obstruct the diffusive flow of a gas or liquid by a thin septum with a single circular perforation, the velocity of the flow through each unit area of aperture increases as the diameter of the aperture decreases. One might therefore expect that if a number of fine holes were suitably arranged in such a septum, the acceleration of flow through the individual holes might assume such proportions that a perforated septum of this kind would exercise little or no obstruction on the diffusive flow, although in such a case the aggregate area of the holes might only represent a small fraction of the total area of the obstructing septum.

Strange and paradoxical as such a conclusion may at first sight appear, it will bear the test of experiment.

I have here a thin film of celluloid—in fact, a piece of the ordinary Kodak roller-film. This has been perforated with holes about ·4 millimetre in diameter, arranged at a little more than 2·5 diameters apart, so that there are just 100 of such perforations on a square centimetre of area. The clear holes represent about one-tenth of the area of the film, nine-tenths of the sieve being blocked up with impervious celluloid.

Here are two columns of gelatine, down which a blue solution of copper-ammonium sulphate have been diffusing for equal times. One of these columns is unobstructed in any way, being in direct contact with the coloured liquid. In the other case a finely perforated celluloid film has been interposed, which has the effect of blocking out nine-tenths of the cross-section of the column. You see that, notwithstanding this, there is no appreciable difference in the amounts of coloured salt which have diffused in the two cases; the salt has, in fact, gone through the finely pierced septum as readily as if no obstruction were present.

(N.B.—The celluloid film is itself not permeable.)

We find that exactly the same holds good with gaseous diffusion.

If finely perforated septa of this kind are luted on to short tubes containing caustic soda and are exposed to still air, the amount of carbonic acid diffusing through the holes in the diaphragm can be compared with the amount which we know would diffuse down the open tube under like conditions.

Some results of this kind are given in Table IV.

Table IV.—Diffusion of Atmospheric CO_2 through Multiperforate Septa into Tube 4 cm. long. Diam. of Holes ·380 mm.

No. of Holes per Square cm.	Diameters Apart.	CO_2 Diffusing through Septum per Hour.	Open Tube, Diffusion per Hour.	Percentage of Septum Diffusion on Open Tube Diffusion.	Percentage, Area of Cross-section occupied by Holes.
		c.c.	c.c.		
100·00	2·63	·361	·346	104·3	11·34
25·00	5·26	·148	·342	43·2	2·82
11·11	7·8	·131	·352	37·2	1·25
6·25	10·52	·110	·353	31·1	·70
15·7	15·7	·068	·334	20·4	·31

I must now ask you to follow me in a somewhat theoretical excursion in quest of an explanation of these curious facts.

We have seen that when steady diffusion is going on down a cylindrical column which is absorbent at the bottom, there is a

uniform diminution in the density of the diffusing substance from one end of the column to the other, evidenced in the case of a coloured substance by a gradual and uniform thinning out of the colour in the direction of the axis of the column. But in any horizontal cross-section of the column, the colour is of the same intensity in all parts of the section, which means of course that the diffusing substance is of equal density along these planes.

In a diagrammatic section of such a column we should therefore represent the surfaces of equal density by straight lines drawn at right angles to the axis of the cylinder, and the stream lines of the diffusing substance by straight lines drawn parallel to the axis.

I am able to show you the horizontal lines of equal density in a cylinder, produced by a process of intermittent diffusion presently to be described.

When diffusion goes on into a flat absorbent disc, or aperture, instead of into a cylinder, it is clear that the stream lines of the diffusing substance must strongly converge towards the disc, instead of moving vertically downwards as they do in the cylinder, and it is also clear that the lines or surfaces of equal density in the diffusing substance, must form curved surfaces of some kind over the disc. We must now consider the exact form which these lines and surfaces will take.

It so happens that there is a problem in electrostatics which is analogous to the one before us, and it is one which has been fully worked out by mathematical physicists.

When an insulated conductor receives an electric charge, the form taken by the surfaces of equi-potential around the conductor depends on its shape, and on the nature and distribution of other charges in its neighbourhood.

If we suppose the absorbing disc or perforation used in our diffusion experiments to be replaced by an electrified disc of similar dimensions, embedded flush in a wide non-conducting rim, then the surfaces of equal electric potential in the air above the disc will take the form represented in Fig. 1. The surfaces will form a series of *hemi-spheroids* which in any vertical section passing through the centre of the disc will give a series of ellipses, having their common foci in the edges of the disc. Faraday's lines or tubes of force on the other hand will, in this case, be represented by a series of hyperbolas, also having their foci in the edges of the disc.

Now we have every reason to believe that in a diffusion experiment with an absorbent disc the surfaces of equal density of the diffusing substance over the disc are the exact analogues of the surfaces of equal potential over the similar electrified disc, and that the stream lines of the diffusing substance are the analogues of the lines or tubes of force. If this be so, the diagram will equally well represent an experiment in which, for instance, the carbonic acid of perfectly still air is being absorbed by a disc of soda solution, surrounded by a wide rim.

Fig. 2 represents what we might expect to be the state of things when diffusion takes place through a circular aperture in a diaphragm. Here the stream lines of the substance, which are convergent as they approach the aperture, diverge again when the opening is past, and we should expect to get a double system of the ellipsoidal zones of equal density on either side of the aperture.

Did time admit I could show you that this hypothesis is not only capable of giving reasonable and consistent explanations of all the phenomena of diffusion into and through apertures, but completely explains the " diameter law " and also enables us to predict the amount of gas, vapour, or solute which will pass under given conditions, and the results can be verified by experiment.

I have only time to glance at one or two readily verifiable deductions from this hypothesis. In the first place it fully accounts for what I have called the " diameter law," that is to say that diffusion through circular apertures in a diaphragm is proportional to their diameters, not to their areas.

In two diagrams on the wall we have represented the arrangement of the equi-density curves and stream lines over two absorbent discs, one double the diameter of the other. We may take these discs to represent an alkaline solution absorbing carbonic acid from the air.

The two systems are on the same relative scale, one in fact being the image of the other magnified by two diameters.

It will be seen that a curved line corresponding to any given actual density of the diffusing substance must be twice as far from the surface of the larger disc as it is from the surface of the smaller; that is to say, the *gradient of density* on which the flow depends, is twice as steep over the small disc as it is over the large one. From this it follows that for equal areas the flow into the smaller disc is twice that into the larger, and that the *total* flow must be proportional to the diameters, which is just what is found to be the case.

Wherever we get conditions favourable for the formation of a system of equi-density zones on one or both sides of a perforated diaphragm, diffusion will go on in accordance with this " diameter law." But one system of zones is quite sufficient for the purpose, so that in a case like that of Fig. 2, which represents the course of diffusion of atmospheric CO_2 in perfectly still air into an absorbent chamber, we might allow the outer system of equi-density shells over the aperture to be completely swept away by air currents and still the " diameter law " would hold good on account of the inner series of zones, which, from their position, are protected from the air currents. This explains in a very satisfactory manner why it is much more easy to demonstrate the diameter law with apertures in a diaphragm, than simply with absorbing discs, where only one external system of equi-density shells can exist, which is of course extremely liable to be influenced by disturbing currents.

Satisfactory, however, as this hypothesis is in explaining every-

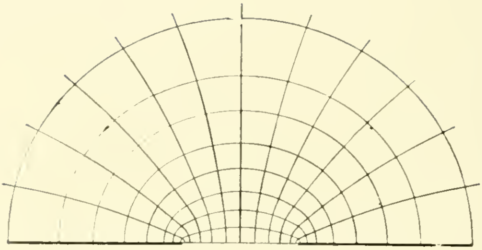

Fig. 1.

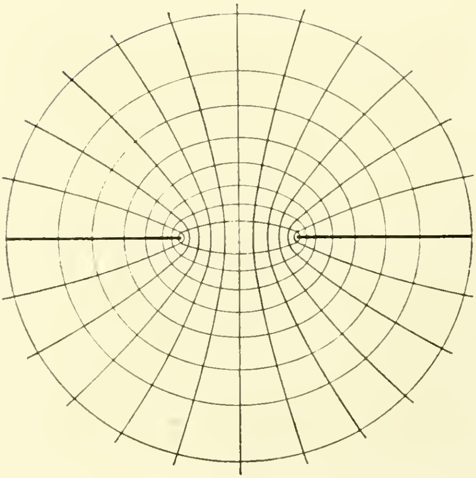

Fig. 2.

Fig. 3.

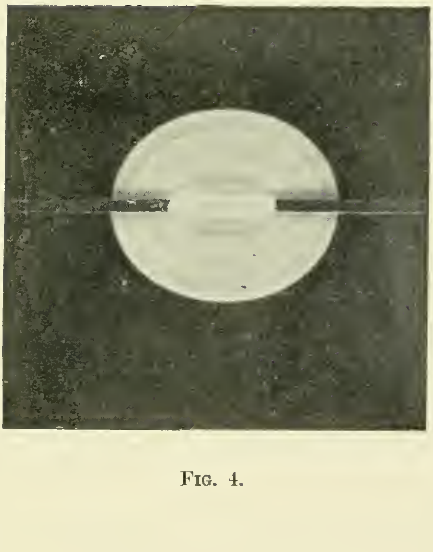

Fig. 4.

thing connected with these curious facts of diffusion, it must be borne in mind that the reasoning on which it is based is in part deductive and in part dependent on an analogy.

Nearly 300 years ago it was said by Sir Thomas Roe that " many things hold well in discourse, and in the theorique, satisfie curious imaginations, but in practice and execution are found difficult and ayric."

Fortunately this does not apply to the present case, and I am able to bring before you this evening for the first time an experimental demonstration of the existence of zones of equal density in the neighbourhood of an aperture through which diffusion is going on, and to show you that they have the exact shape which the theory requires.

I have here a rectangular glass cell divided horizontally by a thin plate of celluloid having a circular hole punched through it. The lower half of the cell is filled with a solution of gelatine containing a little barium chloride, and the upper half with a solution of sodium sulphate.

The relative strengths of the solutions are so adjusted that the two salts, diffusing in opposite directions through the aperture, shall meet somewhere in the gelatine where a precipitate of barium sulphate is thrown down at the surfaces of contact of the two opposing streams of diffusion. The result is that we get a slowly growing spheroidal mass of precipitate, starting from the aperture, and resembling in shape the head of an inverted mushroom.

If we arrange for the diffusion of the sodium sulphate to be intermittent, or better still if we alternate the diffusion of a sulphate with that of a chromate, we get well marked *zonings* in the precipitate forming the spheroid, zonings which correspond to the successive forms which the spheroid has assumed during growth, and which therefore must have been zones of equal density of the diffusing substances. We can study the forms which these assume in relation to the aperture by subsequently cutting sections through the gelatine, but by a little arrangement we can make the apparatus cut its own sections as the diffusion goes on.

This is done by making the aperture in the diaphragm *semicircular* instead of circular, and bringing its straight edge close up to the side of the glass vessel.

I will now throw on the screen some photographs of vertical sections of spheroids of diffusion of this kind. (See Figs. 3 and 4.)

On comparing the lines of equal density around the aperture with the diagrams on the wall, you will at once see that their shape is exactly that required by theory—they describe a series of ellipses having their common foci in the edges of the aperture through which the diffusion is taking place.

The actual stream lines of the diffusing subtance are not visible, but as these must necessarily be normal to the curves of equal density, they can only be represented by a series of hyperbolas, also having their foci in the edges of the aperture.

The electrostatic analogy which has served so well in determining the form of the zones of equal density around single apertures may also be used for predicting their distribution around a series of apertures in a diaphragm.

If we regard the individual holes in a multiperforate diaphragm as so many minute discs, all electrified to a common potential, the lines of equi-potential and the lines of force should take a form something like that represented in the diagram (see Fig. 5), the lines of equi-potential forming complete ellipses in the immediate neighbourhood of the electrified discs, but gradually intersecting and forming a series of wavy lines which become more and more horizontal as the distance gets more remote.

Could they be rendered visible, these are also the forms which we should expect the lines of equal density of a substance to take when it is diffusing through a series of small apertures. I am able to give you a verification of this, by throwing on the screen a photograph showing the result of intermittent diffusion through a series of such apertures. (See Figs. 6 and 7.) The lines of equal density are marked out by the alternate bands of sulphate and chromate of barium, as they were in the last experiment.

From the shape of these lines of equal density it is possible to determine the form of the stream lines of the diffusing substance, and to show that the tendency of a multiperforate septum of this kind, is to locally increase the gradient of density in its neighbourhood and so to accelerate the flow through the small apertures. We get, in fact, a complete and satisfactory explanation of the small amount of obstruction which such a diaphragm produces, when put in the way of a diffusive flow of gas or liquid.

Intermittent diffusion, such as I have described, may be used to illustrate in a variety of ways the distribution of electrical potential around electrified bodies which are within the sphere of each other's action.

It is generally a difficult and laborious task to work out the distribution of the surfaces of equi-potential around electrified bodies which are near enough to influence each other. By this system of intermittent diffusion we may sometimes make nature work out the problem for us. Here, for instance (see Fig. 8), is a figure copied from Clerk Maxwell's 'Electricity and Magnetism,' representing the form which is assumed by equi-potential surfaces around two points, charged with quantities of electricity of the same kind in the ratio of four to one. If the analogy is correct, diffusion through apertures having their diameters in the ratio of two to one, ought to give the same series of figures. You see from the photograph of an actual experiment given in Fig. 9 that this supposition is correct.

In Fig. 10 are given the calculated lines of force at the edges of two parallel plates, one of which is insulated and electrified, the other connected with the earth. These ought to correspond in shape to the equi-density lines of a substance undergoing steady diffusion

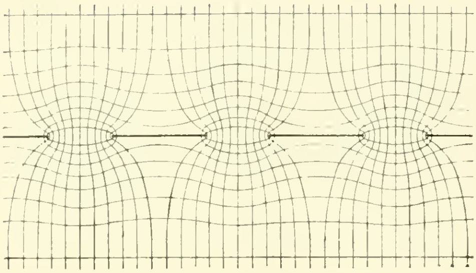

Fig. 5.

Fig. 6.

Fig. 7.

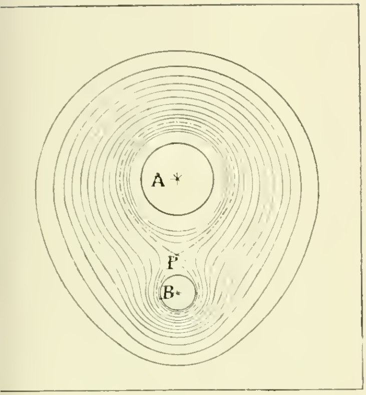

FIG. 8.

FIG. 9.

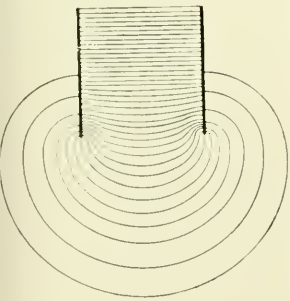

FIG. 10.

FIG. 11.

from between two parallel plates, as in fact you see they do. (See Fig. 11.)

But considerations of this kind, although of interest in showing the striking analogies between certain phenomena of electrostatics and static diffusion, would carry me too far from my main object, and I must again bring you back to the green leaf, which was the starting-point of my lecture.

If we regard the structure of the leaf from the new point of view which now suggests itself, we can readily understand how it is that the stomates, notwithstanding the relatively small area of the leaf surface which they occupy, can drink in the atmospheric carbonic acid with such rapidity.

The finely perforated epidermis of the leaf, tightly stretched over the interior air-spaces whose walls can absorb carbonic acid, constitutes a multiperforate septum which is under the most favourable conditions to produce an acceleration of the diffusive flow of the gas into the leaf.

The laws of gaseous diffusion through small apertures are now so well understood that we can predict with certainty the particular quantitative effect produced on a given diffusive flow by any screen with perforations of known size and distribution providing they are not within a certain number of diameters distant from each other. These deductions can be verified by experimenting with small shallow glass cylinders, made absorbent inside, and closed at the top with very thin discs of celluloid perforated in a known manner. Such a piece of apparatus may be regarded as an artificial leaf, the perforated celluloid representing the epidermis with its stomates, whilst the absorbing solution of caustic soda acts the part of the assimilating centres.

Having obtained confidence in the accuracy of the method of calculation, we can then apply the same principles to determining the efficiency of the leaf stomates, when the whole system is regarded as a piece of mechanism for promoting diffusion.

In the first place, it is found experimentally that the most economical arrangement of very small apertures is to have them set about 8 or 10 diameters apart, for at that distance the interference with each other practically ceases. *This is about the distance at which we generally find the stomates arranged on the underside of most leaves.*

You will remember that the amount of atmospheric carbonic acid which enters an assimilating leaf in an hour, is about ·1 c.c. for every square centimetre of leaf. Now it can be shown that for this amount of gas to enter through the stomates it is only necessary for the CO_2 content of the air just within the leaf to be kept down to 2·8 parts per 10,000, when that of the outer air is three parts per 10,000. This very slight difference in the partial pressure within and without is quite sufficient to account for all the entering CO_2, thanks to the special structure of the leaf.

Thus all the apparent difficulties in the way of accepting the

minute stomates as the sole pathways of gaseous exchange in the leaf entirely disappear when the leaf is studied in this new light, and it becomes evident that the adjustment of the mechanism of the leaf to the physical properties of its surrounding medium is far more perfect than has been hitherto suspected. The leaves of plants have in fact proved themselves better physicists than ourselves, since their structure bears the impress of response to certain properties of gases of which we have hitherto been ignorant.

This is by no means the first occasion on which the plant has given us a lead in physics. The theory of dilute solutions, formulated by Van't Hoff, and indicating that the laws of Boyle and of Avogadro are as applicable to dilute solutions as they are to gases, had its origin in the observations of De Vries and of Pfeffer on the plasmolysis of living cells and the properties of natural semi-permeable membranes.

Nor can we doubt that there are many more such instances which only await detection, and we may reasonably hope that the boundaries of physics and of chemistry will be materially enlarged in unexpected directions if we pay due regard to the whispered hints and slender clues which are on all sides given by the living world of Nature.

[H. T. B.]

WEEKLY EVENING MEETING,

Friday, March 29, 1901.

The Right Hon. Sir James Stirling, M.A. LL.D.,
Vice-President, in the Chair.

The Right Hon. Lord Rayleigh, M.A. D.C.L. LL.D. F.R.S. *M.R.I.*
PROFESSOR OF NATURAL PHILOSOPHY R.I.

Polish.

The lecture commenced with a description of a home-made spectroscope of considerable power. The lens, a plano-convex of 6 inches aperture and 22 feet focus, received the rays from the slit, and finally returned them to a pure spectrum formed in the neighbourhood. The body of the prism was of lead; the faces, inclined at 70°, were of thick plate-glass cemented with glue and treacle. It was charged with bisulphide of carbon, of which the free surface (of small area) was raised above the operative part of the fluid. The prism was traversed twice, and the effective thickness was $5\frac{1}{2}$ inches, so that the resolving power corresponded to 11 inches, or 28 cm., of CS_2. The liquid was stirred by a perforated triangular plate, nearly fitting the prism, which could be actuated by means of a thread within reach of the observer. The reflector was a *flat*, chemically silvered in front.

So far as eye observations were concerned, the performance was satisfactory, falling but little short of theoretical perfection. The stirrer needed to be in almost constant operation, the definition usually beginning to fail within about 20 seconds after stopping the stirrer. But although the stirrer was quite successful in maintaining uniformity of temperature as regards *space*, i.e. throughout the dispersing fluid, the temperature was usually somewhat rapidly variable with *time*, so that photographs, requiring more than a few seconds of exposure, showed inferiority. In this respect a grating is more manageable.

The lens and the faces of the prism were ground and polished (in 1893) upon a machine kindly presented by Dr. Common. The flat surfaces were tested with a spherometer, in which a movement of the central screw through $\frac{1}{100000}$ inch could usually be detected by the touch. The external surfaces of the prism faces were the only ones requiring accurate flatness. In polishing, the operation was not carried as far as would be expected of a professional optician. A

few residual pittings, although they spoil the appearance of a surface, do not interfere with its performance, at least for many purposes.

In the process of grinding together two glass surfaces, the particles of emery, even the finest, appear to act by *pitting* the glasses, i.e. by breaking out small fragments. In order to save time and loss of accuracy in the polishing, it is desirable to carry the grinding process as far as possible, using towards the close only the finest emery. The limit in this direction appears to depend upon the tendency of the glasses (6 inches diameter) to *seize*, when they approach too closely, but with a little care it is easy to attain such a fineness that a candle is seen reflected at an angle of incidence not exceeding 60°, measured as usual from the perpendicular.

The fineness necessary, in order that a surface may reflect and refract regularly without diffusion, viz. in order that it may appear *polished*, depends upon the wave-length of the light and upon the angle of incidence. At a grazing incidence all surfaces behave as if polished, and a surface which reflects red light pretty well may fail signally when tested with blue light at the same angle. If we consider incidences not too far removed from the perpendicular, the theory of gratings teaches that a regularly corrugated surface behaves as if absolutely plane, provided that the *wave-length* of the corrugations is less than the wave-length of the light, and this without regard to the *depth* of the corrugations. Experimental illustrations, drawn from the sister science of Acoustics, were given. The source was a bird-call from which issued vibrations having a wave-length of about 1·5 cm., and the percipient was a high-pressure sensitive flame. When the bird-call was turned away, the flame was silent, but it roared vigorously when the vibrations were reflected back upon it from a plate of glass. A second plate, upon which small pebbles had been glued so as to constitute an ideally rough surface, acted nearly as well, and so did a piece of tin plate suitably corrugated. In all these cases the reflection was *regular*, the flame becoming quiet when the plates were turned out of adjustment through a very small angle. In another method of experimenting the incidence was absolutely perpendicular, the flame being exposed to both the incident and the reflected waves. It is known that under these circumstances the flame remains quiescent at the *nodes* and flares most vigorously at the *loops*. As the reflector is drawn slowly back, the flame passes alternately through the nodes and loops, thus executing a cycle of changes as the reflector moves through *half* a wave-length. The effects observed were just the same whether the reflector were smooth or covered with pebbles, or whether the corrugated tin plate were substituted. All surfaces were smooth *enough* in relation to the wave-length of the vibration to give substantially a specular reflexion.

Finely-ground surfaces are still too coarse for perpendicular specular reflexion of the longest visible waves of light. Here the material may be metal, or glass silvered chemically on the face subsequently to the grinding. But experiment is not limited by the

capabilities of the eye; and it seems certain that a finely ground surface would be smooth enough to reflect without sensible diffusion the longest waves, such as those found by Rubens to be nearly 100 longer than the waves of red light. An experiment may be tried with radiation from a Leslie cube containing hot water, or from a Welsbach mantle (without a chimney). In the lecture the latter was employed, and it fell first at an angle of about 45° upon a finely ground flat glass silvered in front. By this preliminary reflection, the radiation was purified from waves other than those of considerable wave-length. The second reflection (also at 45°) was alternately from polished and finely ground silvered surfaces of the same size, so mounted as to permit the accurate substitution of the one for the other. The heating-power of the radiation thus twice reflected was tested with a thermopile in the usual manner. Repeated comparisons proved that the reflection from the ground surface was about ·76 of that from the polished surface, showing that the ground surface reflected the waves falling upon it with comparatively little diffusion. A slight rotation of any of the surfaces from their proper positions at once cut off the effect. It is probable that the device of submitting radiation to preliminary reflections from one or more merely ground surfaces might be found useful in experiments upon the longest waves.

In view of these phenomena we recognise that it is something of an accident that polishing processes, as distinct from grinding, are needed at all; and we may be tempted to infer that there is no essential difference between the operations. This appears to have been the opinion of Herschel,* whom we may regard as one of the first authorities on such a subject. But, although, perhaps, no sure conclusion can be demonstrated, the balance of evidence appears to point in the opposite direction. It is true that the same powders may be employed in both cases. In one experiment a glass surface was polished with the same emery as had been used effectively a little earlier in the grinding. The difference is in the character of the backing. In grinding, the emery is backed by a hard surface, e.g. of glass, while during the polishing the powder (mostly rouge in these experiments) is imbedded in a comparatively yielding sub-

* Enc. Met., Art. Light, p. 447, 1830: "The intensity and regularity of reflection at the external surface of a medium is found to depend not merely on the nature of the medium, but very essentially on the degree of smoothness and polish of its surface. But it may reasonably be asked, how any regular reflection can take place on a surface polished by art, when we recollect that the process of polishing is, in fact, nothing more than grinding down large asperities into smaller ones by the use of hard gritty powders, which, whatever degree of mechanical comminution we may give them, are yet vast masses, in comparison with the ultimate molecules of matter, and their action can only be considered as an irregular tearing up by the roots of every projection that may occur in the surface. So that, in fact, a surface artificially polished must bear somewhat of the same kind of relation to the surface of a liquid, or a crystal, that a ploughed field does to the most delicately polished mirror, the work of human hands."

stance, such as pitch. Under these conditions, which preclude more than a moderate pressure, it seems probable that no pits are formed by the breaking out of fragments, but that the material is worn away (at first, of course, on the eminences) almost molecularly.

The progress of the operation is easily watched with a microscope, provided, say, with a $\frac{1}{4}$-inch object-glass. The first few minutes suffice to effect a very visible change. Under the microscope it is seen that little facets, parallel to the general plane of the surface, have been formed on all the more prominent eminences.* The facets, although at this stage but a very small fraction of the whole area, are adequate to give a sensible specular reflection, even at perpendicular incidence. On one occasion five minutes' polishing of a rather finely ground glass surface was enough to qualify it for the formation of interference bands, when brought into juxtaposition with another polished surface, the light being either white or from a soda flame; so that in this way an optical test can be applied almost before the polishing has begun.†

As the polishing proceeds, the facets are seen under the microscope to increase both in number and in size, until they occupy much the larger part of the area. Somewhat later the parts as yet untouched by the polisher appear as pits, or spots, upon a surface otherwise invisible. Fig. 1 represents a photograph of a surface at this stage taken with the microscope. The completion of the process consists in rubbing away the whole surface down to the level of the deepest pits. The last part of the operation, while it occupies a great deal of time, and entails further risk of losing the "truth" of the surface, adds very little to the effective area, or to the intensity of the light regularly reflected or refracted.

Perhaps the most important fact taught by the microscope is that the polish of individual parts of the surface does not improve during the process. As soon as they can be observed at all, the facets appear absolutely structureless. In its subsequent action the polishing tool, bearing only upon the parts already polished, extends the boundary of these parts, but does not enhance their quality. Of course, the mere fact that no structure can be perceived does not of itself prove that pittings may not be taking place of a character too fine to be shown by a particular microscope or by any possible microscope. But so much discontinuity, as compared with the grinding action, has to be admitted in any case, that one is inevitably led to the conclusion that in all probability the operation is a molecular one, and that no coherent fragments containing a large number of molecules are broken out. If this be so, there would be much less difference

* The interpretation is facilitated by a thin coating of aniline dye which attaches itself mainly to the hollows.

† With oblique incidence, as in Talbot's experiments (see Phil. Mag., xxviii. p. 191, 1889), achromatic bands may be observed from a surface absolutely unpolished, but this disposition would not be favourable for testing purposes.

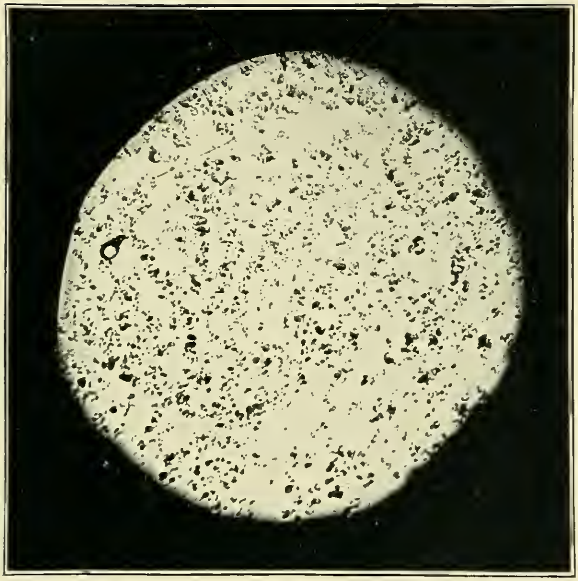

FIG. I.

than Herschel thought between the surfaces of a polished solid and of a liquid.

Several trials have been made to determine how much material is actually removed during the polishing of glass. In one experiment a piece 6 inches in diameter, very finely ground, was carefully weighed at intervals during the process. Losses of ·070, ·032, ·045, ·026, ·032 gms. were successively registered, amounting in all to ·205 gms. Taking the specific gravity of the glass as 3, this corresponds to a thickness of $3 \cdot 6 \times 10^{-4}$ cm., or to about 6 wave-lengths of mean light, and it expresses the distance between the original *mean* surface and the final plane. But the polish of this glass, though sufficient for most practical purposes, was by no means perfect. Probably the 6 wave-lengths would have needed to be raised to 10 in order to satisfy a critical eye. It may be interesting to note for comparison that, in the grinding, one charge of emery, such as had remained suspended in water for seven or eight minutes, removed a thickness of glass corresponding to 2 wave-lengths.

In other experiments the thickness removed in polishing was determined optically. A very finely ground disc was mounted in the lathe and polished locally in rings. Much care was needed to obtain the desired effect of a ring showing a continuously increasing polish from the edges inwards. To this end it was necessary to keep the polisher (a piece of wood covered with resin and rouge) in constant motion, otherwise a number of narrow grooves developed themselves.

The best ring was about half an inch wide. When brought into contact with a polished flat and examined at perpendicular incidence with light from a soda flame, the depression at its deepest part gave a displacement of three bands, corresponding to a depth of $1\frac{1}{2}\lambda$. On a casual inspection this central part appeared well polished, but examination under the microscope revealed a fair number of small pits. Further working increased the maximum depth to $2\frac{1}{2}\lambda$, when but very few pits remained. In this case, then, polish was effected during a lowering of the mean surface through 2 or 3 wave-lengths, but the grinding had been exceptionally fine.

It may be well to emphasise that the observations here recorded relate to a *hard* substance. In the polishing of a soft subtance, such as copper, it is possible that material may be loosened from its original position without becoming detached. In such a case pits may be actually filled in, by which the operation would be much quickened. Nothing suggestive of this effect has been observed in experiments upon glass.

Another method of operating upon glass is by means of hydrofluoric acid. Contrary to what is generally supposed, this action is extremely regular, if proper precautions are taken. The acid should be weak, say one part of commercial acid to two hundred of water, and it should be kept in constant motion by a suitable rocking arrangement. The parts of the glass not intended to be eaten into are, as usual, protected with wax. The effect upon a polished flat

surface is observed by the formation of Newton's rings with soda light. After perhaps three-quarters of an hour, the depression corresponds to half a band, i.e. amounts to $\frac{1}{4}\lambda$, and it appears to be uniform over the whole surface exposed. Two pieces of plate glass, 3 inches square, and flat enough to come into fair contact all over, were painted with wax in parallel stripes, and submitted to the acid for such a time, previously ascertained, as would ensure an action upon the exposed parts of $\frac{1}{4}\lambda$. After removal of the wax, the two plates, crossed and pressed into contact so as to develop the colours, say of the second order, exhibited a chess-board pattern. Where two uncorroded, or where two corroded parts, are in contact, the colours are nearly the same, but where a corroded and an uncorroded surface overlap, a strongly contrasted colour is developed. The combination lends itself to lantern projection, and the pattern upon the screen [shown] is very beautiful, if proper precautions are taken to eliminate the white light reflected from the first and fourth surfaces of the plates.

In illustration of the action of hydrofluoric acid, photographs* were shown of interference bands as formed by soda-light between glass surfaces, one optically flat and the other ordinary plate, upon which a drop of dilute acid had been allowed to stand (Fig. 2). Truly plane surfaces would give bands straight, parallel, and equidistant.

Hydrofluoric acid has been employed with some success to correct ascertained errors in optical surfaces. But while improvements in actual optical performance have been effected, the general appearance of a surface so treated is unprepossessing. The development of latent scratches has been described on a former occasion.†

A second obvious application of hydrofluoric acid has hitherto been less successful. If a suitable stopping could be found by which the deeper pits could be protected from the action, corrosion by acid could be used in substitution for a large part of the usual process of polishing.

In connection with experiments of this sort, trial was made of the action of the acid upon finely ground glass, such for example as is used as a backing for stereoscopic transparencies, and very curious results were observed. For this purpose the acid may conveniently be used much stronger, say one part of commercial acid to 10 parts of water, and the action may be prolonged for hours or days. The general appearance of the glass after treatment is smoother and more translucent, but it is only under the microscope that the remarkable changes which the surface has undergone become intelligible. Fig. 3 is from a photograph taken in the microscope, the focus being upon the originally ground surface itself. The whole area is seen to be divided into cells. These cells increase as the action progresses, the

* The plates were sensitised in the laboratory with cyanine.

† Proc. Roy. Inst., March 1893.

FIG. 2.

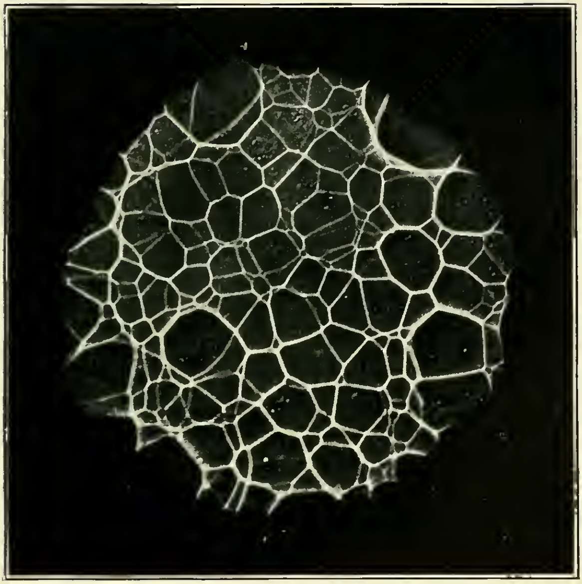

FIG. 3.

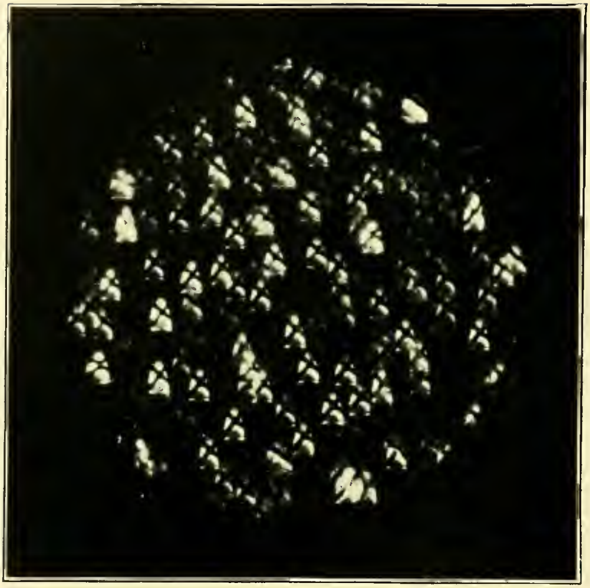

FIG. 4.

smaller ones being, as it were, eaten up by the bigger. The division lines between the cells are *ridges*, raised above the general level, and when seen in good focus appear absolutely sharp. The general surface within the cells shows no structure, being as invisible as if highly polished.

That each cell is in fact a concave lens, forming a separate image of the source of light, is shown by slightly screwing out the object-glass. Fig. 4 was taken in this way from the same surface, the source of light being the flame of a paraffin lamp, in front of which was placed a cross cut from sheet-metal.

The movement required to pass from the ridge to the image of the source, equal to the focal length (f) of the lens, may be utilised to determine the depth (t) of a cell. In one experiment the necessary

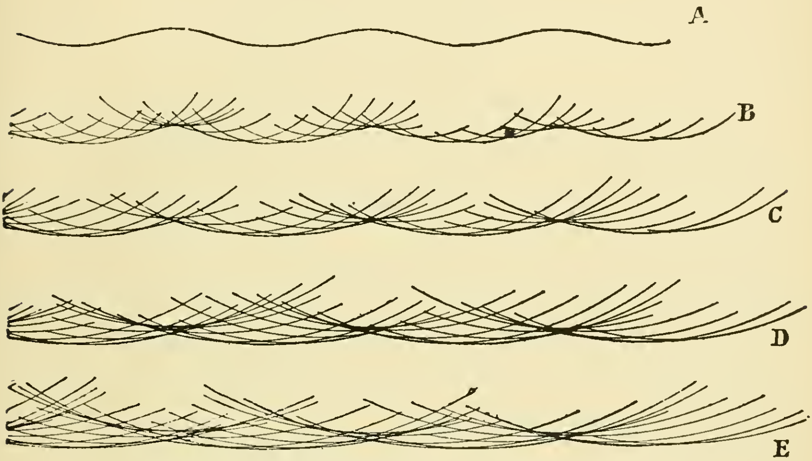

FIG. 5.

movement was ·005 inch. The semi-aperture (y) of the "lens" was ·0015 inch, whence by the formula $y^2 = ft$, we find $t =$ ·00045 inch. This represents the depth of the cell, and it amounts to about 8 wave-lengths of yellow light.

The action of the acid seems to be readily explained if we make the very natural supposition that it eats in everywhere, at a fixed rate, normally to the actual surface. If the amount of the normal corrosion after a proposed time be known, the new surface can be constructed as the "envelope" of spheres having the radius in question and centres distributed over the old surface. Ultimately, the new surface becomes identified with a series of spherical segments having their centres at the deeper pits of the original surface. The construction is easily illustrated in the case of two dimensions. In the figure

A is supposed to be the original surface; B, C, D, E surfaces formed by corrosion, being constructed by circles having their centres on A. In B the ridges are still somewhat rounded, but they become sharp in D and E. The general tendency is to sharpen elevations and to smooth off depressions.

GENERAL MONTHLY MEETING,

Monday, April 1, 1901.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Robert Barnes, M.D.
John Holgate Batten, Esq.
Robert L. Bowles, M.D.
G. Lawson Johnston, Esq.
Joseph Lawrence, Esq.
Edward T. Sturdy, Esq.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned for a donation of £50 from "A Friend" to the Fund for the Promotion of Experimental Research at Low Temperatures.

It was announced from the Chair that His Majesty The King had graciously consented to become Patron of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—General Report on Public Instruction in Bengal for 1899–1900. 8vo. 1900.
Progress Report of the Archæological Survey of Western India for the year ending 30th June, 1900. fol.
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Corfield, Professor W. H.—Small Balance used by Scheele.
A Piece of Antozoniferous Fluor Spar, received from Schreiber in 1863.
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Athenæum for March, 1901. 4to.
Author for March, 1901. 8vo.
Brewers' Journal for March, 1901. 8vo.
Chemical News for March, 1901. 4to.
Chemist aud Druggist for March, 1901. 8vo.
Electrical Engineer for March, 1901. fol.
Electrical Review for March, 1901. 8vo.
Electricity for March, 1901. 8vo.
Electro-Chemist and Metallurgist for March, 1901. 8vo.
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Engineering for March, 1901. fol.
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Horological Journal for March, 1901. 8vo.
Invention for March, 1901.
Journal of the British Dental Association for March, 1901. 8vo.
Journal of Physical Chemistry for Feb. 1901. 8vo.
Journal of State Medicine for March, 1901. 8vo.
Law Journal for March, 1901. 8vo.
Lightning for March, 1901. 8vo.
London Technical Education Gazette for Feb. 1901.
Machinery Market for March, 1901. 8vo.
Nature for March, 1901. 4to
New Church Magazine for March, 1901. 8vo.
Nuovo Cimento for Feb. 1901. 8vo.
Photographic News for March, 1901 8vo.
Physical Review for Feb. 1901. 8vo.
Popular Science Monthly for March, 1901.
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WEEKLY EVENING MEETING,

Friday, April 19, 1901.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. F.R.S., President, in the Chair.

PROFESSOR J. J. THOMSON, M.A. Sc.D. F.R.S.

The Existence of Bodies Smaller than Atoms.

THE masses of the atoms of the various gases were first investigated about thirty years ago by methods due to Loschmidt, Johnstone Stoney and Lord Kelvin. These physicists, using the principles of the kinetic theory of gases, and making certain assumptions (which it must be admitted are not entirely satisfactory) as to the shape of the atom, determined the mass of an atom of a gas; and when once the mass of an atom of one substance is known the masses of the atoms of all other substances are easily deduced by well-known chemical considerations. The results of these investigations might be thought to leave not much room for the existence of anything smaller than ordinary atoms, for they showed that in a cubic centimetre of gas at atmospheric pressure and at 0° C. there are about 20 million, million, million (2×10^{19}) molecules of the gas.

Though some of the arguments used to get this result are open to question, the result itself has been confirmed by considerations of quite a different kind. Thus, Lord Rayleigh has shown that this number of molecules per cubic centimetre gives about the right value for the optical opacity of the air; while a method which I will now describe, by which we can directly measure the number of molecules in a gas, leads to a result almost identical with that of Loschmidt. This method is founded on Faraday's laws of electrolysis; we deduce from these laws that the current through an electrolyte is carried by the atoms of the electrolyte, and that all these atoms carry the same charge, so that the weight of the atoms required to carry a given quantity of electricity is proportional to the quantity carried. We know too, by the results of experiments on electrolysis, that to carry the unit charge of electricity requires a collection of atoms of hydrogen which together weigh about one-tenth of a milligram; hence, if we can measure the charge of electricity on an atom of hydrogen, we see that one-tenth of this charge will be the weight in milligrams of the atom of hydrogen. This result is for the case when electricity passes through a liquid electrolyte. I will now explain how we can measure the mass of the carriers of electricity required to convey a

given charge of electricity through a rarefied gas. In this case the direct methods which are applicable to liquid electrolytes cannot be used; but there are other, if more indirect, methods by which we can solve the problem. The first case of conduction of electricity through gases we shall consider is that of the so-called cathode rays—those streamers from the negative electrode in a vacuum tube which produce the well-known green phosphorescence on the glass of the tube. These rays are now known to consist of negatively electrified particles moving with great rapidity. Let us see how we can determine the electric charge carried by a given mass of these particles. We can do this by measuring the effect of electric and magnetic forces on the particles. If these are charged with electricity they ought to be deflected when they are acted on by an electric force. It was some time, however, before such a deflection was observed, and many attempts to obtain this deflection were unsuccessful. The want of success was due to the fact that the rapidly moving electrified particles which constitute the cathode rays make the gas through which they pass a conductor of electricity; the particles are thus, as it were, moving inside conducting tubes which screen them off from an external electric field; by reducing the pressure of the gas inside the tube to such an extent that there was very little gas left to conduct, I was able to get rid of this screening effect and obtain the deflection of the rays by an electrostatic field. The cathode rays are also deflected by a magnet; the force exerted on them by the magnetic field is at right angles to the magnetic force, at right angles also to the velocity of the particle, and equal to $H e v \sin \theta$, where H is the magnetic force, e the charge on the particle and θ the angle between H and v. Sir George Stokes showed long ago that, if the magnetic force was at right angles to the velocity of the particle, the latter would describe a circle whose radius is $\frac{m v}{e H}$ (if m is the mass of the particle); we can measure the radius of this circle, and thus find $\frac{m}{v e}$. To find v, let an electric force F and a magnetic force H act simultaneously on the particle, the electric and magnetic forces being both at right angles to the path of the particle and also at right angles to each other. Let us adjust these forces so that the effect of the electric force which is equal to Fe just balances that of the magnetic force which is equal to Hev. When this is the case $Fe = Hev$, or $v = \frac{F}{H}$. We can thus find t, and, knowing from the previous experiment the value of $\frac{vm}{e}$, we deduce the value of $\frac{m}{e}$. The value of $\frac{m}{e}$ found in this way was about 10^{-7}, and other methods used by Wiechert, Kaufmann and Lenard have given results not greatly different. Since $\frac{m}{e} = 10^{-7}$, we see that to carry unit charge of electricity by the

particles forming the cathode rays only requires a mass of these particles amounting to one ten-thousandth of a milligram, while to carry the same charge by hydrogen atoms would require a mass of one-tenth of a milligram.*

Thus, to carry a given charge of electricity by hydrogen atoms requires a mass a thousand times greater than to carry it by the negatively electrified particles which constitute the cathode rays; and it is very significant that, while the mass of atoms required to carry a given charge through a liquid electrolyte depends upon the kind of atom—being, for example, eight times greater for oxygen than for hydrogen atoms—the mass of cathode ray particles required to carry a given charge is quite independent of the gas through which the rays travel and of the nature of the electrode from which they start.

The exceedingly small mass of these particles for a given charge compared with that of the hydrogen atoms might be due either to the mass of each of these particles being very small compared with that of a hydrogen atom or *else to the charge carried by each particle being large compared with that carried by the atom of hydrogen.* It is therefore essential that we should determine the electric charge carried by one of these particles. The problem is as follows: Suppose in an enclosed space we have a number of electrified particles each carrying the same charge, it is required to find the charge on each particle. It is easy by electrical methods to determine the total quantity of electricity on the collection of particles, and, knowing this, we can find the charge on each particle if we can count the number of particles. To count these particles the first step is to make them visible. We can do this by availing ourselves of a discovery made by C. T. R. Wilson working in the Cavendish Laboratory. Wilson has shown that, when positively and negatively electrified particles are present in moist dust-free air, a cloud is produced when the air is closed by a sudden expansion, though this amount of expansion would be quite insufficient to produce condensation when no electrified particles are present: the water condenses round the electrified particles, and, if these are not too numerous, each particle becomes the nucleus of a little drop of water. Now Sir George Stokes has shown how we can calculate the rate at which a drop of water falls through air if we know the size of the drop, and conversely we can determine the size of the drop by measuring the rate at which it falls through the air; hence, by measuring the speed with which the cloud falls, we can determine the volume of each little drop; the whole volume of water

* Professor Schuster in 1889 was the first to apply the method of the magnetic deflection of the discharge to get a determination of the value of $\frac{m}{e}$; he found rather widely separated limiting values for this quantity, and came to the conclusion that it was of the same order as in electrolytic solutions; the result of the method mentioned above, as well as those of Wiechert, Kaufmann and Lenard, make it very much smaller.

deposited by cooling the air can easily be calculated, and, dividing the whole volume of water by the volume of one of the drops, we get the number of drops, and hence the number of the electrified particles. We saw, however, that if we knew the number of particles we could get the electric charge on each particle; proceeding in this way I found that the charge carried by each particle was about $6 \cdot 5 \times 10^{-10}$ electrostatic units of electricity, or $2 \cdot 17 \times 10^{-20}$ electro-magnetic units. According to the kinetic theory of gases, there are 2×10^{19} molecules in a cubic centimetre of gas at atmospheric pressure and at the temperature 0° C.; as a cubic centimetre of hydrogen weighs about one-eleventh of a milligram, each molecule of hydrogen weighs about $\frac{1}{(22 \times 10^{19})}$ milligrams, and each atom therefore about $\frac{1}{(44 \times 10^{19})}$ milligrams, and as we have seen that in the electrolysis of solutions one-tenth of a milligram carries unit charge, the atom of hydrogen will carry a charge equal to $\frac{10}{(44 \times 10^{19})} = 2 \cdot 27 \times 10^{-20}$ electro-magnetic units. The charge on the particles in a gas, we have seen, is equal to $2 \cdot 17 \times 10^{-20}$ units. These numbers are so nearly equal that, considering the difficulties of the experiments, we may feel sure that the charge on one of these gaseous particles is the same as that on an atom of hydrogen in electrolysis. This result has been verified in a different way by Professor Townsend, who used a method by which he found, not the absolute value of the electric charge on a particle, but the ratio of this charge to the charge on an atom of hydrogen; and he found that the two charges were equal.

As the charges on the particle and the hydrogen atom are the same, the fact that the mass of these particles required to carry a given charge of electricity is only one-thousandth part of the mass of the hydrogen atoms shows that the mass of each of these particles is only about $\frac{1}{1000}$ of that of a hydrogen atom. These particles occurred in the cathode rays inside a discharge tube, so that we have obtained from the matter inside such a tube particles having a much smaller mass than that of the atom of hydrogen, the smallest mass hitherto recognised. These negatively electrified particles, which I have called corpuscles, have the same electric charge and the same mass whatever be the nature of the gas inside the tube or whatever the nature of the electrodes; the charge and mass are invariable. They therefore form an invariable constituent of the atoms or molecules of all gases, and presumably of all liquids and solids.

Nor are the corpuscles confined to the somewhat inaccessible regions in which cathodic rays are found. I have found that they are given off by incandescent metals, by metals when illuminated by ultra-violet light, while the researches of Becquerel and Professor and Madame Curie have shown that they are given off by that wonderful substance the radio-active radium.

In fact, in every case in which the transport of negative electricity through gas at a low pressure (i.e., when the corpuscles have nothing to stick to) has been examined, it has been found that the carriers of the negative electricity are these corpuscles of invariable mass.

A very different state of things holds for the positive electricity. The masses of the carriers of positive electricity have been determined for the positive electrification in vacuum tubes by Wien and by Ewers, while I have measured the same thing for the positive electrification produced in a gas by an incandescent wire. The results of these experiments show a remarkable difference between the property of positive and negative electrification, for the positive electricity, instead of being associated with a constant mass $\frac{1}{1000}$ of that of the hydrogen atom, is found to be always connected with a mass which is of the same order as that of an ordinary molecule, and which, moreover, varies with the nature of the gas in which the electrification is found.

These two results, the invariability and smallness of the mass of the carriers of negative electricity, and the variability and comparatively large mass of the carriers of positive electricity, seem to me to point unmistakably to a very definite conception as to the nature of electricity. Do they not obviously suggest that negative electricity consists of these corpuscles, or, to put it the other way, that these corpuscles are negative electricity, and that positive electrification consists in the absence of these corpuscles from ordinary atoms? Thus this point of view approximates very closely to the old one-fluid theory of Franklin; on that theory electricity was regarded as a fluid, and changes in the state of electrification were regarded as due to the transport of this fluid from one place to another. If we regard Franklin's electric fluid as a collection of negatively electrified corpuscles, the old one-fluid theory will, in many respects, express the results of the new. We have seen that we know a good deal about the "electric fluid"; we know that it is molecular, or rather corpuscular in character; we know the mass of each of these corpuscles and the charge of electricity carried by it; we have seen, too, that the velocity with which the corpuscles move can be determined without difficulty. In fact, the electric fluid is much more amenable to experiment than an ordinary gas, and the details of its structure are more easily determined.

Negative electricity (i.e., the electric fluid) has mass; a body negatively electrified has a greater mass than the same body in the neutral state; positive electrification, on the other hand, since it involves the absence of corpuscles, is accompanied by a diminution in mass.

An interesting question arises as to the nature of the mass of these corpuscles, which we may illustrate in the following way. When a charged corpuscle is moving, it produces in the region around it a magnetic field whose strength is proportional to the velocity of the corpuscle; now in a magnetic field there is an amount of energy pro-

portional to the square of the strength, and thus, in this case, proportional to the square of the velocity of the corpuscle.

Then, if e is the electric charge on the corpuscle and v its velocity there will be in the region round the corpuscle an amount of energy equal to $\frac{1}{2}\beta e^2v^2$ where β is a constant which depends upon the shape and size of the corpuscle. Again, if m is the mass of the corpuscle its kinetic energy is $\frac{1}{2}mv^2$, and thus the total energy due to the moving electrified corpuscle is $\frac{1}{2}(m + \beta e^2)v^2$, so that, for the same velocity, it has the same kinetic energy as a non-electrified body whose mass is greater than that of the electrified body by βe^2. Thus a charged body possesses, in virtue of its charge, as I showed twenty years ago, an apparent mass apart from that arising from the ordinary matter in the body. In the case of these corpuscles, part of their mass is undoubtedly due to the electrification, and the question arises whether or not the whole of their mass can be accounted for in this way. I have recently made some experiments which were intended to test this point; the principle underlying these experiments being as follows: if the mass of the corpuscle is the ordinary "mechanical" mass, then, if a rapidly moving corpuscle be brought to rest by colliding with a solid obstacle, its kinetic energy being resident in the corpuscle will be spent in heating up the molecules of the obstacle in the neighbourhood of the place of collision, and we should expect the mechanical equivalent of the heat produced in the obstacle to be equal to the kinetic energy of the corpuscle. If, on the other hand, the mass of the corpuscle is "electrical," then the kinetic energy is not in the corpuscle itself, but in the medium around it, and, when the corpuscle is stopped the energy travels outwards into space as a pulse confined to a thin shell travelling with the velocity of light. I suggested some time ago that this pulse forms the Röntgen rays which are produced when the corpuscles strike against an obstacle. On this view, the first effect of the collision is to produce Röntgen rays, and thus, unless the obstacle against which the corpuscle strikes absorbs all these rays, the energy of the heat developed in the obstacle will be less than the energy of the corpuscle. Thus, on the view that the mass of the corpuscle is wholly or mainly electrical in its origin, we should expect the heating effect to be smaller when the corpuscles strike against a target permeable by the Röntgen rays given out by the tube in which the corpuscles are produced, than when they strike against a target opaque to those rays. I have tested the heating effects produced in permeable and opaque targets, but have never been able to get evidence of any considerable difference between the two cases. The differences actually observed were small compared with the total effect, and were sometimes in one direction and sometimes in the opposite. The experiments, therefore, tell against the view that the whole of the mass of a corpuscle is due to its electrical charge. The idea that mass in general is electrical in its origin is a fascinating one, although it has not at present been reconciled with the results of experience.

The smallness of these particles marks them out as likely to

afford a very valuable means for investigating the details of molecular structure—a structure so fine that even waves of light are on far too large a scale to be suitable for its investigation, as a single wave-length extends over a large number of molecules. This anticipation has been fully realised by Lenard's experiments on the obstruction offered to the passage of these corpuscles through different substances. Lenard found that this obstruction depended only upon the density of the substance, and not upon its chemical composition or physical state. He found that, if he took plates of different substances of equal areas and of such thicknesses that the masses of all the plates were the same, then, no matter of what the plates were made, whether of insulators or conductors, whether of gases, liquids or solids, the resistance they offered to the passage of the corpuscles through them was the same. Now this is exactly what would happen if the atoms of the chemical elements were aggregations of a large number of equal particles of equal mass: the mass of an atom being proportional to the number of these particles contained in it, and the atom being a collection of such particles through the interstices between which the corpuscle might find its way. Thus, a collision between a corpuscle and an atom would not be so much a collision between the corpuscle and the atom as a whole, as between a corpuscle and the individual particles of which the atom consists; and the number of collisions the corpuscle would make, and therefore the resistance it would experience, would be the same if the number of particles in unit volume were the same, whatever the nature of the atoms might be into which these particles are aggregated. The number of particles in unit volume is, however, fixed by the density of the substance, and on this view the density (and the density alone) should fix the resistance offered by the substance to the motion of a corpuscle through it; this, however, is precisely Lenard's result, which is a strong confirmation of the view that the atoms of the elementary substances are made up of simpler parts, all of which are alike. This and similar views of the constitution of matter have often been advocated; thus in one form of it, known as Prout's hypothesis, all the elements were supposed to be compounds of hydrogen. We know, however, that the mass of the primordial atom must be much less than that of hydrogen. Sir Norman Lockyer has advocated the composite view of the nature of the elements on spectroscopic grounds, but the view has never been more boldly stated than it was long ago by Newton, who says:

"The smallest particles of matter may cohere by the strongest attraction and compose bigger particles of weaker virtue, and many of these may cohere and compose bigger particles whose virtue is still weaker, and so on for divers successions, until the progression ends in the biggest particles on which the operations in chemistry and the colours of natural bodies depend, and which by adhering compose bodies of a sensible magnitude."

The reasoning we used to prove that the resistance to the motion of the corpuscle depends only upon the density is only valid when

the sphere of action of one of the particles on a corpuscle does not extend as far as the nearest particle. We shall show later on, that the sphere of action of a particle on a corpuscle depends upon the velocity of the corpuscle—the smaller the velocity the greater being the sphere of action—and that, if the velocity of the corpuscle falls as low as 10^7 centimetres per second, then, from what we know of the charge on the corpuscle and the size of molecules, the sphere of action of the particle might be expected to extend further than the distance between two particles; and thus, for corpuscles moving with this and smaller velocities, we should not expect the density law to hold.

Existence of Free Corpuscles or Negative Electricity in Metals.

In the cases hitherto described the negatively electrified corpuscles had been obtained by processes which require the bodies from which the corpuscles are liberated to be subjected to somewhat exceptional treatment. Thus, in the case of the cathode rays the corpuscles were obtained by means of intense electric fields: in the case of the incandescent wire by great heat, in the case of the cold metal surface by exposing this surface to light. The question arises whether there is not to some extent, even in matter in the ordinary state and free from the action of such agencies, a spontaneous liberation of those corpuscles—a kind of dissociation of the neutral molecules of the substance into positively and negatively electrified parts, of which the latter are the negatively electrified corpuscles.

Let us consider the consequences of some such effect occurring in a metal, the atoms of the metal splitting up into negatively electrified corpuscles and positively electrified atoms, and these again after a time re-combining to form a neutral system. When things have got into a steady state, the number of corpuscles re-combining in a given time will be equal to the number liberated in the same time. There will thus be diffused through the metal swarms of these corpuscles: these will be moving about in all directions like the molecules of a gas, and, as they can gain or lose energy by colliding with the molecule of the metal, we should expect by the kinetic theory of gases that they will acquire such an average velocity that the mean kinetic energy of a corpuscle moving about in the metal is equal to that possessed by a molecule of a gas at the temperature of the metal; this would make the average value of the corpuscles at 0° C. about 10^7 centimetres per second. This swarm of negatively electrified corpuscles when exposed to an electric force will be sent drifting along in the direction opposite to the force; this drifting of the corpuscles will be an electric current, so that we could in this way explain the electrical conductivity of metals.

The amount of electricity carried across unit area under a given electric force will depend upon and increase with (1) the number of free corpuscles per unit volume of the metal; (2) the freedom with which these can move under the force between the atoms of the

metal; the latter will depend upon the average velocity of these corpuscles, for if they are moving with very great rapidity the electric force will have very little time to act before the corpuscle collides with an atom, and the effect produced by the electric force annulled. Thus, the average velocity of drift imparted to the corpuscles by the electric field will diminish as the average velocity of translation, which is fixed by the temperature, increases. As the average velocity of translation increases with the temperature, the corpuscles will move more freely under the action of an electric force at low temperatures than at high, and thus from this cause the electrical conductivity of metals would increase as the temperature diminishes. In a paper presented to the International Congress of Physics at Paris in the autumn of last year, I described a method by which the number of corpuscles per unit volume and the velocity with which they moved under an electric force can be determined. Applying this method to the case of bismuth, it appears that at the temperature of 20° C. there are about as many corpuscles in a cubic centimetre as there are molecules in the same volume of a gas at the same temperature and at a pressure of about a quarter of an atmosphere, and that the corpuscles under an electric field of 1 volt per centimetre would travel at the rate of about 70 metres per second. Bismuth is at present the only metal for which the data necessary for the application of this method exist; but experiments are in progress at the Cavendish Laboratory which it is hoped will furnish the means for applying the method to other metals. We know enough, however, to be sure that the corpuscles in good conductors, such as gold, silver or copper, must be much more numerous than in bismuth, and that the corpuscular pressure in these metals must amount to many atmospheres. These corpuscles increase the specific heat of a metal, and the specific heat gives a superior limit to the number of them in the metal.

An interesting application of this theory is to the conduction of electricity through thin films of metal. Longden has recently shown that when the thickness of the film falls below a certain value, the specific resistance of the film increases rapidly as the thickness of the film diminishes. This result is readily explained by this theory of metallic conduction, for when the film gets so thin that its thickness is comparable with the mean free path of a corpuscle, the number of collisions made by a corpuscle in a film will be greater than in the metal in bulk, thus the mobility of the particles in the film will be less and the electrical resistance consequently greater.

The corpuscles disseminated through the metal will do more than carry the electric current, they will also carry heat from one part to another of an unequally heated piece of metal. For if the corpuscles in one part of the metal have more kinetic energy than those in another, then, in consequence of the collisions of the corpuscles with each other and with the atoms, the kinetic energy will tend to pass from those places where it is greater to those where it is less, and in this way heat will flow from the hot to the cold parts of the metal; as

the rate with which the heat is carried will increase with the number of corpuscles and with their mobility, it will be influenced by the same circumstances as the conduction of electricity, so that good conductors of electricity should also be good conductors of heat. If we calculate the ratio of the thermal to the electric conductivity on the assumption that the whole of the heat is carried by the corpuscles, we obtain a value which is of the same order as that found by experiment.

Weber many years ago suggested that the electrical conductivity of metals was due to the motion through them of positively and negatively electrified particles, and this view has recently been greatly extended and developed by Riecke and by Drude. The objection to any electrolytic view of the conduction through metals is that, as in electrolysis, the transport of electricity involves the transport of matter, and no evidence of this has been detected; this objection does not apply to the theory sketched above, as on this view it is the corpuscles which carry the current; these are not atoms of the metal, but very much smaller bodies which are the same for all metals.

It may be asked, if the corpuscles are disseminated through the metal and moving about in it with an average velocity of about 10^7 centimetres per second, how is it that some of them do not escape from the metal into the surrounding air? We must remember, however, that these negatively electrified corpuscles are attracted by the positively electrified atoms and in all probability by the neutral atoms as well, so that to escape from these attractions and get free a corpuscle would have to possess a definite amount of energy: if a corpuscle had less energy than this then, even though projected away from the metal, it would fall back into it after travelling a short distance. When the metal is at a high temperature, as in the case of the incandescent wire, or when it is illuminated by ultra-violet light, some of the corpuscles acquire sufficient energy to escape from the metal and produce electrification in the surrounding gas. We might expect too that, if we could charge a metal so highly with negative electricity, that the work done by the electric field on the corpuscle in a distance not greater than the sphere of action of the atoms on the corpuscles was greater than the energy required for a corpuscle to escape, then the corpuscles would escape and negative electricity stream from the metal. In this case the discharge could be effected without the participation of the gas surrounding the metal and might even take place in an absolute vacuum, if we could produce such a thing. We have as yet no evidence of this kind of discharge, unless indeed some of the interesting results recently obtained by Earhart with very short sparks should be indications of an effect of this kind.

A very interesting case of the spontaneous emission of corpuscles is that of the radio-active substance radium discovered by M. and Madame Curie. Radium gives out negatively electrified corpuscles which are deflected by a magnet. Becquerel has determined the ratio of the mass to the charge of the radium corpuscles, and finds it is the same as for the corpuscles in the cathode rays. The velocity of the

radium corpuscles is, however, greater than any that has hitherto been observed for either cathode or Lenard rays: being, as Becquerel found, as much as 2×10^{10} centimetres per second, or two-thirds the velocity of light. This enormous velocity explains why the corpuscles from radium are so very much more penetrating than the corpuscles from cathode or Lenard rays; the difference in this respect is very striking, for while the latter can only penetrate solids when they are beaten out into the thinnest films, the corpuscles from radium have been found by Curie to be able to penetrate a piece of glass 3 millimetres thick. To see how an increase in the velocity can increase the penetrating power, let us take as an illustration of a collision between the corpuscle and the particles of the metal the case of a charged corpuscle moving past an electrified body; a collision may be said to occur between these when the corpuscle comes so close to the charged body that its direction of motion after passing the body differs appreciably from that with which it started. A simple calculation shows that the deflection of the corpuscle will only be considerable when the kinetic energy with which the corpuscle starts on its journey towards the charged body is not large compared with the work done by the electric forces on the corpuscle in its journey to the shortest distance from the charged body. If d is the shortest distance, e and e' the charge of the body and corpuscles, the work done is $\frac{e e'}{d}$; while if m is the mass and v the velocity with which the corpuscle starts, the kinetic energy to begin with is $\frac{1}{2} m v^2$; thus a considerable deflection of the corpuscle, i.e. a collision will occur only when $\frac{e e'}{d}$ is comparable with $\frac{1}{2} m v^2$; and d, the distance at which a collision occurs, will vary inversely as v^2. As d is the radius of the sphere of action for collision, and as the number of collisions is proportional to the area of a section of this sphere, the number of collisions is proportional to d^2, and therefore varies inversely as v^4. This illustration explains how rapidly the number of collisions and therefore the resistance offered to the motion of the corpuscles through matter diminishes as the velocity of the corpuscles increases, so that we can understand why the rapidly moving corpuscles from radium are able to penetrate substances which are nearly impermeable to the more slowly moving corpuscles from cathode and Lenard rays.

Cosmical Effects produced by Corpuscles.

As a very hot metal emits these corpuscles it does not seem an improbable hypothesis that they are emitted by that very hot body, the sun. Some of the consequences of this hypothesis have been developed by Paulsen, Birkeland and Arrhenius, who have developed a theory of the Aurora Borealis from this point of view. Let us suppose that the sun gives out corpuscles which travel out through interplanetary space; some of these will strike the upper regions of the

earth's atmosphere and will then or even before then, come under the influence of the earth's magnetic field. The corpuscles when in such a field, will describe spirals round the lines of magnetic force; as the radii of these spirals will be small compared with the height of the atmosphere, we may for our present purpose suppose that they travel along the lines of the earth's magnetic force. Thus the corpuscles which strike the earth's atmosphere near the equatorial regions where the lines of magnetic force are horizontal will travel horizontally, and will remain at the top of the atmosphere, where the density is so small that but little luminosity is caused by the passage of the corpuscles through the gas; as the corpuscles travel into higher latitudes where the lines of magnetic force dip, they follow these lines and descend into lower and denser parts of the atmosphere, where they produce luminosity, which on this view is the Aurora.

As Arrhenius has pointed out, the intensity of the Aurora ought to be a maximum at some latitude intermediate between the pole and the equator, for, though in the equatorial regions the rain of corpuscles from the sun is greatest, the earth's magnetic force keeps these in such highly rarefied gas that they produce but little luminosity, while at the pole, where the magnetic force would pull them straight down into the denser air, there are not nearly so many corpuscles; the maximum luminosity will therefore be somewhere between these places. Arrhenius has worked out this theory of the Aurora very completely, and has shown that it affords a very satisfactory explanation of the periodic variations to which it is subject.

As a gas becomes a conductor of electricity when corpuscles pass through it, the upper regions of the air will conduct, and when air currents occur in these regions, conducting matter will be driven across the lines of force due to the earth's magnetic field, electric currents will be induced in the air, and the magnetic force due to these currents will produce variations in the earth's magnetic field. Balfour Stewart suggested long ago that the variation in the earth's magnetic field was caused by currents in the upper regions of the atmosphere; and Schuster has shown, by the application of Gauss' method, that the seat of these variations is above the surface of the earth.

The negative charge in the earth's atmosphere will not increase indefinitely in consequence of the stream of negatively electrified corpuscles coming into it from the sun, for as soon as it gets negatively electrified it begins to repel negatively electrified corpuscles from the ionised gas in the upper regions of the air, and a state of equilibrium will be reached when the earth has such a negative charge that the corpuscles driven by it from the upper regions of the atmosphere are equal in number to those reaching the earth from the sun. Thus, on this view, interplanetary space is thronged with corpuscular traffic, rapidly moving corpuscles coming out from the sun while more slowly moving ones stream into it.

In the case of a planet which, like the moon, has no atmosphere,

there will be no gas for the corpuseles to ionise, and the negative electrification will increase until it is so intense that the repulsion exerted by it on the corpuscles is great enough to prevent them from reaching the surface of the planet.

Arrhenius has suggested that the luminosity of nebulæ may not be due to high temperature, but that the luminosity is produced by the passage through their outer regions of the corpuscles wandering about in space, the gas in the nebulæ being quite cold. This view seems in some respects to have advantages over that which supposes the nebulæ to be at very high temperatures. These and other illustrations, which might be given did time permit, seem to render it probable that these corpuscles may play an important part in cosmical as well as in terrestrial physics.

[J. J. T.]

WEEKLY EVENING MEETING,

Friday, April 26, 1901.

His Grace the Duke of Northumberland, K.G. D.C.L. F.R.S., President, in the Chair.

Hans Gadow, Esq., M.A. Ph.D. F.R.S.

Colour in Amphibia.

The colours of the skin of the Amphibia are of two kinds, pigment or chemical, and structural. The pigments are either diffuse or granular; they are usually black, red, or yellow, and they remain the same whether examined under direct or under transmitted light, although they may change in extent and intensity. The pigment granules are stored up in cells, chromatophores, which send out branched processes and are restricted to the cutis, or leathery part of the skin, mostly to its upper layers. The superimposed epiderm is thin and, as a rule, colourless. Contraction of the chromatophores withdraws the pigment from the surface and causes the skin to appear paler; expansion distributes the pigment more or less equally near the surface, which consequently assumes a darker, more saturated, tint.

Secondly, there are colours which are due to structure. This applies always to green, blue and violet. These colours are produced by interference of the light, with an underlying stratum of black or yellow. Such colours are highly changeable.

A peculiar kind of colouring matter is the white pigment, which may, perhaps, be classified in a third category. It is unchangeable, and consists apparently of tiny crystals of guanine, a product allied to uric acid, and is, as a rule, deposited within cells.

One of the most interesting colours is green. It is not only very common in many frogs, especially such as lead an arboreal life, but it strikes us by its vivid, saturated tints, and last, not least, by the changes which creatures thus endowed can exhibit in their coloration. If you examine a piece of the skin of a green Tree-frog under transmitted light, by holding it against the light, it does not look green at all, but more or less black-brown. The same piece when examined under low power and direct light, shows a mosaic of green polygonal areas, separated by black lines and interrupted by the openings of the skin glands. Seen from the under surface the skin is black. Under strong power the black layer is seen to be composed of ramified black pigment cells, and where the light shines through, the skin appears yellow. The mosaic layer is composed of polygonal cells, each of which consists of a basal half, which is

granular and colourless, while the upper half is made up of drops of yellow oil interspersed in the cell plasma. Since these cells interfere with the light, they are called the interference layer.

A vertical section through the skin shows the following arrangement. Below the black chromatophores, looking rather like the roots of a tree-stump turned upside down. More black is distributed in the upper strata of the cutis, near the interference layer. The superimposed protective epiderm may be left out of account, although its condition somewhat influences the whole phenomenon.

The presence of the layer of interference cells upon the underlying black background produces the well-known phenomenon of the colour of dense media—namely, blue. Only the rays of short wave-

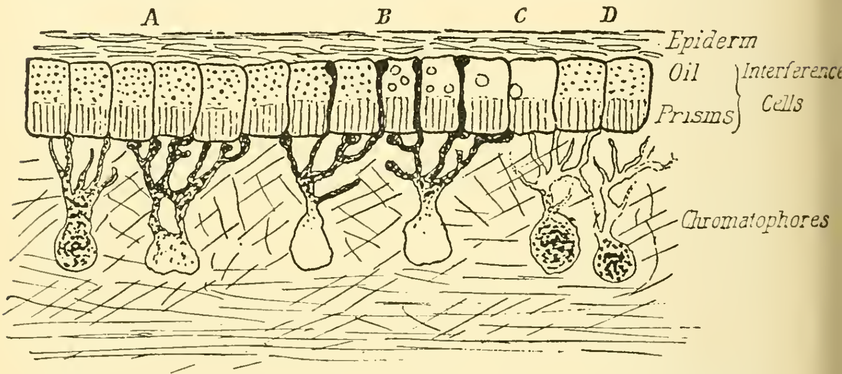

Diagram of a vertical section through the skin of a green Tree-frog, showing the chromatophores in various stages of contraction and expansion. The layer of Oil in the interference cells is indicated by dots, except where the Oil has coagulated into a few big drops.

At A the skin would appear green, at B dark brown, at C leaden grey, at D yellow.

lengths are reflected, while those of greater length are absorbed by the black background. Consequently the skin would appear blue, just as the sky is blue, or diluted milk becomes bluish, and the veins of thin and white-skinned people appear bluish. But in our Frog's skin the blue light is mixed with the yellow coming from the upper half of the cells, from the yellow oil, and the result of blue and yellow is green.

This can be shown experimentally. Application of a drop of caustic potash dissolves the guanine crystals, and the skin at once appears black. On the other hand, remove the yellow oil by extracting it in alcohol, then the green Frog looks blue. An instance of this is the scientific name of the grass-green Australian Tree-frog, *Hyla cœrulea*, so-called because it is always blue in our spirit collections. A scratch in the skin of a living green Tree-frog, just deep

enough to injure or disarrange the yellow layer, appears at once deep blue. If the lesion penetrates the crystal layer, it looks black.

When, by the action of the chromatophores, the black pigment is shoved towards the surface, the green becomes more saturated. When the pigment is pushed still further, between the interference-cells, or even enveloping them partly, then the skin assumes a dull dark-brown to black colour. Tree-frogs sometimes do this when they feel dull and cold.

Again, when the black pigment is withdrawn from the surface into the deepest strata, and when the chromatophores themselves are much contracted, the skin becomes yellow. Lastly, when the chromatophores are more or less contracted, and when the yellow colour, instead of being diffuse, concentrates into small drops, our Tree-frog assumes a leaden grey to silky whitish hue.

It is obvious that those Frogs and Toads which have only black and yellow pigments, but no interference layer, have command over but a limited change of colour, ranging from darker to lighter tints of black, brown and yellow. For instance, in the common Toad and in the common brown or grass-frog.

As a rule the changes are slow. They may take hours or days. One of the most striking by its rapidity of changes is *Hyla aurea*, one of the Australian Tree-frogs.

Whilst the mechanism is clear, the answer to the question, what these changes depend upon, is rather difficult. The play of the chromatophores themselves depends upon various causes. Stoppage of the circulation of the blood in the skin causes contraction of the black chromatophores. A slight overdose of carbon dioxide paralyses them, and they dilate. Low temperature causes also dilation; high temperature, contraction. Hence, hibernating frogs are much darker than they are in the summer. Frogs kept in dry moss, or such as are drying up, turn pale, regardless of light or darkness surrounding them. The Grass-frog and the continental or edible Water-frog seem to depend to a great extent upon temperature, or the amount of moisture in the air, so far as their changes are concerned.

The chromatic functions of Tree-frogs, on the other hand, depend greatly upon the sensory impressions received by the skin. For instance, a dark Tree-frog will turn green when put into an absolutely dark vessel in which there are fresh leaves. Rough surfaces cause a sensation which makes the frog turn dark.

The modern physiologist prefers looking upon all these changes as reflex actions, as not under the control of the will of the creature. Many even assume that the animal neither has control over its colour, nor does it know what it is doing. All this sounds very well in the laboratory, but the frogs, when observed in their native haunts, or even when kept under proper conditions, do not always behave as the physiologist thinks they should.

There is no doubt that in many cases the changes of colour are

not voluntary, not even subconscious, but pure reflex actions. It is quite conceivable that the sensation of sitting on a rough surface starts a whole train of processes: Roughness means bark, bark is brown, change into brown. But one and the same Tree-frog does not always assume the colour of the bark on which it rests. He will, if it suits him, remain grass-green upon a yellow stone, or on a white window-frame. The sensory impression received through the skin of the belly and through the tactile corpuscles of its fingers and toes is the same, no matter if the deal board be painted white, black or green. How does it come to pass that the frog adjusts his colour to a nicety to the general hue or tone of his surroundings, provided he is so inclined?

That is the point. Forty years ago Lord Lister was much nearer the true solution when, by careful observation and ingenious experiments he showed that the guiding impressions are those of the eye. The creature sees, studies its surroundings and then adjusts itself to them, if it thinks it necessary and if it is in the proper mood. This does not prevent such colour changes from becoming an unconscious habit, the habit having been repeated so often, and having become so strong, and last, not least, having proved so useful that at last it comes off whilst volition is suspended. In other words, the change has become a reflex action. But I insist upon this, that at any time the changes can be inhibited by the will, and can be produced at will.

A consideration of the coloration in its totality, the colour-pattern, is intimately connected with the use of the colours to these creatures. Paramount is concealment; and it is an almost universal law that colour is restricted to those parts of the body which are exposed, while neutral tints and white are relegated to the hidden parts, chiefly the under surface.

The production, and still more the distribution, of colour pigments, especially of pretty tints, is pre-eminently due to light.

Concealment and restriction of vivid colours to the exposed visible parts sound at first antagonistic—rather paradoxical—still more so when we remember that many Amphibia have a pattern, or combination of colours which renders them most conspicuous. It is difficult to generalise.

The simplest case of concealment is exemplified by the common Toad. Earthy-brown above, whitish below, with individual and local variations; but the effect is in harmony with the ground upon which this Toad hunts in the dusk.

The same principle applies to the vividly green Tree-frog, whitish below, green above, like the leaves upon which it spends most of its life. And we remember that these creatures can change colour besides, so as to adjust themselves to a nicety to their surroundings.

Then there are what I propose calling Flash colours—vivid, conspicuous—confined to such parts of the body or limbs as are absolutely hidden when the animal is at rest. These colours are shown off only on emergency, suddenly producing a bright flash. One of

the best examples is the little North American Tree-frog (*Hyla versicolor*). It prefers to rest upon bark; for instance that of an oak tree. It is, as a rule, delicately silky-grey, with most variable dark markings. Sometimes the bark is suffused with a tint of green, so that it is well-nigh impossible to discover the frog; but when disturbed he takes a tremendous leap. There is a flash of yellow or orange, due to the suddenly-exposed flanks and inner sides of the legs, and he is gone. He has dazzled his would-be captor by the display of unexpected colour.

A third principle is that of Warning colours; a combination of bold patches, usually saturated yellow, orange or red, upon a black background. All the Amphibia coloured thus are rather poisonous, the venom being contained in the numerous glands of the skin, and, although there are exceptions, they are practically safe from attacks. The conspicuous pattern acts as a warning to the enemy.

The combination of yellow and black in noxious creatures is a widely dispersed principle. It applies to wasps, many butterflies and moths; and the only poisonous lizard, Heloderma, the Gila monster of Mexico, is also black and orange.

Several interesting facts are connected with these warning colours. First, they are displayed upon those parts which are most exposed during the ordinary attitude of the Amphibian, the colours being intended to be seen. Secondly, the pattern is, as a rule, irregular, instead of following the general law of being symmetrically disposed. Lastly, they are subject to much individual variation.

Examples are, the Fire-Salamander, so common on the Continent, black, with yellow patches on the back; several species of the genus Spelerpes in North America. Many Newts, both in Europe and in America, have the warning colour displayed upon the under parts, e.g. *Triton torosus* of N.W. America and *T. alpestris*, the alpine Newt of the Continent. The same principle, display of conspicuous colours on the under surface, while the upper parts are inconspicuous, occurs in many Frogs and Toads of the most widely separated parts of the world. The reasonable explanation seems to be that these creatures spend a great part of their time in the water. When swimming, or suspended motionless in that element, their under surface is the one which is most exposed to attack from other aquatic animals, as fishes, water-tortoises, etc. An example is the little fire-bellied toad of the Continent (*Bombinator igneus*).

A third type of warning pattern is displayed by certain arboreal frogs, which are conspicuous on both the under and upper parts, and this pattern is irregular, asymmetrical, and, individually, variable, as usual in poisonous Amphibia. The best example is the South American genus *Dendrobates*, the poison of which is used by the South American Indians in various ingenious ways; for instance, poisoning of arrows to shoot monkeys with, and dyeing of live parrots.

To revert to the origin of the colour. We have already concluded that a dominant factor in the production of colour is sunlight. The

direct influence of the surroundings in which a particular frog happens to live, be these factors light, temperature or food, or all three combined, will probably affect or change the pigmentation in certain ways, perhaps at first to a very small extent, almost imperceptibly. It stands to reason that the offspring, living under similar conditions, will be acted upon in the same way. That factor, for instance, which has added green to the parents will add green to the children, until by cumulative inheritance a more decidedly green race is produced. And then, but not before, natural selection can step in. Then is the time to decide if the new colour is useful or not. If harmful, that race will soon come to grief; if advantageous, in ways hitherto not dreamed of, it will probably increase. In fact we have to resort to the direct influence of the surroundings, and this implies many factors. How can it otherwise be explained that the same kind of pattern, the same combination of colours, occurs not only in members of the most different groups of Newts, Toads and Frogs, but also in widely separated countries; in other words, in creatures which, we feel absolutely certain, had nothing to do with each other, except that they all are Amphibia?

Such instances of parallel or convergent development supply the strongest argument for assuming that it is the direct influence of the surroundings which models, in our case colours, the creatures, instead of working upon the basis of freaks. The latter idea implies an endless uninterrupted chain of accidental, spontaneous variation, always in the same direction. A single break in the chain, and the whole process would have to begin again. Black, red and yellow are due to corresponding pigments. It is possible that warmth and light, the distribution of the blood-vessels, unknown ingredients of food, have caused them to be deposited in the skin. We may even assume that the guanine crystals, which are certainly a product of the metabolism of the body, and which belong to the uric acid group, have, through the action of the light, been deposited in the skin, thus accounting for blue, green and white. These crystals and the black pigments are in reality noxious waste products, which, instead of being got rid of in the ordinary way, have stuck in the system, but deposited where they cannot do any harm. If their accumulation in the skin, by producing visible colour, happens to turn out useful, all the better, and then we may be sure that natural selection will be busy.

Now as to pattern of colour. The chromatophores are very susceptible to temperature and light, besides being controlled by the nervous system. Higher temperature and bright sunlight cause them to contract, and the respective parts become paler. Lower temperature and absence of sunlight dilate them. These very factors are actually at work whenever a Frog sits in sunlight and vegetation. There is light and shade. Some Tree-frogs—e.g. *Hyla hypochondrialis*, of South America, have such a delicately sensitive skin that the shadow of a blade of grass causes a rapid darkening of the shadowed part of the skin.

A very common pattern of Frogs and Tree-frogs is that of longitudinal stripes. One stripe, generally light, runs along the middle of the back; others, in pairs, extend from the head along the sides of the body and converge backwards. Others again, more irregular, begin on the flanks and run across the folded limbs. There is only one median line, and this is in a unique position and condition so far as nerve- and blood-supply are concerned. This median stripe makes a break in the look of the whole upper surface, thereby rendering the latter less conspicuous. The other stripes are ruled by the law of symmetry; right and left are counterparts. We have only to look at a Frog in its typical attitude when it sits in the grass. We then see at once that the shadows and lights of the green and brown stalks produce a pattern strikingly like that of the European Water-frog and the Australian *Hyla aurea*.

We may safely assume that such stimulative colouring has been in many cases the cause, the origin of the permanent and hereditary adaptive colouring. For when a species has lived for a long time under conditions which over and over again cause the chromatophores to be influenced in a particular way, causing an ever-repeated arrangement, then this pattern may at last fix itself on the animal. At first, by combined repetition, certain parts of the skin will be coached to react in the same way—a kind of predilection, later on a habit will set in, until this fixes itself unchangeably.

Of course we do not assume that our Frog has always been sitting under the same blades of grass, nor that the shadows fall upon the same spots. But grass is grass, and the general effects of its distribution of light and shade are the same, and all we assert is that the outcome of this stimulus will be a marshalling or exciting of the pigments in a linear direction. Nowadays the whole process is infinitely more complex. Our suggestion applies to the primordial frog, of which, however, we know nothing, except that it existed and must have acquired its colours and patterns at some time or other. Those individuals which we can watch now, start already with the inherited capacity of developing that pattern which their ancestors have managed to acquire during countless generations. This individual need not sit in the grass and wait for the colours and shadows to photograph themselves upon its back, and yet this same process is still going on. If our Frog did not live in a grassy country, but in other surroundings, it is absolutely certain that he, and to a greater extent his offspring, would develop into a new colour-variety. The common frog which lives on granitic soil assumes and keeps the speckled colour of the ground, and looks very different from those members of its kind which live on dark moorlands, or among rich vegetation. And these colour varieties have not the same range of changes, since their very pattern has been altered.

There are, in fact, many instances of what seems to support the idea of natural photography. This term has rightly been objected

to, but there are many cases which seem to show that whatever has produced the pattern has been an external influence. For instance, in the common grass-frog the series of spots on the flanks extend right across the folded limb, over the thighs, shanks and feet. They form longitudinal bands only whilst the frog is at rest. When it stretches the limbs the bands are disconnected, and wherever the opposed parts touch and hide each other, there is no special pigmentation. This is a very common arrangement in Frogs. Again, there are cases in which coloured lines go right across the eye, or, what is still more puzzling, right across the gape of the mouth, as, for instance, in the Argentine Toad (*Ceratophrys ornata*). This ugly-shaped creature has a beautifully coloured carpet-like pattern of black and yellow, with green patches. Each patch is surrounded by a narrow line of white or yellow dots interspersed with lines of rusty red and brown. Many of the spots look exactly like the little sun-images which sunlight throws upon the ground when streaming through dense foliage. The toad buries itself half in the soil, preferably in grass or under dense vegetation. If there is not enough green it throws little lumps of soil upon its back, the skin of which at the same time becomes more crinkled and assumes duller tones.

[H. G.]

ANNUAL MEETING,

Wednesday, May 1, 1901.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1900, testifying to the continued prosperity and efficient management of the Institution, was read and adopted, and the Report on the Davy Faraday Research Laboratory of the Royal Institution, which accompanied it, was also read.

Forty-seven new Members were elected in 1900.

Sixty-three Lectures and Nineteen Evening Discourses were delivered in 1900.

The Books and Pamphlets presented in 1900 amounted to about 262 volumes, making, with 653 volumes (including Periodicals bound) purchased by the Managers, a total of 915 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Honorary Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

President—The Duke of Northumberland, K.G. D.C.L. F.R.S.
Treasurer—Sir James Crichton-Browne, M.D. LL.D. F.R.S.
Secretary—Sir William Crookes, F.R.S.

Managers.

Sir Frederick Abel, Bart. G.C.V.O. K.C.B. D.C.L. LL.D. F.R.S.
Sir William de W. Abney, K.C.B. D.C.L. F.R.S.
Sir James Blyth, Bart. J.P.
Sir Frederick Bramwell, Bart. D.C.L. LL.D. F.R.S. M. Inst. C.E.
Thomas Buzzard, M.D. F.R.C.P.
Viscount Gort.
Donald William Charles Hood, M.D. F.R.C.P.
The Right Hon. Lord Kelvin, G.C.V.O. D.C.L. LL.D. F.R.S.
Sir Francis Henry Laking, K.C.V.O. M.D.
Hugh Leonard, Esq. F.S.A. M. Inst. C.E.
Frank McClean, Esq. M.A. LL.D. F.R.S. F.R.A.S.
James Mansergh, Esq. F.R.S. M. Inst. C.E.
George Matthey, Esq. F.R.S.
William Hugh Spottiswoode, Esq. F.C.S.
The Right Hon. Sir James Stirling, M.A. LL.D.

Visitors.

Sir Andrew Noel Agnew, Bart. M.P.
Charles Edward Beevor, M.D. F.R.C.P.
William Henry Bennett, Esq. F.R.C.S.
Francis Elgar, Esq. LL.D. F.R.S. M. Inst. C.E.
Joseph G. Gordon, Esq. F.C.S.
James Dundas Grant, M.D. F.R.C.S.
Lord Greenock, D.L. J.P.
Maures Horner, Esq. J.P. F.R.A.S.
Henry Francis Makins, Esq. F.R.G.S.
Sir Thomas Henry Sanderson, G.C.B. K.C.M.G.
William Stevens Squire, Esq. Ph.D. F.C.S.
Harold Swithinbank, Esq. J.P. F.R.G.S.
John Jewell Vezey, Esq. F.R.M.S.
Roger William Wallace, Esq. K.C.
James Wimshurst, Esq. F.R.S.

WEEKLY EVENING MEETING,

Friday, May 3, 1901.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Charles Mercier, Esq., M.B. F.R.C.S. M.R.C.P.

Memory.

The programme of my discourse to-night is a very modest one. I have no ingenious invention to describe; no startling discovery to announce; I can recommend no specific by which you may attain to the powers of memory of a Porson or a Macaulay; but, in thinking over the subject, it has seemed to me that under the term memory several things that are quite distinct have been included, and the task that I have set myself is to discriminate between these different meanings, to display some of their bearings on one another, and to bring this mysterious faculty into line with experiences which seem to us simpler and more intelligible.

In the first place, there is the very obvious distinction between memory and memories—between the process of remembering and the results of the process; the same distinction that we draw between the process of thinking and the thoughts that result, or between the process of building and the houses that result. I propose to deal first with the result, and then with the process.

The iron wire that you see here is clamped securely at the top, and at the lower end is attached to what is known as a twisting couple. It is so arranged that when a weight is hung on this cord the wire is twisted to the right, and when the weight is hung on this other cord, the wire is twisted to the left. When the weight is removed, the wire at once springs back to its original position. In this case the amount of distortion was within the limit of elasticity of the wire. Now I double the weight, and twist the wire further round, and now, when the weight is removed, the pointer shows that the wire does not return to its original position. It has been twisted beyond the limit of its elasticity, and has acquired what is called a "permanent set." We may look upon this permanent alteration in the shape of the wire as a memory, a structural memory, of the experience that it has undergone. At this position the wire will remain for ever unless it is subjected to some new experience. The memory that it has acquired is a permanent memory.

Now let us take this new position of the wire as our point of

departure, and again subject it to the action of the weight. We find that the application of the same weight produces no addition to the permanent set. We may apply the weight as many times as we please, or we may leave it hanging for a thousand years, and still, when it is removed, the wire will return to the same position, the set will not be increased. Besides the structural memory or change of shape, the wire has undergone another change, a change of the stresses among its particles, such that further modification in the same direction has become more difficult. The same force no longer suffices to produce a set. We may call this a dynamical memory of the experience that the wire has undergone.

If, instead of twisting the wire, we distort its shape in any other way, the result, *mutatis mutandis*, is the same. If the change of shape is within the limit of elasticity, the wire springs back to its former shape the moment the disturbing influence ceases to act. If the change of shape is beyond the limit of elasticity, the metal retains a permanent set, which is a statical or structural memory of the experience that it has undergone; and at the same time, with the permanent set, it acquires a change in molecular disposition, an alteration of the stresses existing among the molecules, such that in future it reacts differently to incident motion. It retains a dynamical memory of the experience.

What is true of iron is true in various degree of other metals, and what is true of metals is true in various degree of other solids, so long as these solids are unorganised. All possess some elasticity, though the limit of perfect elasticity varies very widely. All take permanent set, or entertain structural memories, and all react differently thereafter to incident motion, though the degrees in which they admit of these modifications are very various.

When we pass from unorganised to organised solids, we find that their behaviour under experiences of incident motion is in some respects similar, and in others is widely different. We find that they too are distorted by incident motion; that they too possess elasticity; that they too recover their exact shape after distortion within the limit of elasticity; that they too take a set when distorted beyond the limit of elasticity; and that they too react differently thereafter to motion incident upon them; but there are important differences.

Test the behaviour of a walking-cane or a whip-stock which is bent, and which we may straighten with the hands. By holding the stick at the ends of the bend, and placing the camber across the knee, the stick may easily be straightened. But as soon as we release it, it springs back again into its curve. It is elastic. In order to allow for this elastic recoil, we must over-bend it. We must bend it at least as far in the opposite direction as the bend that we are trying to obliterate. When this is done, and the stick is released, it springs back by its elasticity to a position intermediate between that impressed upon it and that in which we found it. So far the stick behaves in the same way as an iron rod would behave under the same

circumstances. It has taken a set. It has acquired a statical memory of the experience that it has undergone.

But now if we watch the stick, we shall find that this is not the end of the process. It has taken a set, it is true, but the set is not permanent. After remaining at rest for a certain time, the set begins to diminish, at first slowly, then more rapidly, and then with a speed that gradually diminishes. If we clamp the stick in a vice, and observe it carefully, we shall find that it continues for hours, and even for days, to return with diminishing speed towards its original shape. This is a new phenomenon, to which unorganised bodies exhibit no parallel. The stick takes a set, but the set is not permanent. It is a temporary set. It disappears, or at least it diminishes. The return towards its original shape is made, not with a single bound and there an end, but with an initial bound, and then, after a pause, by further progress, for a short time with increasing, and thereafter for a long time with diminishing, velocity. The statical memory of the unorganised body, once acquired, undergoes no change. That of the organised body does not remain constant, but soon begins to fade, and fades at first with increasing, and subsequently with diminishing, speed.

Still more different from that of the unorganised body is the dynamical memory of the organised tissue. When once a bar of iron has been bent, the application of the same force will produce no further distortion, however often it may be repeated, or however long it may be applied. The production of a set makes more difficult the production of a further set in the same direction. But when a stick has once been bent sufficiently to produce a temporary set, then the application of the same force will produce a further set. Its repeated application will increase the set in some proportion to the frequency of the repetition; its continued application will increase the set in some proportion to the time of its continuance. Thus the dynamic memory of the organised body is opposite to that of the unorganised body.

Such are the differences in the behaviour of the two substances when distorted beyond their respective elastic limits. But there are differences also when the distortion is within these limits. When a piece of steel suffers a distortion which is well within the limit of its elasticity, it will retain its former shape however often it may suffer distortion. The hair-spring of a watch will suffer distortion of its shape ten million times in the course of a year, and yet after many years of incessant action it will exhibit no perceptible change of shape. With organised bodies the results of repeated application of force are different. A bow that is in constant use "follows the string" at last; that is to say, it becomes permanently bent. It attains a true permanent set, which undergoes no diminution. So too, the top joint of a fishing-rod becomes permanently curved. A shelf will sag under a weight, and if the weight is at once removed, will entirely recover itself; but apply the weight sufficiently often,

or leave it long enough, and the sag will remain; the set will become permanent.

The behaviour of wood under distortion may be summarised thus:—1. It is, within limits, elastic, and if distorted within these limits it instantly recovers its shape when the distorting force ceases to act. 2. If distorted beyond its elastic limit, it retains a temporary set, which, after a time, diminishes with varying velocity. 3. If the distortion, whether within or without the elastic limit, is sufficiently prolonged, or repeated sufficiently often, the set becomes permanent. 4. The distortion produces in the wood such a change that subsequent similar distortions are facilitated.

So far as the present argument is concerned, the walking-cane, the whip-stock, the bow, the fishing-rod and the shelf may be looked upon as not only organised but live. They are, so far as concerns the reactions that we have considered, in the same state as when they formed part of the living tree. If we subject a dead branch to similar experiences, we find that it behaves in a totally different manner. It takes no set, but breaks. Now, regarding the wood as live, it is evident that if we could apply our observations on wood to other live tissues, our task of investigating at least the structural and dynamic memories of the human brain would be greatly facilitated. There is a very large body of facts, of which but a very small selection can be given here, which harmonise with this assumption, and indicate that we may take it provisionally as a working hypothesis without any great departure from likelihood.

Every organism has its specific shape, and if it is distorted by a transient agent, it will begin after a time to return to its normal shape, by degrees that at first are slow, for a short time increase in speed, and thereafter continually diminish in velocity. But if the distortion is very great, or is very often repeated, or is long continued, then the set becomes permanent.

A tree which is exposed to a prevailing wind, or which is darkened on one side, will grow lop-sided; and if it is exposed long enough to these conditions the distortion will be permanent. But if, while the tree is young enough, that is, before the distorting agent has acted too long, its action is arrested, the tree will, after a pause, begin to recover its symmetrical shape; and it will recover by stages which at first increase in speed, and then become year by year slower and slower. Nay, if the distortion is increased to actual disruption the same redintegration often takes place. If the branch of a tree be broken, or if a limb is torn off a crab or a lobster, the set that is produced is not a permanent set. After a pause, the lost part begins to grow again, and the growth at first increases in speed, and thereafter diminishes until it becomes imperceptibly slow, so that the reproduced part does not for long, perhaps does not ever, attain to the dimensions of the lost part.

Moreover, it is to be noticed that in proportion to the immaturity of the part in course of construction is its vulnerability. The green

succulent sprout, the soft immature claw, is more easily injured than its original. That is to say, when once it has been mutilated, when once it has been distorted, when once it has received a set, subsequent alteration in the same sense is facilitated.

Again, when the mutilation is repeated, subsequent reproduction is less vigorous; and if the mutilation is repeated sufficiently often, reproduction fails. The temporary set becomes, after sufficient repetition, a permanent set.

Similarly, when the higher animals are wounded, there is at first a pause, and then the process of repair sets in, with a speed which for a short time increases, and then gradually slackens, until the latest stages, the devascularisation of the scar, and its assimilation to adjoining tissues, become imperceptibly slow. Moreover, in proportion to the immaturity of the healing process is the vulnerability of the wound. It is more easily injured again than are uninjured parts. The set that has been impressed upon it facilitates a further set in the same direction. Furthermore, if the wound is opened again and again, or is not suffered to close, the healing process at length fails; the temporary set becomes a true permanent set.

When the old physicians spoke of the *vis medicatrix naturæ*, they did but express in other words the tendency of a set to disappear, the transiency of a distortion; and when the physicians of a later day insist upon the frequency with which acute change supervenes upon chronic, they do but state in different terms the existence of that dynamic memory which facilitates a further change in the same direction as a previous change.

It would scarcely be too fanciful a view to regard the succession of organisms in a race, as a continuous body, subject to the influence of distorting agents. Such a distortion takes place when a race of animals or plants is subjected to new conditions of life—for instance, when a wild animal is domesticated, or a wild plant brought into cultivation. In such a case the whole structure of the organism is profoundly modified. As long as the distorting agent acts, as long as the domestication continues, so long continues the distortion. But let individuals of the domesticated race escape and breed in wildness, and after a time they will begin to revert, both in themselves and in their offspring, to the feral type; and this change will take place at first with increasing, and subsequently with diminishing speed. The race has received a temporary set, a structural memory of its experiences in the farmyard, the garden or the greenhouse, a set which diminishes when the distorting agent ceases to act. If we wish to modify the form of an organism, we shall more easily succeed by choosing for our experiment one that has already undergone recent modification than one of fixed type, for practical breeders know, although they express their knowledge in other words, that a set once produced facilitates further change in the same direction.

It does not seem unjustifiable to infer that what is true of these gross and sensible distortions is true also of the delicate and infini-

tesimal distortions that are produced by every wave of motion that is incident upon the nervous system, which is so particularly and marvellously sensitive to disturbance by small increments of motion. We are justified in supposing that it is by the operation of what I have termed dynamic memory, by which a distortion facilitates subsequent distortion in the same sense, that the nervous system has acquired its marvellous sensitiveness to distortion by infinitesimal forces. And it is not a little significant that our conscious memories weaken and fade in much the same ratio to the lapse of time as does the structural memory of the stick. They remain bright and vivid for a short time, then they fade with increasing speed for a time, and then with slackening speed for an indefinite time thereafter.

The living organism is not only acted on; it reacts. It is not merely passive; it is active; and the mode of its action is determined by its structure. According as the structure is modified, so is the function modified. When a structured memory is formed in a tissue having an active function, all future function, all future action, is modified by the existence of the memory. This modification of function that is conditioned by the formation of a structural memory I call Active memory. So long as the structural memory lasts, so long is each exercise of function modified, and the degree of modification is in proportion to the degree of set that remains in the tissue. As the structural memory disappears with the lapse of time, so the mode of the function loses its new peculiarity and returns to the former mode.

Certain regions of the nervous system there are whose activity is accompanied by consciousness. When a new mode of activity occurs in these regions, a new mode of consciousness accompanies the activity. When the activity of that particular nervous process subsides, the accompanying state of consciousness dies away and ceases. But in the tissue is still left a structural memory, such that when that portion of tissue again becomes active, it becomes active in the same way. The process is punctually repeated, and the repetition of the activity, which I call active memory, is accompanied by a repetition of the mode of consciousness, which is conscious memory.

Thus there are four different conditions to which the term memory is applied. There is Structural Memory, which is an alteration in the position of the particles of the tissue. There is Dynamical Memory, which is an alteration among the stresses of the shifted particles. There is Active Memory, which is the altered process that takes place in the altered tissue; and there is Conscious Memory, which is the conscious accompaniment of active memories in certain regions of the nervous system. The three latter forms depend, it will be seen, upon the structural memory, to the consideration of which we may now return.

It is evident that the entire structure, not only of the nervous system, but of the whole organism, may be regarded as a group of statical memories; nay more, it is obvious that the same is true of

the whole material universe. Every modification of form, whether gross or molecular, in every material body, has arisen under the stress of incident motion, and may be regarded as the structural memory of that experience. Confining our attention to organised bodies only, it is evident that the form which every organism assumes, whether in external contour or in internal organisation, is the structural memory which it has retained of the experiences that itself and its ancestors have undergone. Extravagant as the statement appears at first sight, it needs but little consideration to show that it is literally true, and true also that day by day, hour by hour, and moment by moment, the organism, and especially its nervous system, is still acquiring structural memories under the experience of incident motion. So long as the structure thus modified remains functionally inactive, so long the conscious memory of the experience remains in abeyance. The moment that activity of function takes place in the tissue, at that moment a state of consciousness arises which is the counterpart of the state that occurred on the formation of the structural memory, and that is the conscious memory of that experience.

The memories of experiences that are registered in the nervous system may be compared with the memories of aërial vibration that are registered on the waxen cylinder of the phonograph. In both cases the structural change left by the experience bears no resemblance to the experience under which it arose. In both cases the structural change may remain for an indefinite time inert and passive, a mere change of shape, unaccompanied by any process which repeats or recalls the experience during which it was formed. In both cases the modified structure may be started into active function at any moment by appropriate addition of motion; and in both cases it is then, and then only, that an experience is reproduced which is the more or less accurate counterpart of the experience under which the alteration of structure took place.

Each of the four forms of memory that have been enumerated demands separate consideration.

Structural or Statical Memory.—Upon the structural change produced by an experience depend the endurance and the faithfulness of the conscious memory; indeed, when we speak of the endurance of a conscious memory, we use a figure of speech, for the conscious memory does not endure. What endures is the structural memory alone, and what is called the endurance of the conscious memory is the enduring liability of the structural memory to become active and to be attended by consciousness. Keeping this distinction in our minds, it is evident, from the account already given of structural memory, that the endurance of a memory depends upon the amount of set that is impressed by the experience on the tissue, and that this differs much, not only as to different experiences, but as to different parts of the same experience. Of every distortion of tissue that is produced, for instance, by an impression on the senses, some

parts are within the elastic limits of the tissue, and of these no memory remains; while other parts are distorted to various extents, and of these the endurance of the memory is in proportion to the degree of the distortion. In every distortion of tissue there are two elements to be considered: viz. the area over which it is distributed, and the degrees of change within that area; the number of particles displaced, and the amount of their displacement—and the amount of displacement differs much in different parts of the area.

The distortion of a body under the incidence of motion may be within or without the limit of elasticity of the body. If it is within the limit of elasticity, no set, no structural memory remains; and thus it is that of a vast number of our experiences we retain no memory at all. The faces that meet us in the street, the stones and cart tracks in the road, the leaves on the trees, the scraps of conversation, nay, much in the books we read, and in discourses that we hear, it may be in church, it may be in some scientific assembly such as this, are gone the instant they cease to impress the senses, and leave not a trace behind. And when the limit of elasticity is exceeded, and a temporary set remains, we have to recognise that it may vary much within the limits of the disturbed area. We have to regard the change both in its extension and in its intension; both in regard to the area over which it is distributed, and to the degrees of change within that area; both with regard to the number of particles displaced, and to the degree of their displacement. It may well happen, and in point of fact it does always happen, that the amount of distortion suffered by the area affected differs much in different parts of that area. Some parts are not displaced except within the elastic limit, and of the corresponding parts of the experience no structural memory remains—no conscious memory is possible. Of the parts that receive a set, the degree of set is not the same in all, and consequently the endurance of the memory differs much as to different parts of the experience.

Suppose that you arrive overnight in a foreign town, and when you look out of your window in the morning you see a varied landscape of mountain and lake, tower and town, foliage and snow. You turn away from the window, and instantly the greater portion of the scene is gone. No memory at all is retained of a thousand details which undoubtedly did impress the retina, and which, by appropriate means, might have been made to produce memories. Whatever distortion of tissue was produced by the motion arriving from these objects was within the elastic limit of the tissue, and the instant the distorting agent ceased to act, the distortion disappeared. Thereafter you do not find that the memories that you have acquired of the scene fade uniformly. What you retain, as years elapse, is not a fading uniform memory of the whole, but rather that it goes bit by bit. One part after another fades out until all that is left are the most striking features,—on the left-front a mountain of vague shape, from which all detail and all colour, save a white top, has gone; on

the right below, a stretch of blue water; below on the left a confused mass of buildings, without specific shape or colour; dark foliage somewhere between this and the mountain. All the details,—the boats, the moving people, the traffic in the streets, the specific shapes and spatial relations of objects,—all are gone, and all went, not simultaneously, but by gradual and successive effacement. So that we infer that over the disturbed area the disturbance varies much in degree, for the endurance of the set depends, as far as we know, upon its initial extent only.

Upon what then does the amount of the set depend, and what are the factors that determine it? We can scarcely be wrong in supposing that in this, as in other cases, the distortion produced in a body is directly proportional to the amount of the incident motion, and in inverse proportion to the inertia of the particles of the body—to the resistance which they oppose to distortion. The first factor need not be laboured. We all know that the stronger the impression made upon us, the longer and the more faithfully it is remembered. Our ancestors, when they took the schoolboys to the boundary of the parish and there flogged them, were quite aware that a strong impression produces an enduring memory; and the whole experience of our lives testifies to the truth of the statement. But something needs to be said about the second factor in the endurance of a memory, viz. the inertia of the tissue on which the motion is incident.

It is manifest from our daily experience that the inertia of the particles of nervous tissue differs immensely not only in different people, but in different regions of the same brain. We see constantly that the same experience which will produce an enduring set in the brain of one man will not shift the molecules of another beyond the elastic limit. One man will remember and repeat verbatim a leading article in the 'Times' after once reading, while it will take another half-an-hour to learn the Collect for the day. And in the same brain there are differences as wide. The same man who can repeat his leading article after a single reading is unable to recall the simple succession of musical notes that make up some popular tune, even after he has heard them a dozen times. But the man who can remember one verbal composition can remember another, and he who can remember one musical air, or one set of muscular adjustments, can remember another. We must admit, therefore, that there is in certain individual tracts of brain tissue a specific degree of inertia, but that this degree of inertia differs much, not only in different brains, but in different parts of the same brain. The great practical question for us is whether this inertia can be in any way diminished; whether there are any means that we can employ by which we can increase the amount of set that is produced without increasing the intensity of the impression which produces it. Undoubtedly there are such means, but before I state them let me deprecate the notion that there is any royal road to the production of what is called a good memory. The ability to remember may be vastly, indefinitely im-

proved; but it cannot be improved by charms, or nostrums, or conjuring tricks. There is but one way to improve the memory, and that is by patient and persistent labour. The only help that science can give is to show how that labour can be least wastefully employed.

It has been insisted, I fear with wearisome iteration, that structural memory, with which we are now dealing, is not peculiar to the brain tissue, nor even to living matter. It is common to all solids; and in all solids, under practically all circumstances, it is found that the inertia of the particles is diminished, and the production of set facilitated, by increasing the quantity of free motion among the particles. If we want to forge a bar of iron into a new shape, we can immensely facilitate the production of the set that we wish to impose by heating the bar to redness; that is to say, by greatly increasing the individual motion of its particles. The maker of whips or walking-sticks who wishes to bend a hook on a stick, or to straighten a kink out of its shaft, puts the stick in a bath of hot sand, and when he has by this means increased sufficiently the intrinsic motion of its particles, he can bend the stick into what shape he will, and whatever shape he then gives it will endure. The set is easily imposed, and it is permanent. And in the brain tissue also the production of set is facilitated by increase in the intrinsic motion of the particles of the tissue, for we find that whenever the brain is unusually active, the events that are then experienced are remembered with unusual tenacity. We find, for example, that the impressions that we experience in times of great emotional storm and stress are remembered with extraordinary faithfulness for very long periods. Of all that I experienced in the year 1866 I remember vividly the events of one night only, and that the night in which I was shipwrecked; but of that night I have still a very vivid remembrance. Dr. Johnson retained to the end of his life a memory of the appearance of Queen Anne, who touched him for the Evil when he was under three years old, and we can imagine how highly excited the child must have been at such a tremendous experience, and can understand therefore why he remembered it so well. Any one who has been in peril of his life in a burning house remembers for the rest of his life incidents that the firemen who rescue him have forgotten in five minutes, for the impression is made in the one case on a brain already in a state of high activity, in the other on brains which are comparatively quiescent. In these considerations, however, we gain but little practical aid, for a system of mnemonics which could ensure our recollecting an event only by burning down a house when it happened would be too costly for everyday use. Fortunately this is not our only expedient.

There is another mental state in which we have reason to believe that the intrinsic motion of the cerebral elements is increased, and in this state also experiences produce memories that are peculiarly enduring. This is the state that we call Attention. What may be the precise cerebral condition that underlies the act of attention we do not certainly know, but we can scarcely be wrong in asserting

that, whatever its precise nature may be, it is at any rate an active condition. It is a condition in which more free motion exists in the brain, the cerebral elements are in higher activity, the molecular or particular motion is of greater amplitude, than in states of inattention. And correspondingly we find that there is no means at our disposal so effectual for increasing the endurance of memories as giving attention to the experiences that we wish to remember.

There is a third influence which decreases the inertia of the cerebral particles, and makes them more mobile, more easily displaceable, and more apt to receive a set, and this is the habit or custom of being displaced. It is easy to understand that by frequent displacement their connections may become loosened, so that displacement may become more easy, and thus we account in part for the very great influence that practice or cultivation has in improving the memory. I fear it may be disappointing to find that all our researches lead to no easy specific for acquiring a good memory. It remains as true now as in past ages that nothing worthy can be achieved without labour, and to improve the memory there are but two means known to us—attention and practice. The more closely we attend to things, the better they are remembered; the more we practice and cultivate the art of remembering, the more will the memory improve.

A question which has often been mooted is whether we ever forget. Many cases have been recorded, of which the most striking and the best known is that related by Coleridge, which seem to indicate that structural memories may remain inert for an indefinite time, and may at length be revivified and become active. In Coleridge's case, an illiterate servant girl in the delirium of fever recited for hours together in Greek and Hebrew. She had been in the service of a learned pastor, who was accustomed to read the Greek and Hebrew classics aloud in her hearing. All unknown to herself these impressions had created structural memories in her brain, and upon a stimulus of exceptional intensity, these structural memories had become active. We may get a satisfactory answer to this question, I think, by considering the behaviour of the stick after distortion. If the distortion is within the elastic limit, undoubtedly the tissue returns to the *status quo ante*, and no structural memory whatever is retained. But if the distortion is sufficient to produce a temporary set, then, after a pause and a start, the set gradually diminishes. The tissue returns with continually diminishing speed to its original shape. But if this is so, and the experiments that I have made seem to indicate that it is, then the return of the tissue towards the *status quo ante* is an asymptote; that is to say, it continually approaches the state of rest, but never reaches it; and some degree of structural memory will always be retained. And so long as a structural memory exists, so long will an active memory remain possible. But with respect to the slighter distortions, to those which are within the elastic limit, undoubtedly they are completely forgotten.

The subject of Dynamic Memory is too vast to be more than touched upon. We have seen how the dynamic memory of living matter differs from that of not-living matter. In the one the production of a set renders more difficult, impedes and obstructs, further change in the same direction. In the other it facilitates a further change. It needs but little consideration to show that this remarkable property of living matter is the basis of all progress, of all improvement, of all intelligence, even of all morality. It is by virtue of this quality of living matter that practice makes perfect, that use becomes a second nature. It is owing to this property that repetition of an experience has so powerful an effect in fixing the memory of the experience. Every repetition of a distortion increases the set which the distortion leaves behind it; and when the distortion is repeated sufficiently often, the temporary set becomes a true permanent set. When once a new nervous process has been brought about, when a new fact has been observed, when a new muscular adjustment has been obtained, when a new train of reasoning has been thought out, when a new inhibition has been exercised, a change is produced, a dynamic memory is left in the nervous system, by which similar changes in the future are facilitated. Thereafter, not only is that particular fact more obvious when it is next met with, not only is that particular exercise of skill more easy, not only is that train of reasoning traversed with greater ease and rapidity, not only is that particular control more easily exercised, but beyond all this there is a larger change. Observation is increased in keenness not as to that fact only, but as to all facts of the same class, and in less degree as to all facts whatever. Skill in muscular adjustment, nicety and accuracy and deftness of movement, are increased, not only as to that particular act, but to all kindred acts; and in less degree as to all acts whatever. Ability to compare and to distinguish likeness and difference is increased, not only with respect to the matter about which we reasoned, but with respect to all kindred matters, and in less degree to all matters whatever. By each act of self-control not only is it easier to exercise self-control with regard to that particular indulgence, but to exercise self-control generally with regard to all indulgences; and thus on this property that I have called dynamic memory rests, as I have said, all progress and all morality.

Active Memory is no exclusive possession of the nervous system, nor even of living beings. The nervous system displays active memory not because it is a living tissue, but because it is a mechanism; and every mechanism, live or dead, animate or inanimate, displays in its working an active memory of the experiences that it went through during its construction. With very many mechanisms, outside as well as inside the nervous system, the structural arrangement can be altered, and thereupon the mode of working is so modified that the output is altered. A lathe may be so set that it produces a screw, or a cone, or a cylinder, or a sphere, or what not; and the form that its

activity takes is determined by the structural arrangement that has been impressed upon it. A loom may be set to produce a pattern of fern leaves, or roses, or zigzags, or Grecian keys, or what not; and whatever pattern it produces is determined by the structural memory that has been impressed upon it. According as the type has been arranged, so is the character of the print that is produced. And similarly, the cylinder of a phonograph may have impressed upon it the structural memory of a sonata, or a comic song, or a business letter, or a political oration; and when it is set in motion, the output, the mode of action, or what I have termed the active memory, faithfully reflects the structural memory. In all these cases the mechanism may remain for an indefinite time disposed in a structural arrangement, retaining a structural memory, but inactive. But when the loom or the lathe is connected with the engine; when the phonograph is connected with the battery; then the mechanism starts into activity, and then the form which its activity takes is determined by the structural memory. And similarly, the structural memory may remain for an indefinite time in the nervous system, latent and inert; but the moment the mechanism becomes active, its activity assumes a form which is determined by the structure, and thus the structural memory asserts itself.

Although the inanimate and the living mechanisms are precisely alike in the respects that have been considered, there is another respect in which they are profoundly different. Every exercise of activity in the inanimate mechanism impairs the structural memory. The bearings and screws of the lathe wear away, and the quality of the work deteriorates. The running parts of the loom wear out, and the quality of the cloth deteriorates. The face of the type is worn, and the print suffers. The gutter in the wax of the phonograph becomes smoothed by the friction of the style, and the definition of the sounds is spoilt. But with the nervous mechanism, owing to the opposite quality of the dynamic memory, activity has the opposite effect. Every exercise of activity on the part of the nervous mechanism consolidates and improves the structural memory, and renders its future activity more facile, more certain and more accurate.

Active memory in some parts of the nervous system, that is to say in those that are newly formed or in course of formation, is attended by Conscious Memory. So long as a new structural memory exists, but remains the seat of no active process, so long conscious memory remains possible, but not actual. Only when active process takes place in the altered structure does conscious memory arise. If by violence or by disease the structural memory is destroyed, the corresponding conscious memory is for ever lost. When we speak of a man with a well-stored mind, we mean that he has a large number of structural memories in the higher regions of his nervous system. The memories are not in his mind until the structure becomes active, and then the conscious memory arises.

So far there is a parallelism between conscious memory and the

physical memories. The more complete the structural memory, the more faithful is the conscious memory. The more vigorous the active memory the more vivid the conscious memory. But this parallelism exists only so far as we have traced it. It exists only for the first reproduction of the experience. With every subsequent recurrence of the active memory, the structural memory becomes more complete and enduring, and as the structural memory becomes more completely organised, so does the active memory become more easily evoked and more vigorous. But precisely in proportion as the active memory thus improves, in that same proportion does the conscious memory weaken and fade. It is the movements that are most habitual that are performed with least consciousness; it is in the places with which we are most familiar that we find our way with the least thought; it is the form of words that is most often in our mouths that is uttered with the least sense of its meaning; it is the scenery to which we are most accustomed that arouses the smallest interest. The more complete and consolidated and organised the structure, the more facile and certain and readily provoked the function, the less of conscious memory there is; and when structure becomes complete, and function perfect, conscious memory altogether disappears.

By thus distinguishing clearly between the several conditions to which the term memory has been applied, we are able to clear up some of the difficulties which perplexed our predecessors. One of the most puzzling problems with which the philosophers of a past generation had to deal was, What becomes of a memory when it is not actually being remembered? "We are conscious," says Sir W. Hamilton, "of certain cognitions as acquired, and we are conscious of these cognitions as resuscitated. That in the interval, when out of consciousness, they do continue to subsist in the mind, is an hypothesis, because whatever is out of consciousness can only be assumed, . . . but if it cannot be denied that the knowledge we have acquired . . . does actually continue, though out of consciousness, to endure, can we in the second place find any ground on which to explain the possibility of this endurance?" "The solution of this problem," he says in another place, "is to be sought in the theory of obscure or latent modifications (that is, mental activities, real, but beyond the sphere of consciousness)." Thus Sir W. Hamilton satisfies himself by an explanation that is purely verbal, and has no meaning whatever behind it; that is, indeed, as Mill pointed out, a contradiction in terms. To us the problem presents no difficulty. It is, in fact, wrongly stated. It rests upon a confusion about the facts. It is much the same as asking what has become of the colour of the sky at midnight? where does the motion of the engine reside when the steam is cut off? where does the light of the candle go to when the flame is blown out? where is the clangour of the bell stored away before the bell is rung? As we should explain it, conscious memories do not exist except in the process of revival, any more than the sound of the bell exists except when it is ringing; any more

than the light of the candle exists except when it is burning. What endures, when a conscious state is revivable but is not actually revived, is not a conscious state at all, but a structural modification of tissue. When this modified tissue becomes active, then the conscious state recurs, just as, when the bell is struck, it sounds again.

Entirely distinct from the four forms of memories that we have considered is the Process of Remembering. Active memory is a process, it is true, but a process different from the one that we are about to consider. Active memory is the activity of a mechanism; the process of remembering is the starting of the mechanism into activity. The one is the motion of the lathe, or the loom, or the printing press, when it is connected with the engine; the other is the process of making the connection between the engine and the machine, so that the structure may be put in motion. The memory is the appearance that arises in the mind, the process of remembering is the process of bringing this appearance into the mind. In short, what has to be explained is, how do we remember things when we want to? Supposing that we have a structural memory which is capable of becoming active, how can it be started into activity? There are two totally distinct ways in which this is done.

The common and ordinary way, which is often considered the only way, is by what is termed association, which is usually started somewhat in this manner. When two mental states have once been associated together in the mind, then the presence of one of them tends to drag the other in after it. It is by the operation of this law that when we see a friend we remember his name; and that when we hear the name of an object we remember its appearance. It is by the operation of this law that when we see a flash we expect a report; that a thunder-cloud reminds us of rain; that all things remind us, not only of things they resemble, but very often of their opposites—that black reminds us of white; silence reminds us of noise; heat of cold; and so forth. Many theories have been invented to account for the undoubted fact that remembrance takes place very frequently, and indeed usually, by association, theories with which I will not weary you. But I wish to place before you the intimate and necessary part which is played by Attention in the revival of memories. With reference to the engagement of attention upon them, memories may be divided into three classes. There are, first of all, those memories which, so to speak, jump up and slap us in the face. If I toss this glass tumbler into the air, you remember instantly and irresistibly that when it falls to the ground there will be a crash, and the glass will be broken. When you hear the word "cat," you have instantly the image of that luxurious and self-indulgent quadruped brought before your mental vision. You cannot help seeing momentarily the mental picture. It breaks in upon you; it captures you; it forces your attention will you nill you. "Cat!" There it is. You see it. You must see it. It will not be denied. Now bring before your minds the four forms of memory that I have been dealing with

to-night. Bring them up in the order in which they have been dealt with. In this case the memories do not come up with the same spontaneous vigour. They do not rise up with imperious clamour, and demand and compel your attention. On the contrary, the attention has to make the first advance. Before you can bring them before your mind, you are conscious of putting forth some slight exertion. You must go out a step to meet them. Now take another case, and try to remember what it was you had for breakfast yesterday morning. This is a much more serious affair. You have to put forth a very sensible amount of effort, you have to concentrate upon the subject a considerable grasp of attention before you can recall the experience to your mind. And finally, let me ask you to put yourselves again into the position in which doubtless every one of you has been at one time or another, in which some word or some name with which you are thoroughly familiar, of which you know quite well that you possess the structural memory, yet cannot be recalled to mind. You labour over it until you are weary, but you cannot remember it. Your attention searches laboriously the whole field of consciousness, lest peradventure the name should be lurking in some obscure corner, but it is not to be found; and presently the attention comes back, weary and footsore, from going to and fro in the field of consciousness and from walking up and down in it; or perhaps it would be more polite to say that it comes back like the dove into the ark, having found no rest for the sole of its foot. If the memory of the word is in the slightest degree active, if there is a presentation in any part of the field of consciousness, however remote from its focus, we can turn the glare of attention on it, and bring it into light; but if there be no active memory at all, then attention is powerless. Then we must sit down passively and wait events. And usually we do not have to wait long. Presently, when we least expect it, when we are thinking of something else, when the whole subject has passed out of our mind, and the attention is engaged elsewhere, the name jumps up and strikes us. It bangs at the door and demands admission. Now this I believe to be a very important mental phenomenon. Here is a conscious memory which rises up spontaneously and clamours for attention. It is not like the case of the smashing tumbler and the cat, for those memories were violently dragged into consciousness by their associations with states that were already present. But this name is not dragged in. It is not invited. It has received no provocation; but in it comes, and will not be denied. The importance of this occurrence is that it is a conspicuous instance of what, I believe, is very common in a less emphatic form. I altogether deny that all memories arise by association. I believe that very many memories arise spontaneously, and without any connection with what is already in the mind. I know it is so in my own case, and I believe that it is common with others. We are sitting thinking out some problem, or thinking that we are thinking out a problem, and suddenly, without any discernible provocation, some bit of doggerel

verse presents itself; or some visual scene: a bit of seashore seen in a long past holiday; the front of the house that we lived in twenty years ago; a room of the college in which we used to study, comes before the mind. It comes with no imperious clamour, but it rises, as it were, half-way, and gently but repeatedly solicits attention, until in the end it has to be attended to. I have often compared these unsolicited, unattached memories to flakes of bran in a boiling pot. They rise to the surface, brought up by currents that we cannot discern, they float for a while, and then subside, and give place to others that come similarly out of the depths. Trivial as these observations may appear, I believe that they are really by no means unimportant. They form, I believe, the bases of many of our dreams; and such successions of unconnected memories are often recognisable in delirium and in mania. In all of these states the content of consciousness is characterised by its disconnectedness. It is mainly composed of memories, and of memories that are not brought into the mind by association, but arise by their own spontaneity, and by their inherent vigour capture the wandering attention.

[C. M.]

GENERAL MONTHLY MEETING,

Monday, May 6, 1901.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Dudley Wilmot Buxton, M.D.
William Isaac Last, Esq.
James Stirling, Esq.
Henry Paul Whiting, Esq.

were elected Members of the Royal Institution.

It was announced that His Grace the President had nominated the following Vice-Presidents for the ensuing year:—Sir Frederick Bramwell, Bart., The Right Hon. Sir James Stirling, Sir William Abney, K.C.B., The Right Hon. Lord Kelvin, G.C.V.O., Mr. George Matthey, Mr. Frank McClean, Sir James Crichton-Browne, Treasurer, and Sir William Crookes, Hon. Sec.

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WEEKLY EVENING MEETING,

Friday, May 10, 1901.

George Matthey, Esq., F.R.S., Vice-President, in the Chair.

Professor Jagadis Chunder Bose, M.A. D.Sc., Professor of Presidency College, Calcutta.

The Response of Inorganic Matter to Mechanical and Electrical Stimulus.

When we pinch a living muscle, or send through it an electric shock, certain changes take place. A responsive twitch is produced; the muscle is changed in form, becoming shorter and broader; the particles of the living substance are strained under the stimulus. The effect of the shock then disappears, and the muscle is seen to relax into its usual form.

Mechanical Response.

This sudden change of form then, is one, but not the only, mode of response of a living substance to external stimulus. Under the stress the muscle is thrown into a state of distortion or strain. On the cessation of stimulus it automatically recovers. As long as it is alive, so long will it respond and recover, being ready again for new response. This brief disturbance of a living poise, to be immediately restored to equilibrium of itself, is quite unlike the rolling of a stone downhill from a push. For the stone cannot regain its original position, but the living tissue at once reasserts its first stable poise on the cessation of stress. Thus a muscle, as long as it is alive, remains ever-responsive. It is in intimate relation with the forces by which it is surrounded, always responding to, and recovering from, the multitudinous disturbances of its physical environment.

The living body is thus affected by external stimuli—mechanical shock, sound, electrical stimulus, and the stimuli of heat and light—which evoke in it corresponding responses.

In the case of the contraction of muscle by mechanical or electric shock, the effect is very quick, and the contraction and relaxation take place in too short a time for detailed observation by ordinary means. Physiologists, therefore, use a contrivance by which the whole process may be recorded automatically. This consists of a

lever arrangement, by which the contracting muscle writes down the history of its change, and recovery from that change. The record may be made on a travelling band of paper, which is moving at a uniform rate (Fig. 1). This autographic record gives us the most accurate information as to the characteristic properties and condition of the muscle. It gives us, too, its history and all its peculiarities.

Characteristics of the Response Curve: (1) Amplitude, (2) Period, (3) Form.

Just as one wave of sound is distinguished from another by its amplitude, period, and form, so are the curves of different muscles distinguished. For example, the period of tortoise muscle may be as large as several seconds, whereas the period for the wing of an insect is as small as $\frac{1}{300}$th of a second. In the same muscle, again, the form of the curve may undergo changes from fatigue, or from the effects of various kinds and quantities of drugs. In the autographic record of the progressive death of a muscle, the writing is bold and vigorous at first, but grows lethargic on the approach of death. In some strange way the molecules lose their mobility, rigidity supervenes, and the record of the dying muscle comes to an end. We may thus find out the effects of various external influences by studying the changes of form of the muscle curve.

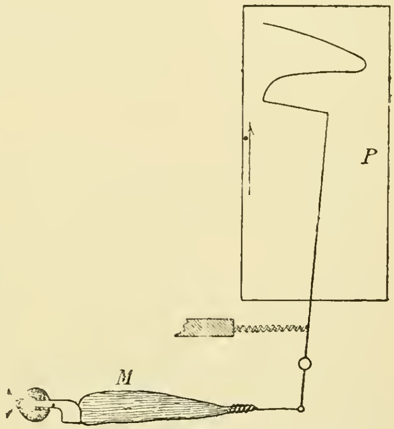

FIG. 1.—Mechanical Lever Recorder. The muscle M with the attached bone is securely held at one end, the other end being connected with the writing lever. Under the action of stimulus the contracting muscle pulls the lever and moves the tracing point to the right over the travelling recording surface P. When the muscle recovers from contraction, the tracing point returns to its original position. See on P the record of muscle curve.

Different Forms of Stimuli: Electrical, Mechanical.

We may stimulate the living substance in various ways — by light, or by thermal, chemical, electrical, or mechanical stimuli. Of these, the electric means of stimulation is the most convenient, whereas the mechanical gives rise to the fewest complications. With regard to this response of living substances, the most important matters of study are the responses to single stimulus and to rapidly-succeeding stimuli, and the modification of response by fatigue and drugs.

A single shock causes a twitch, but the muscle soon recovers its original shape. The rising portion of the curve is due to contrac-

tion, whereas the falling portions exhibit recovery (see curve in Fig. 1).

Incomplete Tetanus.

If, instead of a single stimulus, a succession of stimuli be superposed, the frequency of individual contractions also increases; the muscle has not time to recover; we get a jagged curve (a^1, Fig. 11). But when the frequency is sufficiently increased, the intermittent effects are fused, and we get an almost unbroken curve. When the muscle attains its maximum contraction (corresponding to the frequency and strength of stimulus), it appears to be held rigid, and recovers only on the cessation of stimulus (b^1, Fig. 11).

Complete Tetanus.

Fatigue.

When the muscle is continuously excited it grows fatigued. The height of the curve grows continuously less. This is seen in a series of single twitches (Fig. 4). It is also seen in tetanus, where there is a decline of the upper portion of the curve.

Influence of Drugs.

Drugs may act as stimulants, or produce depression, according to their nature. As extreme cases of such depressing agents we may instance poisons, which kill the response of living tissue. All signs of sensibility then disappear.

Other Modes of Expression of Living Response.

This mechanical method of studying the response of living substances is, however, very limited in its application. For example, when a piece of nerve is stimulated, there is no visible change of form. When light falls on the retina there is no change of form, but it responds by transmitting to the brain a visual impulse. What, then, is this visual impulse which is sent along the optic nerve, causing the sensation of light?

Thanks to the work of Homgren, Dewar, McKendrick and others, it is possible to answer this question. If we excise an eye, say of a frog, and substitute a galvanometer in the visual circuit in the place of the perceiving brain, it is found that each time a flash of light falls on the eye there is produced an electric twitch—that is to say, there is a sudden production of current, which ceases on the cessation of light-stimulus. Stronger light produces stronger electric twitch in the galvanometer, just as it produces stronger sensation in the brain. The visual impulse thus appears to be the concomitant of an electric impulse. This conclusion is supported by the fact that a luminous sensation is occasioned (without the action of light), by simply sending an electric current to the brain through the optic nerve.

The visual circuit is therefore like an electric circuit. The retina is a potential voltaic element. The nerve is the conductor. The brain is the detector of current, or a very highly sensitive galvanometer. Unless these three elements are in good order, no light-message can be perceived. We must have the current-generator or retina, and

the conductor or optic nerve, free from injury. Finally, just as the galvanometer will fail to detect a current if its suspension-thread be broken by rough usuage, so, after a violent blow, the brain will no longer perceive, though the terminal organ, the retina, and the connecting optic nerve may be intact.

So we see that stimulus evokes an electrical change also, in a living tissue. I shall now proceed to enter into some detail regarding this electric mode of response.

Hydraulic Model.

The various complicated phenomena of electric response may perhaps be rendered more easily intelligible by means of a hydraulic model (l, Fig. 2). Imagine an indiarubber pipe full of water, whose two ends A and B are at the same level. There would then be no current in the side or canal-pipe P. But suppose the end A is struck, a wave of disturbance will travel from A towards B. At a given moment the level at the A end will be raised,

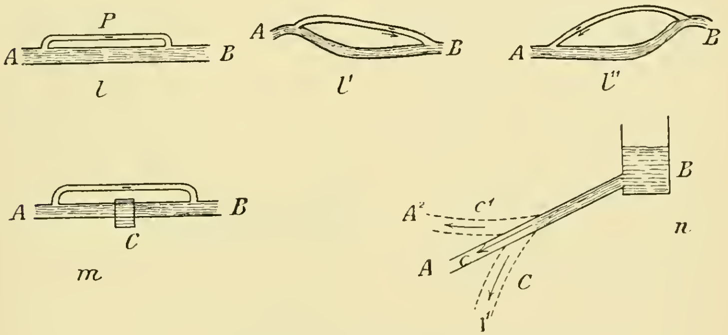

Fig. 2.—Hydraulic Models.

and the side tube will exhibit a current from A to B (l^1, Fig. 2); but, after a little while, A will subside to the normal level, the disturbance having meanwhile travelled to B, whose level will now be raised. The current in the side tube will now be reversed in direction (l^{11}, Fig. 2). A disturbing shock applied to one end of A B will thus produce a diphasic variation, and a float or indicator in the side pipe will exhibit this effect by alternate movement, first to one side and then to the other. If, however, the rate of transmission of disturbance be very great, then the indicator will fail to show any movement, inasmuch as it will be acted on by two equal and opposite impulses almost simultaneously.

1. To make the indicator exhibit the effect of shock in producing disturbance of level, we may proceed as follows. We may clamp the pipe in the middle at C (see *m*, Fig. 2), so that when one end is struck the disturbance may not proceed to the other end, the clamp

acting as a block. In such a case, when A is struck, the indicator will move to the right; when B is struck, it will move to the left. Thus we obtain effects which are reversible.

2. Or we may detect the effect of shock by the variations that it may produce in the intensity of the current. Take the case of *n*, Fig. 2, where there is a permanent difference of level between the two ends; one end, say B, being also more securely held, so that a shock produces less disturbance of level there than at A. As there is a permanent difference of level between B and A, there will be a current the normal intensity of which (c) will depend on the resting difference of level between B and A. If the pipe be now struck, A will be relatively more disturbed, and there may then be produced either a decrease (B A^2) or an increase (B A^1) of original difference of level. In the former case we shall have less current (c^1), that is to say, the shock will have the effect of *a negative variation* of current; in the latter case there will be a greater flow (C) or *a positive variation.*

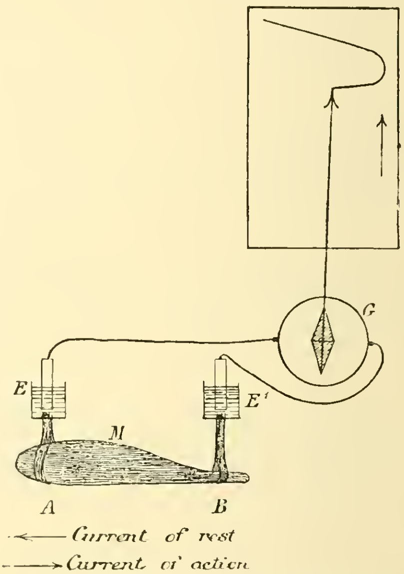

Fig. 3.—Magnetic Lever Recorder. *M* muscle; *A* uninjured, *B* injured ends. *EE*' non-polarising electrodes connecting *A* and *B* with galvanometer *G*. Stimulus produces "negative variation" of current of rest. Index connected with galvanometer needle records curve on travelling paper (in practice, moving galvanometer spot of light traces curve on photographic plate). Rising part of curve shows effect of stimulus; descending part, recovery.

These models may help us in framing a mental image of that electrical variation which constitutes the response to stimulus of a living tissue.

Electric Response.

If we take a piece of living muscle whose surface is uninjured, then any two points (A and B) on such surface being in a similar molecular condition, their electrical level or potential will be the same. They are *iso-electric.* No current will be exhibited by the indicating galvanometer when two non-polarisable electrodes* con-

* Zinc rods in solution of $ZnSO_4$, with two dependent strips of cloth, moistened with NaCl solution passed round the muscle at A and B.

nected with it are applied to A and B. But if one of the two points, say B, be injured by a cut, or burn, application of strong acids, or by alkalies, then, the conditions of A and B being different, there will be a difference of electric level or potential between them, and a current will flow from the injured to the uninjured, that is from B to A (Fig. 3). This current remains approximately constant as long as the muscle is at rest, and is for this reason known as "current of rest." As it is primarily due to injury, it is also known as "current of injury." If now the muscle be thrown into an excitatory state * by stimulus, there will be a greater relative disturbance at the uninjured A, and the original difference of electric level will be disturbed. In this case we have an analogue to c' in n, Fig. 2, where the shock produced a decrease of original difference of level. There would thus be a *negative* variation or diminution of the original current of rest. This negative variation is sometimes called an "action current." The transitory electrical variation constitutes the "response." Its intensity measures the intensity of the stimulus.

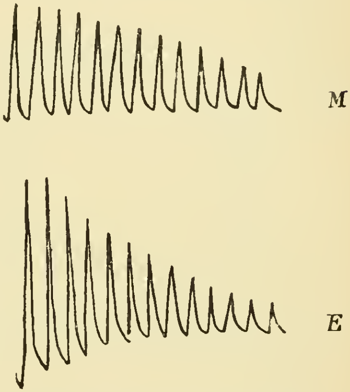

Fig. 4.—Simultaneous records of the (M) mechanical and (E) electrical response of gastrocnemius muscle of a frog. The muscle exhibits fatigue (Waller).

But we saw in the hydraulic model the possibility of a positive variation or increase of current, by shock. This is also found to be the case in some types of living response. In the retina, the stimulus of light produces a positive variation. It will thus be seen that there are two kinds of response given by living substances: (1) the negative, instanced by muscle, and (2) the positive, shown by the retina. Again, the same tissue under different conditions may give rise to responses having opposite signs. Thus Dr. Waller finds that while fresh nerve gives negative, the stale nerve gives positive variation.†

We have here, then, a way of obtaining curves of response by electric means. After all it is not very different essentially from the mechanical method. In this case we use a magnetic lever, the needle of the galvanometer, which is deflected by the electric pull of the current, generated under the action of stimulus, just as the mechanical lever was deflected by the mechanical pull of the muscle contracting under stimulus (Fig. 3).

* The excitatory reaction is, in the case of some living substances, of a more or less local character. In others, as nerves, it may be conducted to distant points.

† *See* Waller, 'Animal Electricity,' p. 61.

If a piece of muscle be taken, and simultaneous records of its response be made by the mechanical and electrical recorders, it will be found that the one is practically a duplicate of the other. This is well shown in a pair of records taken by Dr. Waller, and here reproduced (Fig. 4). It will be seen that the peculiarities of either curve are re-exhibited in the other. The muscle acted on grew fatigued, and both sets of response-curves show this effect by their gradual diminution of amplitude.*

Response in Plants.

I find that the electric response seen in animal tissues is also strongly exhibited by the tissues of plants. For experimental illustration we may take the leaf-stalk of horse-chestnut.†

1. Let us take such a stalk, and securely tying two strips of moistened cloth to A and B to prevent shifting of contact, connect these with two leading non-polarisable electrodes, E and E^1 (see Fig. 5). From what has been said before, it will be seen that these two points being practically iso-electric, little or no current will flow through the galvanometer. If, now, we apply a mechanical stimulus to the whole stalk either (1) by tapping or (2) by holding it at its two ends, and giving it a rapid torsional vibration, we shall have similar disturbances produced both at A and B, and there will be practically no resultant current of response.

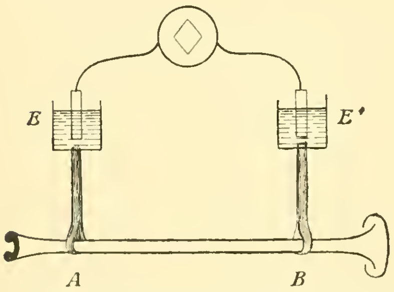

Fig. 5.—Response in plants. There being no block, effects at *A* and *B* are equal and opposite; hence no resultant effect.

2. We may now use the block method (Fig. 6). That is to say, the stalk is held securely in the middle by a clamp C, so that a disturbance made at one end will not reach the other. The electric contact is made with the uninjured, therefore iso-electric, points A and B, by securely tying the stalk with strips of moistened cloth at those points, as in the experiment just described. If now the A half be subjected to taps, or to torsional vibration, there will be a current of response through the stalk from the excited to the unexcited end. If the B end be next excited, a current in the reverse direction will be observed, in this case also from the excited to the unexcited end (*a*, Fig. 6).

* Waller, 'Animal Electricity,' p. 13.

† Various parts of plants—leaves, stems, stalks, and roots—will give electric response. In some there is rapid fatigue, whereas in others there is little fatigue. I intend to publish at a future date a more detailed account of these responses and their modifications by anæsthetics, poisons, and other agencies.

3. Or, taking again an unblocked stalk, let one contact be made in the usual manner at the end A, and the other at the end B, which is now injured by a cut (see Fig. 7). There will now be a permanent difference of electric level between the two ends, and a current of injury will be found to flow through the stalk from the injured to the uninjured. This contact at the injured end may be made in a very simple manner by passing a strip of moistened cloth through a slit in the stalk at B. Or, better still, instead of the cut

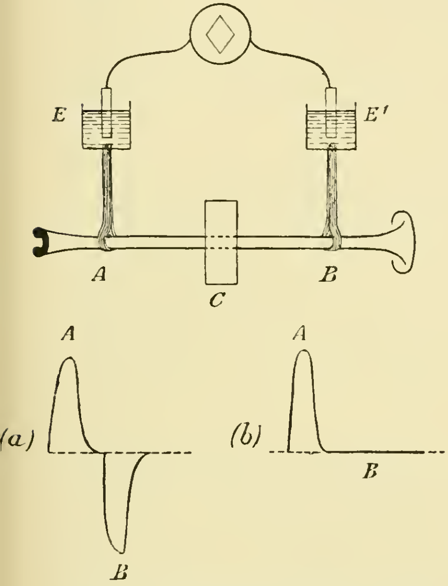

FIG. 6.—Response in plants by block method and response curves. *C*, clamp or block. Stimulation of *A* end produces current in one direction, that of *B* end in opposite direction, as shown by curves given in (*a*). In (*b*) is shown abolition of response in *B* half when killed.

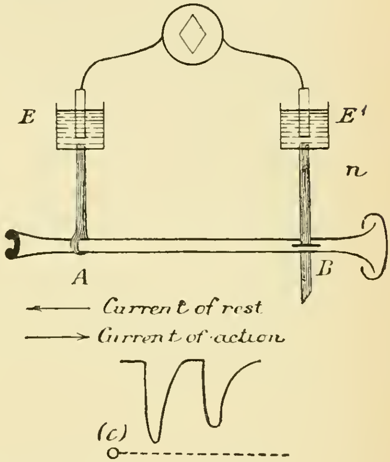

FIG. 7.—Response in plants—negative variation. There is a resting current owing to injury at *B*. Stimulation produces a diminution of this resting current, as shown in (*c*). The dotted line represents the galvanometer zero.

we may use a few drops of strong KHO solution, to injure the B end. If now the stalk be subjected to mechanical stimulus, it will be found that there is a responsive negative variation, or a diminution in the original current of rest (*c*, Fig. 7).

Thus we see that under stimulation the plant, like muscle or nerve, is thrown into an excitatory state of which the electrical change is the concomitant, this electric response being regarded by physiologists as proof of the living condition of the substance.

4. But how can we be certain that this electrical indication is peculiar, *sui generis*, to the physiological or living state? The crucial

test is supposed to lie in the modification of response under anæsthetics or poisons, when "that which is physiological, i.e. dependent on the physico-chemical conditions peculiar to the living state, will be suppressed; that which is purely physical will persist." * In order, then, to determine whether response in plants is or is not of a physiological character, we may subject them to the action of chloroform. Taking a fresh stalk we get the usual strong response. We now apply chloroform, and find as anæsthetic action proceeds, that the responses wane, and are finally abolished. There are various other poisons which I find to be very effective in killing response.

5. The physiological nature of the response may be further demonstrated if we repeat experiment 2, after killing the stalk by brief immersion in hot water. No response current will now be evoked on stimulation of either A or B end.

As the conditions in 2 and 5 were exactly similar, except for the fact that in the former case the stalk was alive, and in the latter killed, on the method of difference we are justified in concluding that the response was physiological, or characteristic of a living state of matter.

6. Or we may demonstrate the same fact in a more striking manner by a modification of experiment 2. One half of the stalk, say the B half, is killed, by dipping that half in hot water. On now subjecting the B half to stimulation, there is no response; but stimulation of the A end gives strong response (*b*, Fig. 6).

Universal Applicability of the test of Electric Response.

Nothing has yet been said of the advantage of the electrical over the mechanical method of obtaining response. As has been said before, the mechanical method is limited in its application. A nerve, for example, does not undergo any change of form when excited, and its response cannot therefore be detected by this method. But by the electrical method we are able to detect, not only the response of muscles, but that of all forms of living tissue.

The intensity of electrical response is also a measure of physiological activity. When this physiological activity of the living substance is diminished by anæsthetics, the electrical responses are also correspondingly diminished. And when the living tissue is in any way killed, the electrical response disappears altogether. Hence it is said that "the most general and the most delicate sign of life is the electrical response." †

Thus, electrical response is regarded as the criterion between the living and non-living. Where it is, life is said to be; where it is not found, we are in presence of death, or else of that which has never lived: for in this respect there is a great gulf fixed between the

* Waller, 'Animal Electricity,' p. 104.

† "*The Electrical Sign of Life*. . . . An isolated muscle gives sign of life by contracting when stimulated. . . . An ordinary nerve, normally connected

organic living and the inorganic or non-living. The phenomena of the inorganic are dominated merely by physical forces, while on the other side of the chasm, in the domain of the living, inscrutable vital phenomena, of which electric response is the sign-manual, suddenly come into action.

But is it true that the inorganic are irresponsive? That forces evoke in them no answering thrill? Are their particles for ever locked in the rigid grasp of immobility? As regards response, is the chasm between the living and inorganic really impassable?

Thanks to the courtesy of the authorities of the Davy-Faraday Laboratory, I have been enabled to complete the investigations on this subject, commenced in India, under this very roof. I shall now proceed to submit the question before you to an experimental test.

Inorganic Response.

Taking a piece of tin wire, I arrange it in exactly the same way as the stalk of the horse-chestnut (see Figs. 6 and 8). That is to say, it is clamped in the middle, and secure electrolytic contacts are made, through non-polarisable electrodes, which lead to a galvanometer. If all strains have been completely removed, the two points, A and B, will be found iso-electric. If now I take the end A and strike it, or subject it to torsional vibration, you will observe that the galvanometer spot on the screen, hitherto quiescent, moves in one direction, showing the existence of the "current of action." I stop the disturbance, and you watch it creeping back to its original position, exhibiting a complete recovery. As long as the wire is excited, so long will the electric variation persist. Greater intensity of vibration will produce greater electric variation. Stimulation of the B end will produce a deflection in the opposite direction.

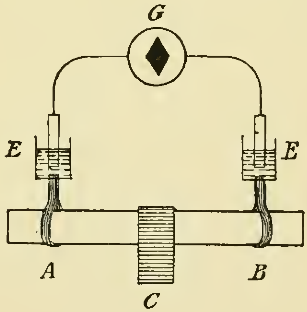

Fig. 8.—Experimental arrangement to show electric response in metallic wires. *C*, clamp.

Or, following experiment 3, we may demonstrate the fact of electric response by the method of injury. One end of the wire is touched with KHO, and the usual current of injury is observed. On now stimulating the wire, a diminution of this current of injury, or *negative variation*, will be produced.

with its terminal organs, gives sign of life by means of muscle, which by direct or reflex path is set in motion when the nerve-trunk is stimulated. But such nerve, separated from its natural termini, isolated from the rest of the organism, gives no sign of life when excited, either in the shape of chemical or of thermic changes, and it is only by means of an electrical change that we can ascertain whether or no it is alive. . . . The most general and most delicate sign of life is then the electrical response."—Waller, in 'Brain,' pp. 3 and 4, Spring 1900.

With tin wire under normal conditions, the current through the wire is always from the unexcited to the excited end, and from the excited to the unexcited through the galvanometer.* But just as in living substances we find two opposite kinds of response (e.g. nerve giving negative and retina positive variation), so also the responses given by some inorganic substances are of opposite signs to that of tin. For example, silver sometimes, especially in cold weather, passes into a peculiar molecular condition in which it gives the reverse response to that of tin, the "action current" in the wire being from the excited to the unexcited. An interesting transition from one class to the other is sometimes found in the behaviour of lead. Under feeble stimulus the current is away from the stimulated, and under stronger, towards that end. The majority of metals, however, behave like tin.

This simple form of experiment with metallic wire has been devised for the special purpose of bringing the essential points out clearly. But it labours under certain defects. Unless carefully carried out, there may be shifting of contacts; there may be variations of resistance by the evaporation of the liquid contacts; and quantitative measurements also are rendered difficult, for want of some means of graduating the intensity of stimulus. I will now describe a perfect form of apparatus for exhibiting the electrical response of metallic wires to mechanical stimulus, in which all these difficulties have been completely overcome.

Experimental Modifications.

In the typical experiment (Fig. 8), instead of making the galvanometer connection through electrolytic contacts we may cut A B into two (*b*, Fig. 9), and place the galvanometer in the gap, connecting A B directly by electrolyte.

This leads to *c*, Fig. 9, where A and B are held parallel to each other in an electrolytic bath (water).† Mechanical vibration may now be applied to A without affecting B, and *vice versâ*.

The actual apparatus, of which this is a diagrammatic representation, is seen in *d*, Fig. 9.

Two pieces, from the same specimen of wire, are clamped separately at their lower ends by means of ebonite screws, in an L-shaped piece of ebonite. The wires are fixed at their upper ends to two electrodes (leading to the galvanometer), and kept moderately and uniformly stretched by spiral springs. The handle, by which a torsional vibration is imparted to the wire, may be slipped over either electrode. The amplitude of vibration is measured by means of a graduated circle, not shown in the figure.

* The galvanometer in the above arrangement is interposed, as it were, in the electrolytic part of a voltaic cell. The portion of tin wire under excitement becomes zincoid. I mention this, as some misunderstandings and wrong inferences have arisen from not distinguishing between the direction of the current in the electrolytic part and that in the rest of the circuit.

† In all the experiments hereafter described the electrolyte is water unless the contrary be stated.

It will be seen from these arrangements:

(1) That the cell depicted in *d*, Fig. 9, is essentially the same as that in Fig. 8.

(2) That as the wires in the cell are immersed to a definite depth in the electrolyte there is always a perfect and invariable contact between the wire and the electrolyte. The difficulty as regards variation of contact is thus eliminated.

(3) That as the wires A and B are clamped below, we may impart a sudden molecular disturbance to either A or B by giving a

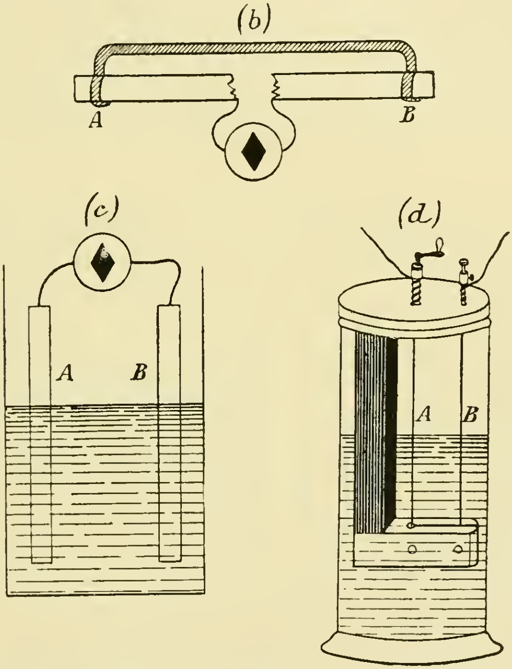

Fig. 9.—Modifications of experimental arrangement to show electric response in metals.

quick torsional vibration round the vertical wire, as axis, by means of the handle. As the wire A is separate from B, disturbance of one will not affect the other. Vibration of A produces a current in one direction, vibration of B in the opposite direction. Thus we have means of verifying every experiment by obtaining corroborative and reversal effects. When the two wires have been brought to exactly the same molecular condition by the processes of annealing or stretching, the effects obtained on subjecting A or B to any given stimulus are always equal.

But if, to begin with, the two were not in the same molecular condition, an initial P.D. would exist between them, and then, owing to the difference in the anodic and kathodic sensitiveness, the responses given by the two would not be identical.

Usually I interpose an external resistance varying from one to five megohms according to the sensitiveness of the wire. The resistance of the electrolyte in the cell is thus relatively small, and the galvanometer deflections are proportional to the E. M. variations. It is always advisable to have a high external resistance, as by this means one is not only able to keep the deflections within the scale, but one is not troubled by minute accidental disturbances.

When the cell is freshly made, the wires, owing to the strain set up during the mounting, may exhibit slightly erratic responses. Both should then be short-circuited, and after being subjected to vibrations for a time, the cell should be allowed a short period of rest. In this way, after a little practice, it is always possible to bring the response to a normal condition. The responses subsequently obtained become extraordinarily consistent. There is no reason why perfect results should not be arrived at, if these conditions are fulfilled.

Application of Stimulus.

If now a rapid torsional vibration be given to A (or B), there will be induced an electromotive variation. The intensity of stimulus is increased with the amplitude of vibration. Greater intensity of response is always obtained with greater intensity of stimulus.

Considerations showing that Electric Response is due to Molecular Disturbance.—1. The electromotive variation varies with the substance. With superposition of stimuli, a relatively high value is obtained in tin, amounting sometimes to nearly half a volt, whereas in silver the electro-motive variation is only about ·01 of this value. The intensity of the response, however, does not depend on the chemical activity of the substance, for the electromotive variation in the relatively chemically-inactive tin, and even gold, is greater than that of zinc. Again, the sign of response in silver, positive or negative, depends on its molecular condition.*

2. It may be thought that the electro-motive variation is due to some thermo-electric effect, inasmuch as the wire may be heated by vibration. The heat produced by a single vibration, however, must be very small. In order to test whether heating of the wire would produce effects comparable in magnitude to that produced by vibration, I made a cell with lead wire (the external resistance interposed in the circuit was 100,000 ohms). On subjecting one wire to the heating action of concentrated light from an arc lamp, during a continuous exposure of one minute, the effect on the galvanometer was a deflection of barely one division of the scale. But when the same wire was subjected to five quickly succeeding vibrations, lasting altogether only a few seconds, there was produced the very large deflection of 180 divisions.

Numerous other effects will be described presently which cannot be explained on the thermo-electric theory of action. I find for instance that the

* It is curious to note that the response of silver filings to Hertzian waves also depends on the molecular condition of the silver. In one condition there is produced a diminution of resistance, or positive effect: in the other the resistance is increased, i.e. the effect shown is negative. (See my Paper on "Electric Touch," Proc. Roy. Soc., Aug. 1900.)

intensity of response is very much modified by the effect of varying doses of chemical reagents. For example, with a ·25 per cent. strength of potash solution, the response was 57 divisions; but the increase of this strength to ·75 and upwards completely abolished all response.

3. It may be urged that the electro-motive effect is due in some way to (1) the friction of the vibrating wire against the liquid, or (2) some unknown surface action at the point in the wire of the contact of liquid and air surfaces. It is, however, to be remembered as regards (1), that the amount of this friction is exceedingly small; the movement of the wire at the lower fixed end being zero, that at the upper end is through an angle of about 180°. (2) Variation of surface, similarly, must be almost non-existent under the arrangements adopted for experiment.

Both these questions may, however, be subjected to a definite and final test. When the wire to be acted on is clamped below, and torsional vibration is imparted to it, a strong molecular disturbance is produced. If now it be carefully released from the clamp, and the vibration repeated as before, there could be little molecular disturbance due to torsion of the wire, but the liquid friction and surface variation, if any, would remain. The effect of any slight disturbance outstanding owing to shaking of the wire would be relatively very small.

We can thus determine the effect of liquid friction and surface action by repeating experiments with and without clamping. In a tin wire cell (with interposed external resistance equal to 1,000,000 ohms), the wire A was subjected to a series of vibrations through 180°, and a deflection of 210 divisions was obtained. A corresponding negative deflection resulted on vibrating the wire B. Now A was released from the clamp, so that it could be rotated backward and forward in the water by means of the handle. On vibrating the wire A no measurable deflection was produced, thus showing that neither water friction nor surface variation had anything to do with the electric action. The vibration of the still clamped B gave rise to the normal strong deflection.

As all the rest of the circuit was kept absolutely the same in the two different sets of experiments, these results conclusively prove that the electromotive variation is solely due to the molecular disturbance produced by mechanical vibration in the acted wire.

The question of surface action again can be finally disposed of if we take a cell (with external high resistance) and tilt it backwards and forwards. This will produce a great surface variation, yet little or no current will be detected. But vibration of the wire will produce the normal strong response.

The same strong response is obtained, further, when the air surface is completely abolished, vibration being communicated to a completely-immersed wire by means of an ebonite clip-holder.

A new and theoretically interesting molecular voltaic cell may thus be made, in which the two elements consist of the *same metal.* Molecular disturbance is in this case the source of energy. A cell once made may be kept in working order for a considerable time by pouring in a little vaseline to prevent evaporation of the liquid.

I shall now proceed to describe in detail the response curves obtained with metals, and as a substance which gives good results I shall take tin. The records given in this paper were obtained, some by following the galvanometer deflection with a pencil, others by direct photography, and have been exactly reproduced. The gal-

vanometer used was similar, as regards sensitiveness and the period of swing of the needle, to that employed for physiological records.

Effects of Single Stimuli.

Fig. 10, *a*, gives a series, each of which is the response curve for a single stimulus of uniform intensity (amplitude of vibration, 180°). Observe the perfect similarity of all these curves, and their resemblance to the curves of response in living tissues (Fig. 10, *b*). The rising portion of the curve is somewhat steep, and the recovery convex to the abscissa, the fall being relatively rapid in its first, and less rapid in its latter, parts. As the electric variation is the concomitant effect of molecular disturbance—a temporary upset of molecular equilibrium,—on the cessation of the external stimulus the excitatory state and its expression in electric variation disappear, with the gradual return of the molecules to their condition of equilibrium, a process which is seen clearly in the curve of recovery.

Different metals exhibit different periods of recovery, and this

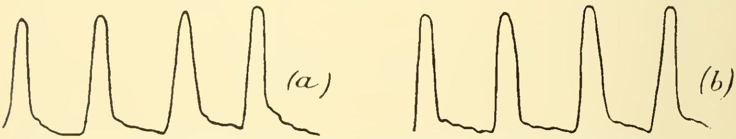

Fig. 10.—(*a*) Series of electric responses to successive mechanical stimuli at intervals of half a minute, in tin. (*b*) Mechanical responses in muscle.

again is modified by any influence which affects the molecular condition.

That the excitatory state persists for a time even on the cessation of stimulus can be independently shown by keeping the galvanometer circuit open during the application of stimulus, and completing it at various short intervals after the cessation, when a persisting electrical effect, diminishing rapidly with time, will be apparent.*

We have already seen how similar the response-curves of the in-

* Observe how similar the above is to the excitatory electrical effect due to stimulus, in living tissue, such as nerve. "The excitatory state evoked by stimulus manifests itself in nerve fibre by electromotive changes, and as far as our knowledge goes, by these only. . . . The conception of such an excitable living tissue as nerve implies that of a molecular state which is in stable equilibrium. This equilibrium can be readily upset by an external agency, the stimulus, but the term 'stable' expresses the fact that a change in any direction must be succeeded by one of opposite character, this being the return of living structure to its previous state. Thus the electric manifestation of the excitatory state is one whose duration depends upon the time during which the external agent is able to upset and retain in a new poise the living equilibrium, and if this is extremely brief, then the recoil of the tissue causes such manifestation to be itself of very short duration."—'Text-book of Physiology,' edited by Schäfer, p. 453.

organic are to those of the living substance. We have yet to see whether the similarity extends to this point only, or goes still further. Are the response-curves of the inorganic modified by the influence of external agencies, as the living responses were found to be? If so, are the modifications similar? I shall now place two sets of curves side by side, when it will become apparent whether or no similar external influences produce similar results in the two classes of phenomena.

Effect of Superposition of Stimuli.

It has been said that, with rapidly succeeding stimuli, when the intermittent effects of single shocks are fused, a tetanic condition is produced in a muscle, and we obtain an almost unbroken curve (see *b*′, Fig. 11). If the frequency is not sufficiently great, there is an incomplete tetanus, and the response-curve becomes jagged (see *a*′, Fig. 11).

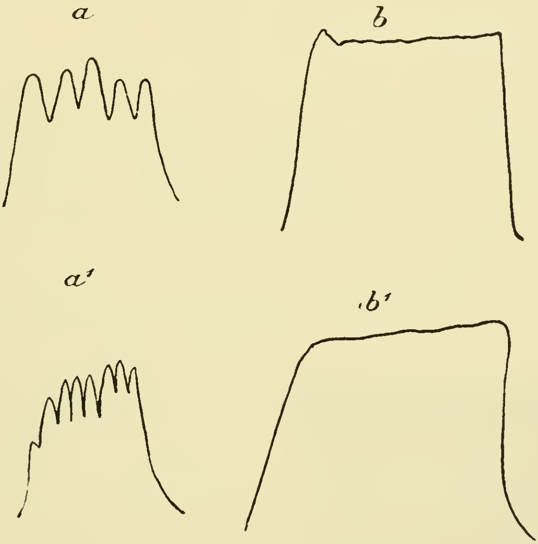

FIG. 11.—Effects analogous to (*a*) incomplete and (*b*) complete tetanus, in tin. (*a*[1]) Incomplete and (*b*[1]) complete tetanus in muscle.

The very same thing occurs in metals. I subject the wire to quickly succeeding vibrations. The curve rises to its maximum; further stimulation adds nothing to the effect, and the deflection is held, as it were, rigid, so long as the vibration is kept up. With lesser frequency of stimulus, we find an incomplete state of tetanus, and the curve becomes jagged (see *a* and *b*, Fig. 11).

It is also curious that the maximum effect is produced almost

invariably after a definite period of stimulation. In tin, at least, successive tetanic curves are almost exactly similar. The maximum effect depends on the intensity of the stimulus (Fig. 12).

Fatigue. Amongst living substances we find nerve practically indefatigable. Successive curves are exactly similar. But with muscle there is a rapid decline in the responses (Fig. 4). Fatigue, however, disappears after a period of rest. It is generally supposed to be due to the working of two processes conjointly —the breakdown of force-producing material and the accumulation of "fatigue-stuffs." It is thought that fatigue is removed by the action of the circulating blood in bringing in fresh material and carrying away fatigue products. But that this cannot furnish a complete explanation of the phenomena is shown by the fact that excised bloodless muscle acted on by stimulus, recovers from fatigue after a short period of rest, though here there is no blood-supply to repair the damage and remove the waste products.

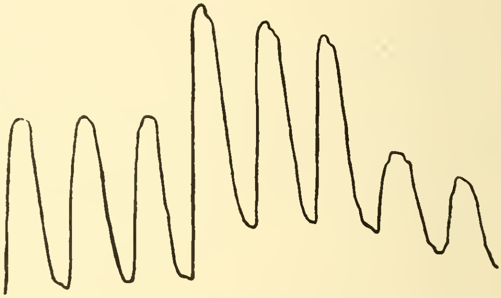

Fig. 12.—Tetanic curves in tin, showing effects of different intensities of stimuli. The three curves to the left show effect of vibration with amplitude of 90°; the next three are due to stronger intensity of stimulus, amplitude = 180°; the amplitude was now reduced to 90°, and the last two, owing to fatigue, show feebler response than the first three.

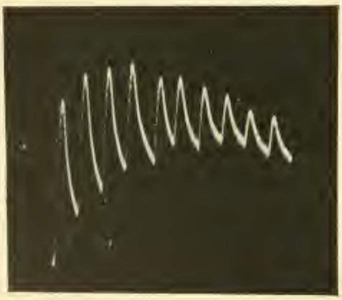

Fig. 13.—Photographic record of fatigue in tin (compare with Fig. 4).

Turning to inorganic substances, we find different metals exhibiting fatigue. But tin exhibits very little, reminding us in this of the behaviour of nerves. Even here, however, after prolonged action, fatigue is sometimes observed. The fatigue curve here reproduced was obtained from tin that had been acted on for several days, and its remarkable similarity to the curve of fatigue in muscles will be at once apparent (see Figs. 13 and 4). That fatigue is primarily due to over-strain, and not to fatigue products, is seen from the fact that a brief period of repose hastens its removal in this case also.

Stimulus of Light. Perhaps before completing what I have to say on the modification of response by external agencies, it will be well to make some reference to the action of other forms of stimulus and other modes of detection of response. In this investigation I have used the mechanical form of stimulus as being the simplest and giving rise to the fewest complications. Time does not allow of my entering here upon the question of the action of electric stimulus in causing response. I have dealt with this subject in some detail elsewhere. I may, however, say a few words on the effect of light stimulus.

If one of the sensitised wires in the cell already described be subjected to light it will give an electric response, and under certain circumstances an oscillatory after-effect will be seen on the cessation of light. This latter fact may, perhaps, explain certain phenomena of visual recurrence to be noticed presently.

Artificial Retina. The molecular strain produced by stimulus can not only be detected by the phenomena of electromotive variation, but also by conductivity variation.* Acting on this principle, I have been able to construct an artificial retina. The sensitive receiver is contained inside a hollow spherical case, provided with a circular opening in front, in which a glass lens is placed, corresponding to the crystalline lens. You now see before you a complete model of an artificial eye.† When this is interposed in an electric circuit, with a sensitive galvanometer as indicator, you observe the response to a flash of light by the galvanometer deflection. I throw red, yellow, green and violet lights upon it in succession, and you see how it responds to all. Note how strong is the action of yellow light, the response to violet being relatively feeble. Indeed, the most striking peculiarity of this eye is that it can see lights not only some way beyond the violet, but also in regions far below the infra-red, in the invisible regions of electric radiation. It is in fact a *Tejometer* (Sanskrit *tej* = radiation), or universal radiometer.

Observe how each flash of invisible light I am producing with this electric radiation apparatus, calls forth an immediate response, and how the eye automatically recovers without external aid. This will show the possibility of an automatic receiver which will record Hertzian wave-messages without the intervention of the crude tapping device.

This retina has, as will be seen with regard to spectral vision, an enormous range, extending far beyond the visual limits. We can, however, reduce its powers to a merely human level by furnishing it

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* See 'On the Similarity of Effect of Electrical Stimulus on Inorganic and Living Substances.'—*Electrician*, Sept. 1900.

† I hope to publish a complete account of this instrument at a future date. The descriptions which follow are more detailed than time permitted on the 10th of May.

with a water lens, which, in its liquid constitution, approximates closer to the lens of the eye than does the glass substitute. In this case the, to us invisible, radiations are absorbed by the liquid, and do not reach the sensitive retina. Perhaps we do not sufficiently appreciate, especially in these days of space-signalling by Hertzian waves, the importance of that protective contrivance which veils our sense against insufferable radiance.

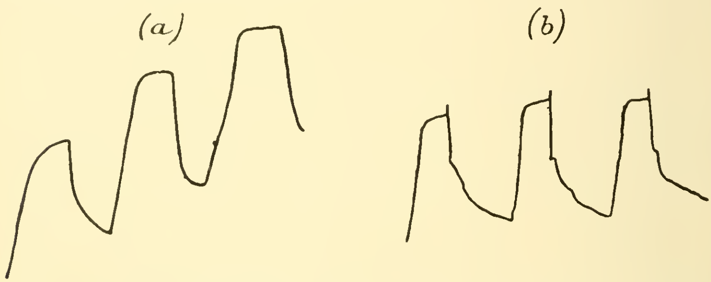


FIG. 14.—(*a*) Response curves of artificial retina for short periods of illumination followed by darkness. The ascending portions show the growing effect of light, the horizontal portion the balancing tetanic effect. The descending portion of the curve exhibits recovery during the absence of light. (*b*) Same in frog's retina (Waller).

I give here two sets of curves, one exhibiting the response of the artificial retina and the other of that of a frog, to show the general resemblance of the two (Fig. 14).

*Binocular Alternation of Vision.**

I have referred to the fact that sometimes on the cessation of light, an after-oscillation is observed, which may correspond to the after-oscillations of the retina, and give a probable explanation of the phenomena of recurrent vision. When we have looked at a bright object for some time with one eye, we find, on closing both eyes, that the image of this object alternately appears and disappears. It was while studying this subject that I came upon the curious fact that the two eyes do not see equally well at a given instant, but take up, as it were, the work of seeing, and then (relatively speaking) resting, alternately. There is thus a relative retardation of half a period as regards maximum sensation in the two retinas. This may be seen, by means of a stereoscope, carrying, instead of stereo-photographs, incised plates through which we look at light. The design consists of two slanting cuts at a suitable distance from each other. One cut, R, slants to the right, and the other, L, to the left (see Fig. 15). When the design is looked at through the stereoscope, the right eye will see, say R, and the left L; the two images

* See *Electrician*, Sept. 1900.

will appear superimposed, and we see an inclined cross. When the stereoscope is turned towards the sky, and the cross looked at steadily for some time, it will be found, owing to the alternation already referred to, that while one arm of the cross begins to be dim, the other becomes bright, and *vice versa.* The alternate fluctuations become far more conspicuous when the eyes are closed; the pure oscillatory after-effects of the strained sensitive molecules are then obtained in a most vivid manner. After looking through the stereoscope for ten seconds or more, the eyes are closed. The first effect observed is one of darkness, due to the rebound. Then *one* luminous arm of the cross first projects aslant the dark field, and then slowly disappears; after which the second (perceived by the other eye) shoots out suddenly in a direction athwart the first. This alternation proceeds for a long time, and produces the curious effect of two luminous blades crossing and re-crossing each other. Another method of bringing out the same facts in a still more striking manner, is to look at two different sets of writing, with the two eyes. The resultant effect is a blurr, due to superposition, and the inscription cannot be read with the eyes open. But on closing them, the composite image is analysed into its component parts, and thus we are enabled to read better with eyes shut than open!

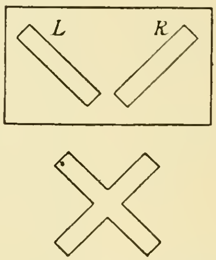


FIG. 15.—Stereoscopic design to show binocular alternation of vision.

You will thus see how, from observing the peculiarities of an artificial organ, we are led to discover unsuspected peculiarities in our own. We stand here on the threshold of a very extended inquiry, of which I can only say that as it has been possible to construct an artificial retina, so I believe it may not be impossible to imitate also other organs of sense.

Effects of Chemical Reagents.

We now return to the consideration of mechanical stimulus and the modification of its responses, as shown in our cell. We have seen the remarkable parallelism between organic and inorganic response under various conditions. There still remains the study of the effects of chemical reagents. For drugs profoundly modify the response of living substances. With respect to this function, they fall into three classes, some acting as stimulants, others as depressors, and yet others again as poisons, by which response is altogether killed. Amongst the last may be mentioned mercuric chloride, strong solutions of acids, and alkalies like potash. Again, drugs which in large doses become poisons, may, when applied in small quantities, act as stimulants.

It may be thought that to these phenomena, inorganic matter

could offer no parallel. For they involve possibilities which we have regarded as exclusively physiological. Accustomed in animal bodies to see the responsive pass into the irresponsive state at the moment of death, we look on this sequence as peculiar to the world of the living. And on this fact is based the supreme test by which physical and physiological phenomena are differentiated. That only can be called living which is capable of dying, we say, and death can be accelerated by the administration of poison. The sign of life as given by the electric pulses then wanes, till it ceases altogether. Molecular immobility—the rigor of death—supervenes, and that which was living is no longer alive.

Is it credible that we might, in like manner, kill inorganic response by the administration of poison? Could we by this means induce a condition of immobility in metals, so that, under its influence, their electric pulsations should wane and die out altogether?

Before we attempt the action of poisons let us study the exciting effect of stimulants. You observe in the galvanometer scale the normal extent of response under successive uniform stimuli applied to one wire of the cell. I now add a few drops of sodium carbonate to the water in the cell and you observe the growing exaltation of the response. There are other stimulants besides this which would induce a still higher increase of sensibility, even to an astonishing degree* (Fig. 16).

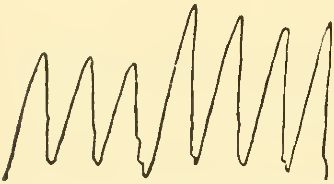


Fig. 16.—Curves showing stimulating action of Na_2CO_3. The three curves to the left show normal response; the four to the right increased response after addition of Na_2CO_3.

I now pass on to the effect of poisons. Any of the substances already enumerated may be used as the toxic agent. I take a fresh cell, and first demonstrate before you its normal electrical pulsation. By means of a pipette I now inject into the cell the toxic dose. Its effect is at once evident to you. After a few preliminary flutterings the electric pulses cease to beat, and all our efforts, by intense stimulation to reawaken them, fail (Fig. 17).

But we may, sometimes at least, by the timely application of a suitable antidote, revive the dying response, as I do now, by an appropriate injection. See how the lethargy of immobility passes away; the pulse-throb grows stronger and stronger, and the response in our piece of metal becomes normal once more (Fig. 19).

There remains the very curious phenomenon, known not only to

* The external resistance, as has been said previously, is kept very large, and the slight variation of the internal resistance of the cell has no effect on the deflection. The increased response can also be shown by capillary electrometer. Note also the disappearance of response by the influence of large dose.

students of physiological response but also in medical practice, that of the opposite effects produced by the same drug when given in large or in small doses. Here too we have the same phenomena reproduced

Before After

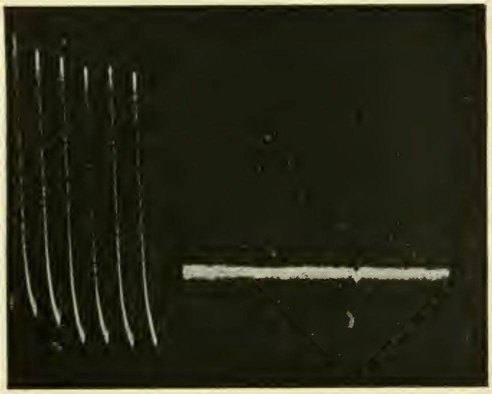


FIG. 17.—Photographic record, showing the killing action of strong dose of KHO (1 per cent.) on tin. The electric response is abolished after the application of potash. Compare the effect of KHO on nerve in Fig. 18.

Before After

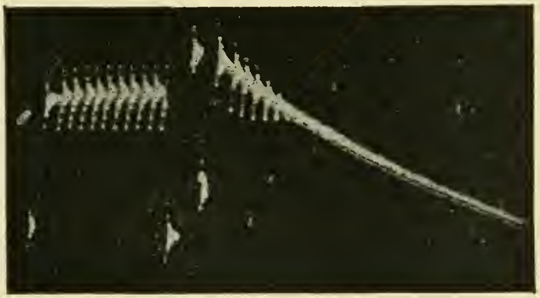


FIG. 18.—Killing action of KHO on electric response of nerve (Waller).

in an extraordinary manner in inorganic response (Fig. 20). The same reagent which becomes a poison in large quantities may act as a stimulant when applied in small doses.

I have shown you this evening autographic records of the history of stress and strain in the living and non-living. How similar are the writings! So similar indeed that you cannot tell one from the other apart. We have watched the responsive pulses wax and wane

Fig. 19.—(*a*) Normal response; (*b*) effect of poison; (*c*) revival by antidote.

in the one as in the other. We have seen response sinking under fatigue, becoming exalted under stimulants, and being killed by poisons, in the non-living as in the living.

Amongst such phenomena, how can we draw a line of demarcation, and say, "here the physical process ends, and there the physiological begins"? No such barriers exist.

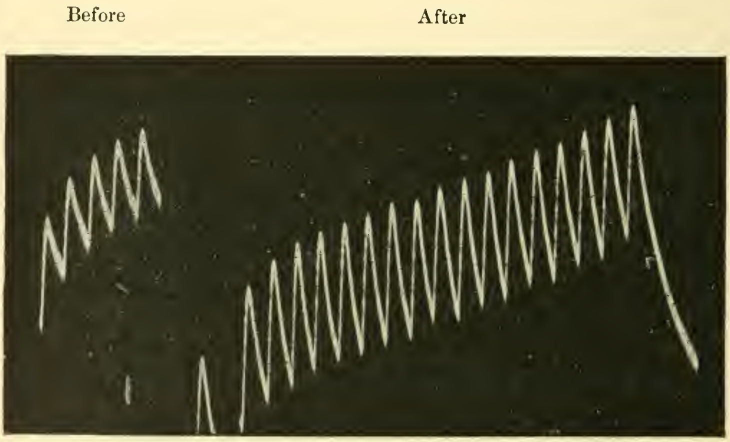


Fig. 20.—Photographic record, showing stimulating effect of small dose of KHO (0·2 per cent.). Compare with Fig. 17.

Do not the two sets of records tell us of some property of matter common and persistent? Do they not show us that the responsive processes, seen in life, have been foreshadowed in non-life?—that the physiological is, after all, but an expression of the physical?—

that there is no abrupt break, but one uniform and continuous march of law?

If it be so, we shall but turn with renewed courage to the investigation of mysteries which have long eluded us. For every step of science has been made by the inclusion of what seemed contradictory or capricious in a new and harmonious simplicity. Her advances have been always towards a clearer perception of underlying unity in apparent diversity.

It was when I came upon the mute witness of these self-made records, and perceived in them one phase of a pervading unity that bears within it all things—the mote that quivers in ripples of light, the teeming life upon our earth, and the radiant suns that shine above us—it was then that I understood for the first time a little of that message proclaimed by my ancestors on the banks of the Ganges thirty centuries ago—

"They who see but one, in all the changing manifoldness of this universe, unto them belongs Eternal Truth—unto none else, unto none else!"

WEEKLY EVENING MEETING,

Friday, May 17, 1901.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

The Right Hon. Earl Percy, M.P.

Turkish Kurdistan.

It is, perhaps, as well that I should preface my remarks by guarding against a misconstruction, which might possibly arise from the title I have chosen for my address. There is, of course, no clearly defined area of Asiatic Turkey to which the name Kurdistan can be applied; and of the many scattered districts which the Turks designate by that word I propose to deal with only a comparatively small one on the present occasion.

Roughly speaking, the whole of the Western area, which we call Asia Minor, the Turks call Anadolu, or Anatolia; while the eastern districts between the Black Sea and the plains of the Tigris, and extending westwards as far as Diarbekr and Kharput, which we sometimes term "Armenia," the Turks, who refuse to recognise that expression at all, describe as "Kurdistan." The two names are precisely similar in one respect, that they simply denote the fact that in these regions the Kurds and Armenians, although not a majority of the population, are more numerous than in any of the other Asiatic provinces. And just as Armenians are found outside this area, particularly in the great commercial centres of the interior and along the shores of the Levant, so the Kurds are found scattered over all the mountainous uplands, from the Taurus and the Anti-Taurus in the west to the Dersim, south of Erzingian, and the Alpine ranges along the Persian frontier. Now in theory all these Kurds are Turkish subjects, and amenable to Turkish law; but in practice the government exercises little or no effective control except over those who have exchanged the nomadic for a settled pastoral life, and have consequently acquired the same status as the ordinary Turkish or Armenian peasant. The roving migratory Kurds still enjoy a large measure of independence under their respective tribal chiefs, and would never come within reach of the civil authority or the tax collector at all, were it not for the severity of the winter climate, which forces most of them every year to leave their hill "Yailas" and to seek pasture for their flocks in the warmer valleys and plains of the south. There are two districts of Asiatic Turkey where this independence survives in its most unlimited form: (1) the "Dersim,"

which is almost unknown to Europe, where the safety of the traveller is extremely precarious, and which, as I have not visited it, I cannot describe; and (2) the small tract of country known as "Hakkiari," which is bounded on the north by Lake Van, on the west by the Tigris, on the south by the Mosul plain, and on the east by the great snow ranges which form the dividing line between Turkey and Persia.

I have selected the latter district to-night for three reasons: I have traversed it twice myself in different directions and by little-known roads; it contains some of the most beautiful and striking mountain scenery I know anywhere; and it is the home, not only of the fiercest and most formidable Kurdish clans, but also of the only remaining race of Christians under the sway of the Sultan who have preserved almost intact their civil as well as their religious autonomy.

A few words are sufficient to give in brief outline the *historical* events which led to the establishment in this region of the various races which now inhabit it.

Of these the Kurds are, in one sense at all events, by far the oldest. An Aryan people, supposed by some to have been of the same family as the Medes, and to have come originally from the north-west provinces of Persia, bordering on the Caspian Sea, their name, as we all know, appears in Xenophon's History under the form of the Kardouchoi, who harassed the Ten Thousand in their attempt to force a passage through the upper gorges of the Tigris. But whatever the original stock may have been, no common type of physique, any more than of religion or language, unites the widely diffused tribes of the present day. Some are Shiahs, some Sunnis; some have adopted much the same language as the Turks, others a dialect scarcely distinguishable from Persian. The Rayat Kurds near Pergri, east of Lake Van, dye their short stubbly beards with red henna like the subjects of the Shah; the Zaza Kurds of the Dersim and Upper Euphrates, strong, broad-chested men of dark complexion and almost Grecian profile, grow theirs thick, black and shaggy: the Bekeranli, in the neighbourhood of Diarbekr, have the slim figures, small bullet heads, neat trimmed black mustachios, and straight lanky hair, curled at the tips, which distinguish the Bakhtiaris of Persia; while among the Oramar, on the very confines of that Empire, you find fair complexions, high narrow heads, blue or grey eyes, and the nearest approach I have ever seen in real life to the languid features of the Burne-Jones Knight. The Turks, as I have said, employ the word Kurd to convey a certain vague geographical conception, but it carries with it no idea of nationality, and, indeed, it is frequently used as a kind of term of contempt, much as our ancestors might have used the term Highlander to describe a cateran or cattle-lifter.

If the "Mountains of Ararat" mentioned in the cuneiform texts as the country to which Sharezer fled after the murder of his father

Sennacherib, correspond, as there is good reason to believe, to the mountains between the Upper Zab and the Tigris near Jezireh, it is not improbable that the same race occupied the country from a period anterior to the rise of the Assyrian Empire, and preserved their autonomy long after its fall. Xenophon tells us how the Persians, after the annihilation of the formidable force which they had despatched for the purpose of reducing the mountaineers to subjection, ended by reluctantly acquiescing in their independence; and in 1139, when the power of the Abbasside Caliphs was rapidly declining, the famous Atabeg or Viceroy of Mosul, Imadeddin Zengy I., the friend of Saladin's father, was compelled, before embarking on his campaign against the Crusaders of Diarbekr and Urfah, to first of all guard against a diversion in his rear by sending an expedition against the Kurds of Ashib, the modern fortress of Amadiyah. The protracted struggle between the Persians and the Ottoman Turks favoured the interests of the Kurds and lent them additional importance in the eyes of the rival combatants. At the beginning of the sixteenth century their principal habitat seems to have been, as it is now, the province of Diarbekr; and Selim I., after getting rid of the most troublesome element among them by the simple expedient of a wholesale massacre of the Shiah population, persuaded a number of the remaining tribes to transfer their quarters to the Erzerum frontier, to act as a buffer against invasion from the north. No doubt this deliberate policy on the part of the earlier Sultans of encouraging emigration contributed materially to the intermixture of the various races and the obliteration of their original characteristics.

Meanwhile the Kurds east of the Tigris consolidated their power undisturbed, and when the nineteenth century opened, the greater part of the country north and east of Mosul was parcelled out between six or seven powerful hereditary chieftains, whose authority was perforce recognised, however unwillingly, by the Turkish Government. Thus the district of Rowanduz, on the frontier, was governed by one Mehemet Pasha, a descendant of the Abbassides, who, in 1834, enlisted a numerous army of Kurds, under the standard of his brother Resul, seized Amadiyah, Kirkuk and Mosul, and was with difficulty defeated by the Sultan's general, Reshed Pasha, who marched his army of 20,000 across the whole length of the continent from Sivas through Kharput and Diarbekr. Suleimaniyeh again was under the rule of another hereditary chief, Ahmed Pasha, who made a similar attempt to extend his authority at the expense of the Turks in 1843. Bahdinan, lying south of the Supna in the fork between the Zab and the Tigris, formed a third principality under Ismail Beg; and Berwari, a little to the north, a fourth province under Abdul Samed Beg: the raids and counter-raids between this tribe and the Nestorians of Tiyari being a prime cause of the bad feeling which led to the famous massacres four years later. Lastly, Bohtan, the district watered by the Bohtan river, the chief affluent of the Tigris, was under the jurisdiction of the infamous Bedr Khan Beg, the prin-

cipal author of those outrages; while Hakkiari, which included Julamerik until the latter was separated in 1841 by the Turkish Government and assigned to a sixth chief, Suleiman Beg, was ruled by the equally notorious Nurulla Beg from his headquarters at Bashkala. It was not until the year 1847, when the chiefs of Hakkiari and Bohtan contrived to massacre, between them, close on 4000 of the Christians of Tiara and Tkhoma, that the protests of the Powers compelled the Turkish Government to recognise the necessity of adopting vigorous measures to put an end to a state of things which, in the course of fifteen years, had led to two serious insurrections against their own authority, and, by the extermination of all the leaders of the most powerful Christian clans, threatened to withdraw the sole remaining counterpoise to the aggressive power of the Kurdish confederacy. Bedr Khan Beg was exiled to Candia, and his overthrow was followed by the gradual suppression of almost all the old hereditary chiefs. The few that survive to-day have either sunk to the position of small private landowners, despised but humoured by the Turks because of the affection and almost superstitious reverence with which their dependants still regard them; or else their authority is confined within the immediate limits of their own clans, instead of extending, as I shall presently show it once did, over the main section of the Christian community.

Passing from the Kurds, the next immigrants, if local traditions be accepted, were the JEWS, or, rather, the Israelites, whom the Assyrian king transplanted after the fall of Samaria and settled in the districts east of Samaria. Perhaps, like the Kurd militia of Selim I., they were intended to act as a check upon their unruly neighbours, for in the seventeenth chapter of the Second Book of Kings it is expressly stated that the Samaritans were placed in "Halah, Habor, on the River Gozan, and *in the cities of the Medes*"; and in this connection it is curious to notice the expression "the Hebrew fortress" used by St. Thomas of Marga (ninth century), probably to describe the Jewish colony of Nebi Yunus, near Mosul. Jews are, of course, found in considerable numbers in many of the larger centres of population throughout Asiatic Turkey, but it is remarkable that along the frontier we find them grouped in remote country villages and occupied in those agricultural or pastoral pursuits for which the majority of their fellow-countrymen elsewhere appear to have lost all taste or inclination. There is a large colony at Akkra; other settlements occur at Amadiyah, and in the district of Berwari in the west; Girdi and several villages between Neri and Rowanduz to the east are almost entirely Jewish, and a few are found even as far north as Bashkala. They wear the same dress and speak the same language as the Kurds (though Dr. Badger met some in the Supna who used the Aramaic of the Chaldeans), and the markedly Hebraic features of many of the Kurds themselves in this region attest the large amount of intermarriage which still goes on between the two races.

Last, but by no means the least important in point of numbers, come the *Christian population* whom we in Europe generally call Nestorians, but who describe themselves as the Syrians of the East. It is impossible to make any confident assertion about their origin, but in the absence of any evidence to the contrary, there is no reason why we should reject their own traditional claim to Babylonian and Assyrian descent. The Babylonians are believed to have been of shorter stature and darker colour than the Assyrians, and there is a similarly marked distinction between the physical characteristics of the Nestorian tribes at the present day.

The men of Tiyari who inhabit the main valley or gorge of the Great Zab from Julamerk to Lizan, and those of Tkhoma, who occupy the valley branching off to the east of Lizan, are for the most part short, thick-set men with light olive complexions, eagle noses, dark eyes, flowing beards, and coal-black hair, which they plait in a multitude of long thin pigtails, and let fall over their shoulders and bare chests from under tall, conical hats of white or black felt. The inhabitants of the central valleys of Baz and Jelu, on the other hand, are of taller, thinner build, their complexions are almost as fair as those of Europeans, they often have blue eyes and short red hair, and their head-dress is the low, rounded cap of the Kurd swathed in dirty strips of red or black linen. Otherwise the dress is the same in all cases and for all weathers. The feet are either bare or cased in thick woollen socks and felt sandals, which are indispensable for keeping a footing on the slippery mountain paths. A short jacket is worn over the loose open shirt, which is tucked into pyjamas, and confined at the waist by a broad sash containing a churchwarden pipe or "kaluna" of rosewood, and an ebony-handled dagger cased in a sheath of wrought silver. In Jelu most of the men are well armed, besides, with modern rifles which they smuggle in from Persia; whereas in Tiyari and Tkhoma they rarely have anything better than an old rusty flint-lock.

Although it is probable that the Christian population of Hakkiari received large reinforcements of their numbers at a later date, and more especially during the terrible invasion of Tamerlane in the fourteenth century, the original nucleus of immigrants were an offshoot from the primitive monastic settlements of Mesopotamia, founded, on the model of those of St. Anthony in Egypt, by Mar Awgin, a native of the island of Clysma, near Suez. Taking up his abode in the mountains near Nisibis, he found a zealous friend and supporter in St. James, the famous bishop of that See, who proceeded to found a monastery on Mount Kardo, the modern Jebel al Gudi near Jezireh, where, with the assistance of an angel, he had discovered a wooden plank; thus verifying the traditional claim of the mountain against the rival pretensions of Ararat and Sippan to have been the first resting place of the Ark of Noah. Other monasteries like that of Mar Mattai on Jebel Maklub, near Mosul, sprang up soon after, and Mar Awgin is said to have obtained a pledge for their protection

from Jovian, the lieutenant of Julian the Apostate. Many of their inhabitants, however, fell victims to the fierce persecution of Sapor in 330, and when, 33 years later, the Persian king advanced to the occupation of Nisibis, it was only through the display of miraculous powers by Mar Awgin that he was induced to renounce his hostility, and even to grant sites for the erection of churches and monasteries. This permission was immediately followed by the despatch of a band of 72 missionaries, and during the next 14 years the monasteries provided harbours of refuge from the renewed persecutions of Valens. It is no doubt owing to the fact that the valleys were only peopled by degrees from these hill stations, that the modern Nestorians are still known by such significant names as the Sons of the Cave or the Sons of the High Place (Rumta).

By the close of the fifth century the infant church had extended its sway over the greater part of Armenia and Persia, and by the close of the eighth, Central Asia, India, and China were reckoned among the number of its chief Metropolitan Sees. Many of the wealthy Persian nobles had forsaken the tenets of Magianism and become liberal patrons of the monasteries. Chosru II. (590–628) himself built a convent in honour of his Christian wife Shirin, and his active intervention in the affairs of the rapidly growing community is shown by the dispute which arose in connection with his nomination of Gregory of Nisibis in opposition to Gregory of Seleucia for the office of Catholicos. Two years after his assassination, his daughter Boran, the reigning queen, sent an embassy of Nestorian bishops to Heraclius for the purpose of promoting friendly relations between the two empires, and it was not until the middle of the eighth century that the fortunes of the Church began to decline with the decay of the Persian and the rise of the Arab power.

At first the Mahommetans appear to have treated the Christians in a friendly manner, and when their attitude changed, the change, as at the present day, was due not to religious antagonism but to jealousy of their superior wealth. The Nestorians themselves always date the erection of their churches in reference to Mahomet, and the earliest writers bear out the common tradition which assigns many of them to a period anterior to his time. Dr. Badger mentions the legend that the monastery church of Mar Audishu near Amadiyah was built 366 years before the date of the Prophet, and like another church of the same name in the valley of Tal, it is still regarded with great veneration and visited as a place of pilgrimage even by the Kurds and Persians. In Jelu, one of the most treasured relics of the great church of Mar Zaia is a handkerchief covered with Arabic writing which the natives believe to be a firman of the Prophet himself according sanction and protection to their worship; and when the Turkish troops occupied the village at the time of my visit in 1899, this was the only article which they apparently thought it worth their while to carry away. A similar tradition asserts that the substance of the firman given by the Porte to each successive occupant

of the Patriarchal See, confirming his spiritual authority over the Nestorians of the Empire, was originally accorded by Mahomet to Ishy Yau, the then Patriarch of the East, residing at Bagdad; and Assemani, in his Bibliotheca Orientalis compiled for Clement XI., gives the Latin text of Bar Hebræus' account of this extraordinary transaction. The treaty, he says, was negotiated by the help of large presents through the agency of Said the Christian Prince of Najran (Nagranensium). By its terms Mahomet gave the Christians a "diploma" commending them to the protection of the Arabs, safeguarding their religion and laws, and exempting them from military service. If they entered a Moslem household they were to be shielded from insults to their faith, they were to be allowed to erect churches as they pleased, and the Arabs were even enjoined to assist them in the work; and finally the amount which might be raised in taxes from the rich and poor was laid down with strict and minute precision. The Caliphs of Bagdad seem to have observed an attitude of general tolerance to all faiths, and Assemani mentions a decision of Caliph Mamun in the case of a dispute between the Jews of Tiberias and Babylon, by which he laid the rule that any ten Christians, Jews or Magians might meet and select whomever they pleased to preside over their respective communities. This negative attitude, however, was gradually abandoned, and the Arabs not only extorted money from their protégés but ceased even to protect them from the Kurds. In 747 the Governor of Mosul levied a tribute of 375*l.* from the monks of Beth Abhe on the Upper Zab, and some 60 years later the monastery itself was plundered by the Kurds of Kartaw. In 1370 another roving band sacked the monastery of Mar Mattai, and it is at once a remarkable proof of the vitality of the church, and of the yielding character of Mahommetanism in those days, that in spite of all these hindrances the process of proselytism went on and prospered, many of the Turks themselves becoming converts during the eleventh century, through the efforts of Ebedjesus the Metropolitan of Maru.

It is not too much to say that if the Nestorians exist as a distinct nationality to-day, it is because they exist as a separate church; and they certainly afford the most striking, if not the only, surviving example of a purely ecclesiastical system of civil government. At their head, supreme in all matters lay, as well as spiritual, and enforcing his decisions by the decree of excommunication, which operates as a kind of boycott, stands the Patriarch Mar Shimun (My Lord Simon) Father of Fathers, and Great Shepherd, Catholicos of the East and salaried representative of the Turkish Government. During the first four centuries after the conversion of the people by St. Thomas the Apostle, and Mar Addai, one of the Seventy, Mar Shimun's predecessors were not patriarchs at all, but simply metropolitans, occupying the See of Seleucia Ctesiphon on the Tigris and subject to the jurisdiction of the patriarchs of Antioch. Owing to the difficulties of the journey, and perhaps also to the dangers of riot among the non-Christians of Antioch, fomented for their own purposes by

the Persian Kings, the Syrian Metropolitan was soon dispensed from the obligation of coming to the Orontes to be consecrated, although it was not till 431, when Nestorius was excommunicated by the Council of Ephesus, and the See of Seleucia refused to recognise the validity of that decision, that the head of the Eastern Church assumed the full position and powers of an independent patriarch.

This secession was followed in the sixteenth century by an internal dispute about the succession to the patriarchate, which rent the Church into two sections: (1) those who continued to recognise the authority of, and to pay tribute, to Mar Shimun, and who to-day number in Persia and Turkey about 50,000; and (2) those who recognise the authority of Mar Elia, the patriarch of Babylon, and reside principally in the vicinity of Mosul. Very soon after the schism this section began to make overtures for recognition by the Church of Rome, but owing to their disinclination, at first, to sacrifice their ecclesiastical autonomy, their advances were coldly received by Paul IV., when they applied to him in 1607. Innocent XI., however, at the close of the century, took the important step of appointing a successor to the vacant See of Diarbekr, and the Nestorians of the plain, having now formally entered into communion with the See of St. Peter, are known by the distinctive title of the Uniat Chaldean Church.

Originally the appointment of the Catholicos was vested in the Bishop and Metropolitans of the Church, and it was not till 1450 that their choice was limited by a rule laid down by the then occupant of the patriarchial chair, that in future his successors must be selected from among his nearest relatives. This change appears to have been followed by the assertion on the part of the laity of their right to make the appointment themselves, and until comparatively recently the election was controlled by the two rival clans of Tiyari and Tkhoma. This practice naturally led to incessant feuds and heart-burnings, and the Catholicos, by general consent, now designates his own successor during his lifetime.

The only conditions are that he must belong to the patriarch's family, and that he must be a Nazarite: that is to say, he must be unmarried (a condition imposed in the fourteenth century), he must never have tasted wine or meat, and his mother during her pregnancy must have observed the same regulations. These rules apply also to the bishops and the metropolitans, and thus the highest ecclesiastical dignitaries form a kind of hereditary aristocracy deriving their salaries from their own small properties and from the tithes and offerings of the people. Under such circumstances it is not surprising that these offices are occasionally filled by quite young men, and the achievement of Cardinal Richelieu, who became bishop at the age of 23, is thrown into the shade by that of the present patriarch designate (Benjamin) and the bishop of Jelu (Zaia Mar Sergis), who are both children of 14. The priesthood, though in practice often confined to the same families, and more numerous than its present duties would appear to require, is still elected by the

villagers, and, having no source of income beyond the small fees paid for marriages and burials, its members devote themselves, like the rest of the population, to manual labour. Both priests and deacons have been allowed to marry since the close of the fifth century, and are usually called to the ministry at the age of 17 or 18. In conducting the service the priest reads the collects while the deacon reads the litany, and neither the Sacrament of the Eucharist nor of Baptism can be administered without the co-operation of the two orders, although if a deacon cannot be found, a second priest may, in case of necessity, act in a diaconal capacity.

Besides the clergy each village has its own headman or Malek, appointed by the Catholicos, and they collect the annual tribute, which is paid by him to the Turkish Government. The privilege of paying tribute, instead of the ordinary taxes, is enjoyed by all the mountain tribes or Ashirets, and distinguishes them from the Rayats of the plain, who stand in the same position as the Turkish or Armenian peasant, pay the sheep tax and the military exemption tax, and are much more exposed to official rapacity and Kurdish raids. The hill tribesman rarely comes in contact with any government official. When he does so it is because the tribute has been withheld, or because intervention is necessary to punish some glaring outrage, or to settle disputes between the Kurds and Christians, who are perpetually raiding and massacring one another. There is not very much to choose between the two, and when quarrels arise between the various Christian Ashirets they have little scruple in calling in their Mussulman neighbours to help them.

Up to the middle of last century, as I have said, Hakkiari, like the rest of the country, was governed by great Kurdish chiefs who protected their own clients and robbed everyone else, so that it was to the interest of the Christian tribes (who were not then recognised as a corporate body by the Turkish Government) to range themselves in what were called Bazikki or "wings," under the patronage of one or other of these over-lords. Generally speaking, the Christians of the main valley of the Zab formed one league with the Kurds of Artousha to the west of the river, while those on the east allied themselves with the neighbouring tribes of Oramar and Apenshai.

There appears to have been a curious parallelism at this epoch between the tribal organisation of the Moslems and Christians; for the predecessors of Nurulla Beg, the Kurdish Mira of Hakkiari, although hereditary chieftains in the sense that they belonged to one family, depended for election to their office on the consent of their own clansmen. They had no prejudice against the Christians as such, and Dr. Badger believed that the dislike which culminated in 1830 in the murder of the German traveller, Schulz, was inspired by fear lest the intrusion of the foreigner might be merely a precursor of Turkish supremacy. Nurulla Beg himself allowed Dr. Grant, the American missionary, to build an establishment in Ashitha, and for centuries before his time the native Christians were not only tolerated and protected, but even admitted to considerable privileges in common

with their Mussulman allies. This may possibly be explained by the fact that when Malek Sambo, the Kurd, first obtained his hold over Hakkiari in the eighth century, he did so by the help of the Christians of the fortress of Diz. At all events they were exempted from tribute as well as from the haratch or poll-tax which was levied in the rayat districts, and on condition that they supplied an armed contingent for the purposes of common defence they were allowed to share in the councils of the tribe, and even to assist in the election of the chief (himself a nominal tributary of the Turkish Government). In cases where a dispute arose between a Kurd and a Christian, it was brought originally before a Court in which the Patriarch and the Mira sat together, and when later the two Courts were separated, the litigants were allowed to choose the tribunal they preferred. On the whole the system worked fairly well, but it was manifestly open to great abuses, and after the massacres in 1847, the abolition of the Mira's authority was followed by the introduction of a regular system of government. Hakkiari was constituted a separate province or vilayet, with a vali or governor at Bashkala and a subordinate official or kaimakam at Julamerk. As, however, both these dignitaries, as well as the members of their Councils, were Kurds (with the exception of two Christians on the Medjliss at Bashkala), and as the Kurds managed during the Russo-Turkish war to arm themselves with modern rifles, the new plan proved to have all the disadvantages and none of the advantages of the old one. Consequently, in 1888, Hakkiari was incorporated with the province of Van, where the English Consul now resides at the capital in constant communication with the Vali, while his subordinate at Julamerk is no longer a Kurd, although, like all minor officials in out-of-the-way places, he is generally meddlesome, obstinate and obstructive to travellers.

Having said thus much of the history and main characteristics of the inhabitants, I propose to deal briefly with the geographical features and scenery of the country, and the ordinary life of the people.

Kochanes, the seat of the Catholicos, is a tiny village situated on a green tableland, between six and seven thousand feet above the sea. To the west and south it is overhung by lofty peaks (on the summit of which M. Binder found traces of what he conceived to be a petrified forest), while on the remaining two sides it descends abruptly to the level of two small torrents which unite here on their way to join the Greater Zab. The Patriarch's house is built on the very verge of one of these precipices: a large stone structure with rooms comfortably furnished in the Persian style, and supplied with the luxuries, not often met with in these parts, of wooden floors and carpeted divans. Here he spends his time, partly in religious exercises which occupy most of his morning, and partly in deciding the quarrels of his subjects, and the difficulties which arise from time to time with the Turkish authorities at Van and Julamerk. His unmarried sister, Asiah, keeps house for him, and is rarely visible outside the kitchen; while his brother's children, Benjamin the Patriarch designate, and Surma, who surreptitiously took the veil

when she was scarcely out of her teens, are educated by a priest who devotes most of his life to copying old manuscripts with a reed pen and gall ink.

The only other edifice worthy of notice is the church of Mar Shalita, perched on a high rock on the verge of the opposite precipice, and surrounded by a clump of poplars. The first monastery churches appear to have been built of clay and brick, but those of the present day are of solid limestone, and the general plan of all of them is the same—a plain oblong nave divided off from the sanctuary by a curtain, and containing no furniture of any kind, except a couple of lecterns, a string of sheep bells stretched across the west end, and rung at intervals during the service, and, at Kochanes, a low, truncated pillar which serves as a stand for the censer. There are no longer any screens dividing the men from the women as in the earliest buildings, and the congregation either stand or squat in Oriental fashion on their heels, after leaving their shoes, as the Mussulmen do, at the entrance. Icons are forbidden, and the only form of decoration are the curtains and hangings on the walls. A tiny light is always kept burning before the altar, and another in the nave is lit at the commencement of the service. Otherwise the church is almost entirely dark, for little daylight penetrates through the small slit windows, and when the priest reads the gospels he has to do so by the aid of spiral tapers of beeswax. No one but the clergy may enter the sanctuary, and even they may not do so without having fasted beforehand. The vestry or baptistry usually forms an adjunct to it with a separate entrance, which at Zerani in Jelu is closed by quaintly carved wooden doors. The chief peculiarities of Mar Shalita are the small chamber in the north wall, where the Communion bread is prepared, and the minute entrance doorway, scarcely more than four feet high, which can only be reached from the outside by means of a ladder. This gives the building the appearance of a fort rather than of a church, and is due to the apprehension entertained by the Kochanes people that the Kurds after a successful raid might be tempted to stable their cattle or horses in the interior.

From Kochanes, after crossing a range of about 8000 feet, you reach the main valley of the Great Zab, the traditional Pison of Genesis, at Julamerk. Between this point and Lizan, a distance of about forty miles, lies the district of Tiyari, divided roughly into two equal portions (Upper and Lower Tiyari) by the Levin river, which flows in from the north-west at a place called Shinna, or the Precipice. For the greater part of its course through this tract the Zab flows between sheer walls of cliff, which in places come so close down to the water's edge that there is scarcely room for a mule to pass, and most of the villages are built high up on any grassy slope which affords sufficient space for cultivation. Artificial steps called "stangi" (the work, according to M. Binder, of Kurdish prisoners, but according to local tradition, of rival suitors for the hand of the chief's daughter) are laboriously cut in the rock to facilitate the passage of caravans; and isolated fields of millet, hemp and rice are banked up

with stones above the stream, and irrigated by tiny channels hewn out in the same way. There are few trees of any kind except terebinth and juniper, but the boulders and cliffs are often carpeted with vines and blackberry bushes, and their interstices filled with little brilliant patches of purple and yellow colchicum.

In Upper Tiyari the stream, though rapid, is generally fordable, but in Lower Tiyari it runs deep, even in autumn and early winter, and on one occasion near Lizan, a Turkish soldier in my escort was drowned while attempting to cross it. The ordinary bridge for foot passengers consists of nothing but a slippery poplar stem thrown across a chasm, but at Lizan a more ambitious structure has been attempted. Stone piers are built out on either bank, and a row of poplar trunks inserted in the masonry. These in their turn are covered with successive layers of poles, each projecting further across the stream, and finally the surface is covered with a loose basket-work of osier, and large flat stones are laid over it to conceal the gaps and crevices.

Leaving the main gorge of the Zab and turning eastward, we find four lateral valleys formed by small tributary streams. Diz, lying along the course of the Deezen Su, and Baz, along that of the Kun Su, provide alternative means of access to Jelu; Tal is a tiny ravine leading up to the high bleak volcanic range which separates Tiari from Tkhoma; and the Salabegan river provides a second and easier avenue of communication between Tkhoma and the Zab valley below Lizan.

In *Tkhoma*, although the scenery is tamer than that of Tiyari, there is far more cultivation, and the greater width of the valley allows of the villages being built on the low ground. The stream, however, is very liable to sudden rises from the melting snows, and when I visited the place many of the fields had been so overwhelmed with stones and débris as to be rendered totally unfit for cultivation. Owing to the greater intensity of the summer heat the houses in Tkhoma, unlike those of Tiyari, are provided with an upper story. The walls, built of rough blocks of stone, are raised to a height of about 8 feet, with a low narrow doorway, and roofed with poplar trunks. In the centre, or at the far end of the room, a hole is scooped out in the mud floor to hold the fire, which is fed with branches and twigs of dwarf oak, and the smoke, after circling about the blackened rafters, escapes through small loopholes in the walls, which also serve for purposes of defence against the Kurds. During the winter the inmates sleep on quilts and strips of felt near the fire, and in the poorer cabins a wooden railing serves to partition them off from the buffaloes, sheep and poultry, which are herded together on the further side. During the summer they remove for the night either to the upper story, which has an open front screened with vines and fig-trees, or else to the mud roof, which has been rolled smooth and water-tight by means of heavy stone cylinders. The main object being to get as far from the mosquitos as possible, a kind of tall scaffolding is raised, with a platform supported by four

poplar trunks, and sometimes you find these curious sleeping-places erected in the very centre of a dry torrent bed.

Like the Persians, the men of Tkhoma and Salabegan are excessively clever carvers, and make beautiful spoons, with elaborate chains attached to them, out of single pieces of wood. Another of their accomplishments, and one not usually associated with the male sex, is that of knitting socks; while a less useful habit, which reminds one of similar customs in India, is that of adorning themselves, and any guests to whom they wish to pay special honour, by sticking marigold flowers behind their ears.

Retracing our steps from Tkhoma to Julamerk, and following the Zab northward, we enter, after a few hours' ride along a deep cleft, the valley of the Deezen river, which forms the connecting link between the central mountain chain and the transverse range of the Jelu Alps, containing some of the loftiest summits in Hakkiari. The approach to this district is marked by a distinct change in the shape of the peaks, many of which bear a strong resemblance to the pointed sugar-loaf formation of the Austrian Dolomites, and also by the thick forest growth of oak, beech and hawthorn which clothes their lower slopes. Tobacco and maize are cultivated in considerable quantities, and one is struck with the uncommon size and girth of the planes, maples, sycamores and walnuts which border the edge of the stream. In the depth of winter the Jelu is probably not passable at all, and caravans would take the easier route through the valley of Baz. When I crossed the Douruk pass in October the snow lay deep at an altitude of 10,200 feet, and a little further to the west the peak of Galeashin rises to an elevation of 13,500 feet, and glaciers mark the limit of perpetual snow. The trend of the range is from north-east to south-west, and the sides are so steep that but for the natural ledges formed by the projecting strata it would be impossible to cross them at all. The villages are perched on small grassy promontories along the foot of the ravines, and in order to pass from one to the other it is necessary to toil painfully up, it may be, 2000 feet, while the muleteers cling on behind to the mules' tails, only to descend again almost immediately to the original level. In this way you can skirt the whole range from Zerani, which lies under the three great peaks, the Douruk, the Suppa Douruk and the Shinna Jelu, to Ishtazin, which commands the eastern passes into the fertile lake basin of the Diza or Gawar plain. This portion of the country is called Upper Jelu, while Lower Jelu stretches southward to the borders of the practically unknown territory of the Oramar and Apenshai Kurds.

The readiest avenue of communication between the two is by the gorge of the Ishtazin river, which, flowing parallel to the Greater Zab, joins it in the lower reaches a little above Rizan. So far as I know, the greater part of the district south of this river, the region of the Dustik Kurds, has never been explored by Europeans. I succeeded myself in reaching the village of Oramar, but was prevented from going any further by the absolute refusal of both the soldiers

and the muleteers to accompany me. From the Nestorian hamlet of Zir, which stands at a level of about 7000 feet, a small burn flows down through a beautiful wooded glen, gradually narrowing to a slit between precipitous walls scarcely more than twenty yards apart. After emerging from this defile you reach the banks of the Ishtazin river and cross to the opposite side, which can only be done at low water, as there is no bridge for the animals and the current is deep and strong. The level of the stream at this point is only 3800 feet, but the village of Oramar stands 2000 feet higher, and above it to the south-east rise two gigantic reddish-coloured cliffs with snow summits, called the Jaita (the Slippery) and the Kalabiri Oroch, which are probably higher than the Jelu peaks, although in making any comparison the difference of at least 4000 feet in the elevation of their respective bases must be taken into account. To the westward the view is obstructed by the chaotic mass of pinnacled crags, between which the river winds, and during the greater part of its course till it joins the Zab the character of the scenery is much the same as that which prevails in the northern reaches between Oramar and Ishtazin. The Kurds, however, told me that further south the hills decrease rapidly in size, and that very soon after passing the main ridge of Oramar the country presents much the same aspect of rolling, grassy downs, sprinkled with oak scrub, which marks the districts between Neri and Rowanduz.

The Kurds whom I met at Oramar were not striking in appearance. Most of them were tall, but thin and weakly looking, and their houses, compared with those of the Christians, gave one an impression of extreme squalor and poverty. In other parts of Asiatic Turkey it is very rare to find the nomad Kurds inhabiting stone villages at all. They are too lazy to build themselves, and being a pastoral, not an agricultural people, simply move their tents from one grazing ground to another without even attempting to cultivate the soil.

Oramar itself is obviously a Nestorian village from which the original owners have been expelled, and the church converted into a mosque. The Kurds occupy it during the summer, and sow a few acres to supply their immediate wants; but during the winter they migrate with their flocks and herds, like their neighbours, the Herki and the Apenshai, to the plains of the Lesser Zab, raiding the settled villages by the way. For this reason they have a bad name among the peaceful section of the inhabitants, but the stories of their cruelty and fanaticism have probably been grossly exaggerated. They rob, —as indeed do the Christians, with far less excuse—but except where resistance is offered, or in cases of blood feud, they do not generally indulge in personal violence or insult women. Their own women go unveiled, and in England would be considered decidedly forward in their manners; while the extent of their fanaticism may be judged from the fact that they seriously discussed the advisability of putting us up for the night in the mosque, and finally accommodated us with the best quarters to be had in the sheikh's own house. Personally, I

believe that a traveller need have little hesitation in traversing the whole district under their escort, provided that he had made careful inquiries beforehand as to the state of feeling at the time between the rival tribes, and satisfied himself that those to whom he entrusted his safety were numerically strong enough to ensure it.

Of course much depends on the character of the chief for the time being. In the district of Shemsdin, for instance, between Neri and Girdi, there are two great Kurdish chiefs, one of whom, Moussa Beg, is looked up to by all the surrounding Christians and their "Matran" Mar Khnanishu, as their special patron and protector; while the other, Sheikh Sadiq, is the biggest fanatic and scoundrel unhung, and has more than once invited guests to his house at Neri for the express purpose of murdering them. It so happens that Moussa Beg, although he has neither the quasi-religious prestige nor the wealth which the Sheikh derives from his monopoly of the famous Shemadin tobacco, is a far abler man, and with a small but highly efficient fighting force he is able to defend himself and his clients, besides inflicting severe reprisals on his enemies, by sallying out at intervals from the strong martello tower which he has built on the top of a steep isolated hill at the south edge of the Diza plain. The result is that the Sheikh finds it more profitable to turn his attention to lesser fry, and in the only case which I came across of the massacre and depopulation of an entire valley the victims were not Christians, but Bradost Kurds, who chanced to have embroiled themselves with the Herki, and so offered an easy prey to any third party—in this case Sheikh Sadiq—who chose to attack them.

Time forbids me to enter into more details to-night, but I trust that I have said enough to show that Hakkiari is a not uninteresting country. It presents a large field for the mountaineer, the botanist and the geologist; the mineral deposits include zinc, rock alum, lead, sulphur, copper, mercury, iron, coal and tin, and the sportsman will find a fair amount, if not a large variety of game. Wild boar and the Syrian bear abound in the woods of oak and terebinth, magnificent ibex heads can be obtained on the higher peaks of Jelu and Oramar, and in Tiyari the rare "giant" partridge is not infrequently met with in addition to the ordinary red-legged variety. The right season of the year to visit the country (for any one but an M.P.) is of course the late spring, when the soil, which in autumn yields little but thistles and the prickly gum-tragacanth, is in many places covered with iris, gentian, anemone and violets; and all but the higher crests are free from snow. The Nestorian Christians are always hospitable, and so far as my limited experience goes, the Kurds will treat you very well if they have not already waylaid and robbed you before you arrive at their villages. As for the Turks, they have little real authority in the interior, and mainly for that reason they try to prevent you from going anywhere where you are likely to be attacked and so involve them in trouble. Their policy is very like that of the Indian government, which does everything in its power to debar travellers from entering the trans-frontier districts; and when people blame the

Turks for allowing lawlessness and crime to go unpunished and unchecked in those remote confines of the Empire, it must in fairness be remembered that not until comparatively recently have we ourselves adopted a forward policy in our own sphere, and abandoned the attempt to check tribal license by means of intermittent punitive expeditions. The problem in Asiatic Turkey is in fact more complicated than in India, owing to the existence side by side of rival Christian and Mussulman populations. The Turks show a laudable freedom from bigotry in subsidising Mar Shimun, much as we subsidise the Afridis, and if he fails to keep the peace between his own subjects and the Kurds they are inclined to say, " It is no business of ours. Let the two fight it out and settle their squabbles among themselves." Were they to undertake the responsibility of directly administering the country (and that would be no easy matter considering the facilities for guerilla warfare which such a mountainous country affords to the defenders) many of their European critics would probably be the first to denounce them for crushing the liberties of the Nestorian people. From the point of view of the governing power, the Christians are undoubtedly better worth encouraging than the Kurds. The former is physically a finer man, he is at least naturally more industrious (for the Kurd is incurably lazy), and he has a genuine and intense feeling of patriotism and hatred of foreign control, whereas the average Kurd cares about nothing except his own immediate interests. The real reason why the Turks act as they do towards the Christian is that they cannot make up their minds to trust them. They know that the Kurd is a fool, but they think, and with some truth, that the Christian is often a knave. In spite of all his faults, many of which arise from sheer slowness of intellect, the Turk is at least a man of his word, and in spite of all their virtues, their constant gaiety and affectionate disposition, truthfulness is not a quality which can be predicated of the Nestorians any more than of the Armenians. On the other hand there is, I believe, a possible future for the one which there cannot be for the other. The Nestorian at all events preserves the same corporate loyalty and the same capacity for self-government which for centuries has preserved his nationality in the face of far greater perils than any which threaten it to-day, and if the Turks act wisely and do not attempt artificially to bolster up the Kurds and turn them into a perfectly useless body of untrained and undisciplined irregulars, the Christians of Hakkiari may serve them as most valuable allies in any war which tempts the invading army to force a passage from the Persian valleys south of Lake Urmi to the fertile plains of the Tigris and the Lesser Zab.

[P.]

WEEKLY EVENING MEETING,

Friday, May 24, 1901.

The Duke of Northumberland, K.G. D.C.L. F.R.S., President, in the Chair.

Richard T. Glazebrook, Esq., M.A. D.Sc. F.R.S. *M.R.I.*

The Aims of the National Physical Laboratory.

The idea of a physical laboratory in which problems bearing at once on science and on industry might be solved is comparatively new. The Physikalisch-Technische Reichsanstalt, founded in Berlin by the joint labours of Werner von Siemens and von Helmholtz during the years 1883–87, was perhaps the first. It is less than ten years since Dr. Lodge, in his address to Section A of the British Association, outlined the scheme of work for such an institution here in England. Nothing came of this; a committee met and discussed plans, but it was felt to be hopeless to approach the Government, and without Government aid there were no funds.

Four years later, however, the late Sir Douglas Galton took the matter up. In his address to the British Association in 1895, and again in a paper read before Section A, he called attention to the work done for Germany by the Reichsanstalt and to the crying need for a similar institution in England.

The result of this presidential pronouncement was the formation of a committee which reported at Liverpool, giving a rough outline of a possible scheme of organisation. A petition to Lord Salisbury followed, and as a consequence a Treasury Committee, with Lord Rayleigh in the chair, was appointed to consider the desirability of establishing a National Physical Laboratory. The committee examined more than thirty witnesses, and then reported unanimously "that a public institution should be founded for standardising and verifying instruments for testing materials and for the determination of physical constants."

It is natural to turn to the words of those who were instrumental in securing the appointment of this committee, and to the evidence it received, in any endeavour to discuss its aim. As was fitting, Sir Douglas Galton was the first witness to be called. It is a source of sorrow to his many friends that he has not lived to see the Laboratory completed.

And here I may refer to another serious loss which, in the last few days, the Laboratory has sustained. Sir Courtenay Boyle was a

member of Lord Rayleigh's committee, and as such was convinced of the need for the Laboratory and of the importance of the work it could do. He took an active part in its organisation, sparing neither time nor trouble; he intended that it should be a great institution, and he had the will and the power to help. The country is the poorer by his sudden death.

Let me now quote some of Sir Douglas Galton's evidence. "Formerly our progress in machinery," he says, "was due to accuracy of measurement, and that was a class of work which could be done, as Whitworth showed, by an educated eye and educated touch. But as we advance in the applications of science to industry we require accuracy to be carried into matters which cannot be so measured. . . . In the more delicate researches which the physical, chemical and electrical student undertakes, he requires a ready means of access to standards to enable him to compare his own work with that of others." Or again, "My view is that if Great Britain is to retain its industrial supremacy we must have accurate standards available to our research students and to our manufacturers. I am certain that if you had them our manufacturers would gradually become very much more qualified for advancing our manufacturing industry than they are now. But it is also certain that you cannot separate some research from a standardising department." Then, after a description of the Reichsanstalt, he continues, "What I would advocate would be an extension of Kew in the direction of the second division of the Reichsanstalt, with such auxiliary research in the establishment itself as may be found necessary." The second division is the one which takes charge of technical and industrial questions. Professor Lodge, again, gave a very valuable summary of work which ought to be done.

It is now realised, at any rate by the more enlightened of our leaders of industry, that science can help them. This fact, however, has been grasped by too few in England; our rivals in Germany and America know it well, and the first aim of the Laboratory is to bring its truth home to all, to assist in promoting a union which is certainly necessary if England is to retain her supremacy in trade and in manufacture, to make the forces of science available for the nation, to break down by every possible means the barrier between theory and practice, and to point out plainly the plan which must be followed unless we are prepared to see our rivals take our place.

"Germany," an American writer who has recently made a study of the subject has said, "is rapidly moving towards industrial supremacy in Europe. One of the most potent factors in this notable advance is the perfected alliance between science and commerce existing in Germany. Science has come to be regarded there as a commercial factor. If England is losing her supremacy in manufactures and in commerce, as many claim, it is because of English conservatism and the failure to utilise to the fullest extent the lessons taught by science, while Germany, once the country of

dreamers and theorists, has now become intensely practical. Science there no longer seeks court and cloister, but is in open alliance with commerce and industry." It is our aim to promote this alliance in England, and for this purpose the National Physical Laboratory has been founded.

It is hardly necessary to quote chapter and verse for the assertion that the close connection between science and industry has had a predominant effect on German trade. If authority is wanted, I would refer to the history of the anilin dye manufacture, or, to take a more recent case, to the artificial indigo industry, in which the success of the Badische Company has recently been so marked. The factory at Ludwigshaven started thirty-five years ago with thirty men; it now employs more than 6000 and has on its staff 148 trained scientific chemists. And now, when it is perhaps too late, the Indian planters are calling in scientific aid, and the Indian Government are giving some 3500*l.* a year in investigation.

As Professor Armstrong, in a recent letter to the *Times*, says, "The truly serious side of the matter, however, is not the prospective loss of the entire indigo industry so much as the fact that an achievement such as that of the Badische Company seems past praying for here." Another instance is to be found in the German exhibit of scientific instruments at the Paris Exhibition, of which a full account appeared in the pages of *Nature*.

And now, having stated in general terms the aims of the Laboratory and given some account of the progress in Germany, let me pass to some description of the means which have been placed at our disposal to realise those aims. I then wish, if time permits, to discuss in fuller detail some of the work which it is hoped we may take up immediately.

The Laboratory is to be at Bushy House, Teddington. I will pass over the events which led to the change of site from the Old Deer Park at Richmond to Bushy. It is sufficient to say that at present Kew Observatory in the Deer Park will remain as the Observatory department of the Laboratory, and that most of the important verification and standardisation work which in the past has been done there will still find its home in the old building.

Bushy House was originally the official residence of the Ranger of Bushy Park. Queen Anne granted it in 1710 to the first Lord Halifax. In 1771 it passed to Lord North, being then probably rebuilt. Upon the death of Lord North's widow in 1797, the Duke of Clarence, afterwards William IV., became Ranger; after his death in 1837 it was granted to his widow, Queen Adelaide, who lived there until 1849. At her death it passed to the Duc de Nemours, son of King Louis Philippe, and he resided there at intervals until 1896.

In spite of this somewhat aristocratic history, it will make an admirable Laboratory. A description of the Laboratory, with illustrations, will be found in *Nature*, vol. lxiii. p. 300.

The floor space available is much less than that of the Reichsanstalt. But size alone is not an unmixed advantage; there is much to be said in favour of gradual growth and development, provided the conditions are such as to favour growth. Personally I should prefer to begin in a small way if only I felt sure I was in a position to do the work thoroughly: but there is danger of starvation. Even with all the help we get in freedom from rent and taxes, outside repairs and maintenance, the sum at the disposal of the committee is too small.

Science is not yet regarded as a commercial factor in England. Is there no one who realises the importance of the alliance, who will come forward with more ample funds to start us on our course with a fair prospect of success? One candid friend has recently told us in print that the new institution is on such a microscopic scale that its utility in the present struggle is more than doubtful. Is there no statesman who can grasp the position and see that with, say, double the income the chances of our doing a great work would be increased a hundredfold?

The problems we have to solve are hard enough: give us means to employ the best men and we will answer them; starve us and then quote our failure as showing the uselessness of science applied to industry!

There is some justice in the criticism of one of our technical papers. I have recently been advertising for assistants, and a paper in whose columns the advertisement appears writes, "The scale of pay is certainly not extravagant. It is, however, possible that the duties will be correspondingly light."

Now let me illustrate these aims by a more detailed account of some of the problems of industry which have been solved by the application of science, and then of some others which remain unsolved and which the Laboratory hopes to attack. The story of the Jena Glass Works is most interesting; I will take it first.

An exhibition of scientific apparatus took place in London in 1876. Among the visitors to this was Professor Abbe, of Jena, and in a report he wrote on the optical apparatus he called attention to the need for progress in the art of glass making if the microscope were to advance, and to the necessity for obtaining glasses having a different relation between dispersion and refractive index than that found in the material at the disposal of opticians. Stokes and Harcourt had already made attempts in this direction, but with no marked success.

In 1881 Abbe and Schott, at Jena, started their work. Their undertaking, they write five years later in the first catalogue of their factory, arose out of a scientific investigation into the connection between the optical properties of solid amorphous fluxes and their chemical constitution. When they began their work, some six elements only entered into the composition of glass. By 1888 it had been found possible to combine with these, in quantities up to

about 10 per cent., twenty-eight different elements, and the effect of each of these on the refractive index and dispersion had been measured. Thus, for example, the investigators found that by the addition of boron the ratio of the length of the blue end of the spectrum to that of the red was increased; the addition of fluorine, potassium or sodium produced the opposite result.

Now in an ordinary achromatic lens of crown and flint, if the total dispersion for the two be the same, then for the flint glass the dispersion of the blue end is greater, that of the red less than for the crown; thus the image is not white: a secondary spectrum is the result.

Abbe showed, as Stokes and Harcourt had shown earlier, that by combining a large proportion of boron with the flint, its dispersion was made more nearly the same as that of the crown, while by replacing the silicates in the crown glass by phosphates, a still better result was obtained, and by the use of three glasses three lines of the spectrum could be combined; the spectrum outstanding was a tertiary one, and much less marked than that due to the original crown and flint glass. The modern microscope became possible.

The conditions to be satisfied in a photographic lens differ from those required for a microscope. Von Seidel had shown that with the ordinary flint and crown glasses the conditions for achromatism and for flatness of field cannot be simultaneously satisfied. To do this we need a glass of high refractive index and low dispersive power, or *vice versâ*; in ordinary glasses these two properties rise and fall together. By introducing barium into the crown glass a change is produced in this respect. For barium crown the refractive index is greater and the dispersive power less than for soft crown.

With two such glasses, then, the field can be achromatic and flat. The wonderful results obtained by Dallmeyer and Ross in this country, by Zeiss and Steinheil in Germany, are due to the use of these new glasses. They have also been applied with marked success to the manufacture of the object-glasses of large telescopes.

But the Jena glasses have other uses besides optical. "About twenty years ago"—the quotation is from the catalogue of the German exhibition—"the manufacture of thermometers had come to a dead stop in Germany, thermometers being then invested with a defect, their liability to periodic changes, which seriously endangered German manufacture. Comprehensive investigations were then carried out by the Normal Aichungs Commission, the Reichanstalt and the Jena Glass Works, and much labour brought the desired reward."

The defect referred to was the temporary depression of the ice point which takes place in all thermometers after heating. Let the ice point of a thermometer be observed; then raise the thermometer to, say, 100°, and again observe the ice point as soon as possible afterwards; it will be depressed below its previous position. In

some instruments of Thuringian glass a depression of as much as 0°·65 C. had been noted. For scientific purposes such an instrument is quite untrustworthy. If it be kept at, say, 15°, and then immersed in a bath at 30°, its reading will be appreciably different from that which would be given if it were first raised to, say, 50°, allowed to cool quickly just below 30°, and then put into the bath. This was the defect which the investigators set themselves to cure.

Table I. gives some details as to thermometers.

TABLE I.

DEPRESSION OF FREEZING POINT FOR VARIOUS THERMOMETERS.

	Degrees.
Humboldt, 1835	0·06
Greiner, 1872	0·38
Schultzer, 1875	0·44
Rapps, 1878	0·65
English glass	0·15
Verre Dur	0·08
16‴	0·05
59‴	0·02

ANALYSIS OF GLASSES.

	SiO_2	Na_2O	CaO	Al_2O_3	ZnO	B_2O_3
16‴	67·5	14	7	2·5	7	2
59‴	72	11	—	5	—	12

Weber had found in 1883 that glasses which contain a mixture of soda and potash give a very large depression. He made a glass free from soda with a depression of 0°·1. The work was then taken up by the Aichungs Commission, the Reichsanstalt and the Jena factory. Weber's results were confirmed. An old thermometer of Humboldt's, containing 0·86 per cent. of soda and 20 per cent. of potash, had a depression of 0°·06, while a new instrument, in which the percentages were 12·7 per cent. and 10·6 per cent. respectively, had a depression of 0°·65.

An English standard, with 1·5 per cent. of soda and 12·3 per cent. of potash, gave a depression of 0°·15, while a French "verre dur" instrument, in which these proportions were reversed, gave only 0°·08.

It remained to manufacture a glass which should have a low depression and at the same time other satisfactory properties. The now well-known glass 16‴ is the result. Its composition is shown in the Table.

The fact that there was an appreciable difference between the scale of the 16‴ glass and that of the air thermometer led to further investigations, and another glass 59‴, a borosilicate containing 12 per cent. of boron, was the consequence. This glass has a still smaller depression.

Previous to 1888 Germany imported optical glass. At that date

nearly all the glass required was of home manufacture. Very shortly afterwards an export trade in raw glass began, which in 1898 was worth 30,000*l.* per annum, while the value of optical instruments, such as telescopes, field-glasses and the like, exported that year was 250,000*l.* Such are the results of the application of science, i. e. organised common sense, to a great industry. The National Physical Laboratory aims at doing the like for England.

I have thus noted very briefly some of the ways in which science has become identified with trade in Germany, and have indicated some of the investigations by which the staff of the Reichsanstalt and others have advanced manufactures and commerce.

Let us turn now to the other side, to some of the problems which remain unsolved, to the work which our Laboratory is to do and by doing which it will realise the aims of its founders.

The microscopic examination of metals was begun by Sorby in 1864. Since that date many distinguished experimenters, Andrews, Arnold, Ewing, Martens, Osmond, Roberts-Austen, Stead and others, have added much to our knowledge. I am indebted to Sir W. Roberts-Austen for the slides which I am about to show you to illustrate some of the points arrived at. Professor Ewing a year ago laid before the Royal Institution the results of the experiments of Mr. Rosenhain and himself.

This microscopic work has revealed to us the fact that steel must be regarded as a crystallised igneous rock. Moreover, it is capable, at temperatures far below its melting point, of altering its structure completely, and its mechanical and magnetic properties are intimately related to its structure. The chemical constitution of the steel may be unaltered, the amounts of carbon, silicon, manganese, &c., in the different forms remain the same, but the structure changes, and with it the properties of the steel.

Sections of the same steel polished and etched after various treatments show striking differences. For instance, if a highly carburised form containing 1·5 per cent. of carbon be cooled down from the liquid state, the temperature being read by the deflection of a galvanometer needle in circuit with a thermopile, the galvanometer shows a slowly falling temperature till we reach 1380° C., when solidification takes place; the changes which now go on take place in solid metal. After a time the temperature again falls until we reach 680°, when there is an evolution of heat; had the steel been free from carbon there would have been evolution of heat at 895° and again at 766°. Now throughout the cooling, molecular changes are going on in the steel. By quenching the steel suddenly at any given temperature we can check the change and examine microscopically the structure of the steel at the temperature at which it was checked.

[Slides were shown representing the microscopic structures of steels subjected to different treatment as regards temperature and annealing.]

These slides are sufficient to call attention to the changes which occur in solid iron, changes whose importance is now beginning to be realised. On viewing them it is a natural question to ask how all the other properties of iron related to its structure; can we by special treatment produce a steel more suited to the shipbuilder, the railway engineer or the dynamo maker than any he now possesses?

These marked effects are connected with variations in the condition of the carbon in the iron; can equally or possibly more marked changes be produced by the introduction of some other element? Guillaume's nickel steel, with its small coefficient of expansion, appears to have a future for many purposes; can it or some modification be made still more useful to the engineer?

We owe much to the investigations of the Alloys Research Committee of the Institution of Mechanical Engineers. Their distinguished chairman holds the view that the work of that committee has only begun, and that there is scope for such research for a long time to come at the National Physical Laboratory. The executive committee have accepted this view by naming as one of the first subjects to be investigated the connection between the magnetic quality and the physical, chemical and electrical properties of iron and its alloys, with a view especially to the determination of the conditions for low hysteresis and non-agency properties.

At any rate we may trust that the condition of affairs mentioned by Mr. Hadfield in his evidence before Lord Rayleigh's Commission which led a user of English steel to specify that before the steel could be accepted it must be stamped at the Reichsanstalt, will no longer exist.

The subject of wind pressure, again, is one which has occupied this committee's attention to some extent. The Board of Trade rules require that in bridges and similar structures (1) a maximum pressure of 56 lbs. per square foot be provided for; (2) that the effective surface on which the wind acts should be assumed as from once to twice the area of the front surface, according to the extent of the openings in the lattice girders; (3) that a factor of safety of 4 for the iron work and of 2 for the whole bridge overturning be assumed. These recommendations were not based on any special experiments. The question had been investigated in part by the late Sir W. Siemens.

During the construction of the Forth Bridge Sir B. Baker conducted a series of observations. The results of the first two years' observations are shown in Table II., taken from a paper read at the British Association in 1884. Three gauges were used.

In No. 1 the surface on which the wind acted was about $1\frac{1}{2}$ square feet in area; it was swivelled so as always to be at right angles to the wind. In No. 2 the area of surface acted on was of the same size, but it was fixed with its plane north and south. No. 3 was also fixed in the same direction, but it had 200 times the area, its surface being 300 square feet.

Table II.

Revolving Gauge.		Small Fixed Gauge.		Large Fixed Gauge.	
Mean Pressure.		Easterly.	Westerly.	Easterly.	Westerly.
lb.	lb.	lb.	lb.	lb.	lb.
0 to 5	3·09	3·47	2·92	2·04	1·9
5 to 10	7·58	4·8	7·7	3·54	4·75
10 to 15	12·4	6·27	13·2	4·55	8·26
15 to 20	17·06	7·4	17·9	5·5	12·66
20 to 25	21·0	12·25	22·75	8·6	19·0
25 to 30	27·0		28·5		18·25
30 to 35	32·5		38·5		21·5
Above	65		41·0		35·25
(One observation only above 32·5).					

In preparing the table the mean of all the readings of the revolving gauge between 0 and 5, 5 and 10, &c., lbs. per square foot have been taken, and the mean of the corresponding readings of the small fixed gauge and the large fixed gauge set opposite, these being arranged for easterly and westerly winds.

Two points are to be noticed : (1) only one reading of more than 32·5 lbs. was registered, and this, it is practically certain, was due to faulty action in the gauge.

Sir B. Baker has kindly shown me some further records with a small gauge.

According to these, pressures of more than 50 lbs. have been registered on three occasions since 1886. On two other occasions the pressures, as registered, reached from 40 to 50 lbs. per square foot. But the table, it will be seen, enables us to compare the pressure on a small area with the average pressure on a large area, and it is clear that in all cases the pressure per square foot as given by the large area is much less than that deduced from the simultaneous observations on the small area.

The large gauge became unsafe in 1896 and was removed ; but the observations for the previous ten years entirely confirm this result, the importance of which is obvious. The same result may be deduced from the Tower Bridge observations. Power is required to raise the great bascules, and the power needed depends on the direction of the wind. From observations on the power some estimate of the average wind pressure on the surface may be obtained, and this is found to be less than the pressure registered by the small wind gauges. Nor is the result surprising, when the question is looked at as a hydrodynamical problem ; the lines of fluid near a small obstacle will differ from those near a large one, and the distribution of pressure over the large area will not be uniform. Sir W. Siemens is said to have found places of negative pressure near such an obstacle.

As Sir J. Wolfe Barry has pointed out, if the average of 56 lbs. to the square foot is excessive, then the cost and difficulty of erection of large engineering works is being unnecessarily increased. Here is a problem well worthy of attention, and about which but little is known. The same, too, may be said about the second of the Board of Trade rules. What is the effective surface over which the pressure is exerted on a bridge? On this again our information is but scanty. Sir B. Baker's experiments for the Forth Bridge led him to adopt as his rule, Double the plane surface exposed to the wind and deduct 50 per cent. in the cases of tubes. On this point again further experiments are needed.

To turn from engineering to physics. In metrology, as in many other branches of science, difficulties connected with the measurement of temperature are of the first importance.

I was asked some little time since to state, to a very high order of exactness, the relation between the yard and the metre. I could not give the number of figures required. The metre is defined at the freezing point of water, the yard at a temperature of 62° F. When a yard and a metre scale are compared they are usually at about the same temperature; the difficulty of comparison is enormously increased if there be a temperature difference of 30° F. between the two scales. Hence we require to know the temperature coefficients of the two standards. But that of the standard yard is not known; it is doubtful, I believe, if the composition of the alloy of which it is made is known, and in consequence Mr. Chaney has mentioned the determination of coefficients of expansion as one of the investigations which it is desirable that the Laboratory should undertake.

Or, again, take thermometry. The standard scale of temperature is that of the hydrogen thermometer; the scale in practical use in England is the mercury in flint glass scale of the Kew standard thermometers. It is obvious that it is of importance to science that the difference between the scales should be known, and various attempts have been made to compare them. But the results of no two series of observations which have been made agree satisfactorily. The variations arise probably in great measure from the fact that the English glass thermometer, as ordinarily made and used, is incapable of the accuracy now demanded for scientific investigations. The temporary depression of the freezing point already alluded to in discussing the Jena glass is too large; it may amount to three- to four-tenths of a degree when the thermometer is raised 100°. Thus the results of any given comparison depend too much on the immediate past history of the thermometer employed, and it is almost hopeless to construct a table, accurate, say, to ·01, which will give the difference between the Kew standard and the hydrogen scale, and so enable the results of former work in which English thermometers were used to be expressed in standard degrees.

This is illustrated by Table III., which gives the differences as found (1) by Rowland; (2) Guillaume; (3) ? Wiebe, between a Kew thermometer and the air thermometer.

TABLE III.—VALUES OF CORRECTIONS TO THE ENGLISH GLASS THERMOMETER SCALE TO GIVE TEMPERATURES ON THE GAS THERMOMETER SCALE FOUND BY VARIOUS OBSERVERS.

Temp.	Rowland.	Gu'llaume.	Wiebe.
°	°	°	°
0	0	0	0
10	− ·03	− ·009	+ ·03
20	− ·05	− ·009	+ ·00
30	− ·06	− ·002	+ ·02
40	− ·07	+ ·007	+ ·09
50	− ·07	+ ·016	+ ·14
60	− ·06	+ ·014	
70	− ·04	+ ·028	
80	− ·02	+ ·026	
90	− ·01	+ ·017	
100	0	0	

It is clearly important to establish in England a mercury-scale of temperatures which shall be comparable with the hydrogen scale, and it is desirable to determine as nearly as may be the relation between this and the existing Kew scale.

I am glad to say that in the first endeavour we have secured the valuable co-operation of Mr. Powell, of the Whitefriars Works, and that the first specimens of glass he has submitted to us bid fair to compare well with 16‴.

Another branch of thermometry in which there is much to do is the measurement of high temperature. Professor Callendar has explained here the principles of the resistance thermometer, due first to Sir W. Siemens. Sir W. C. Roberts-Austen has shown how the thermopile of Le Chatellier may be used for the measurement of high temperatures. There is a great work left for the man who can introduce these or similar instruments to the manufactory and the forge, or who can improve them in such a manner as to render their uses more simple and more sure. Besides, at temperatures much over 1000° C., the glaze on the porcelain tube of the pyrometer gives way.

So far we have discussed new work, but there is much to be done in extending a class of work which has gone on quietly and without much show for many years at the Kew Observatory. Thermometers and barometers, wind gauges and other meteorological apparatus, watches and chronometers, and many other instruments are tested there in great numbers, and the value of the work is undoubted. The competition among the best makers for the first place, the best watch of the year, is most striking and affords ample testimony to the importance of the work.

Work of this class we propose to extend. Thus, there is no place where pressure gauges or steam indicators can be tested. It is

intended to take up this work, and for this purpose a mercury pressure column is being erected.

Again, there are the ordinary gauges in use in nearly every engineering shop. These, in the first instance, have probably come from Whitworth's, or nowadays, I fear, from Messrs. Pratt and Whitney or Browne and Sharpe, of America. They were probably very accurate when new, but they wear, and it is only in comparatively few large shops that means exist for measuring the error and for determining whether the gauge ought to be rejected or not. Hence arise difficulties of all kinds. Standardisation of work is impossible.

In another direction a wide field is offered in the calibration and standardisation of glass measuring vessels of all kinds, flasks, burettes, pipettes, &c., used by chemists and others. At the request of the Board of Agriculture we have already arranged for the standardisation of glass vessels used in the Babcock method of measuring the butter fat in milk, and in a few months many of these have passed through our hands. We are now being asked to arrange for testing the apparatus for the Gerber and Leffman-Beam methods, and this we have promised to do when we are settled at Bushy. Telescopes, opera-glasses, sextants, and other optical appliances, are already tested at Kew, but this work can, and will, be extended. Photographic lenses are now examined by eye; a photographic test will be added, and I trust the whole may be made more useful to photographers.

I look to the co-operation of the Optical Society to advise how we may be of service to them in testing spectacles, microscope lenses and the like. The magnetic testing of specimens of iron and steel, again, offers a fertile field for inquiry. If more subjects are needed it is sufficient to turn over the pages of the evidence given before Lord Rayleigh's Commission, or to look to the reports which have been prepared by various bodies of experts for the executive committee.

In electrical matters there are questions relating to the fundamental units on which, in Mr. Trotter's opinion, we may help the officials of the Board of Trade. Standards of capacity are wanted; those belonging to the British Association will be deposited at the Laboratory. Standards of electromagnetic induction are desirable; questions continually arise with regard to new forms of cells other than the standard Clark cell, and in a host of other ways work will be found.

I have gone almost too much into detail. It has been my wish to state in general terms the aims of the Laboratory to make the advance of physical science more readily available for the needs of the nation, and then to illustrate the way in which it is intended to attain those aims. I trust I may have shown that the National Physical Laboratory is an institution which may deservedly claim the cordial support of all who are interested in real progress.

[R. T. G.]

WEEKLY EVENING MEETING.

Friday, May 31, 1901.

Frank McClean, Esq., M.A. LL.D. F.R.S., Vice-President, in the Chair.

A. Henry Savage Landor, Esq., *M.R.I.*

With the Allies in China.

After the fall of Tientsin native city, and the excitement of looting it, it was felt by the Allies that a good rest was necessary before an attempt could be made to relieve the Legations in Pekin.

The idea prevailed in Tientsin that the Ministers and all foreigners in the capital could not have escaped massacre. If the Imperial troops in Pekin were as well armed and drilled as those who fought in Tientsin, we could but surmise that the Legations, with the small guards and limited ammunition they possessed, could not have withstood a long and severe siege.

The generals of the Allies thought that at least 25,000 men were necessary for an advance on Pekin. Some suggested that 40,000 would be a figure at which a greater chance of success might be expected. The rainy season would be coming shortly, and would render the country almost impassable. The railway had been destroyed.

Various unsuccessful attempts had been made to communicate with Pekin by means of disguised messengers. Day after day passed, and we in Tientsin heard no news of the besieged, which made us fear the worst.

It was not till July 29 that the Allies woke up to the real state of affairs, on the receipt of a pathetic letter from Sir Claude MacDonald. It said: "If the Chinese do not press their attack we can hold out for some days—say ten; but if they show determination, it is a question of four or five, so no time should be lost if a terrible massacre is to be avoided."

Directly afterwards a second messenger brought to the American Consul a small piece of tissue paper on which was a cypher message from Mr. Conger, the United States Minister. It was dated July 21, and said that the Legations had sufficient provisions, but little ammunition. Fifty had already been killed. The Japanese Consul received a messenger a few days later.

It was plain that the besieged in the Legations were in a sorrowful plight. At any cost, an attempt to relieve them must be made at once.

For two or three days there was a great commotion in Tientsin to prepare for the advance. Pekin carts were commandeered in all directions, as well as saddles, ponies, mules, donkeys and rickshaws.

On August 3, a conference of generals was held, at which it was decided that the combined forces of the Allies now ready in Tientsin should make an immediate start for Pekin, without waiting for the arrival of further reinforcements. It was agreed that the advance could be made on August 4.

It was not till the afternoon of that day that the troops began to move out of the settlement, raising clouds of dust on the road, and rattling the heavy gun carriages over the rickety wooden bridges outside Tientsin native city.

The troops that took part in the advance consisted of:—

Japanese.—One brigade infantry, and all the cavalry available; four companies of artillery; one company of engineers.

British.—Royal Welsh Fusiliers, Royal Artillery, 7th Bengal Infantry, 1st Bengal Lancers, 1st Sikhs, 24th Punjab Infantry.

American.—9th and 14th Infantry.

The Japanese, British and Americans were to work in a joint movement on the west bank of the Pei-ho river; while the Russians (East Siberian Regiment and Cossacks), French, Germans, Italians and Austrians were to march on the east bank.

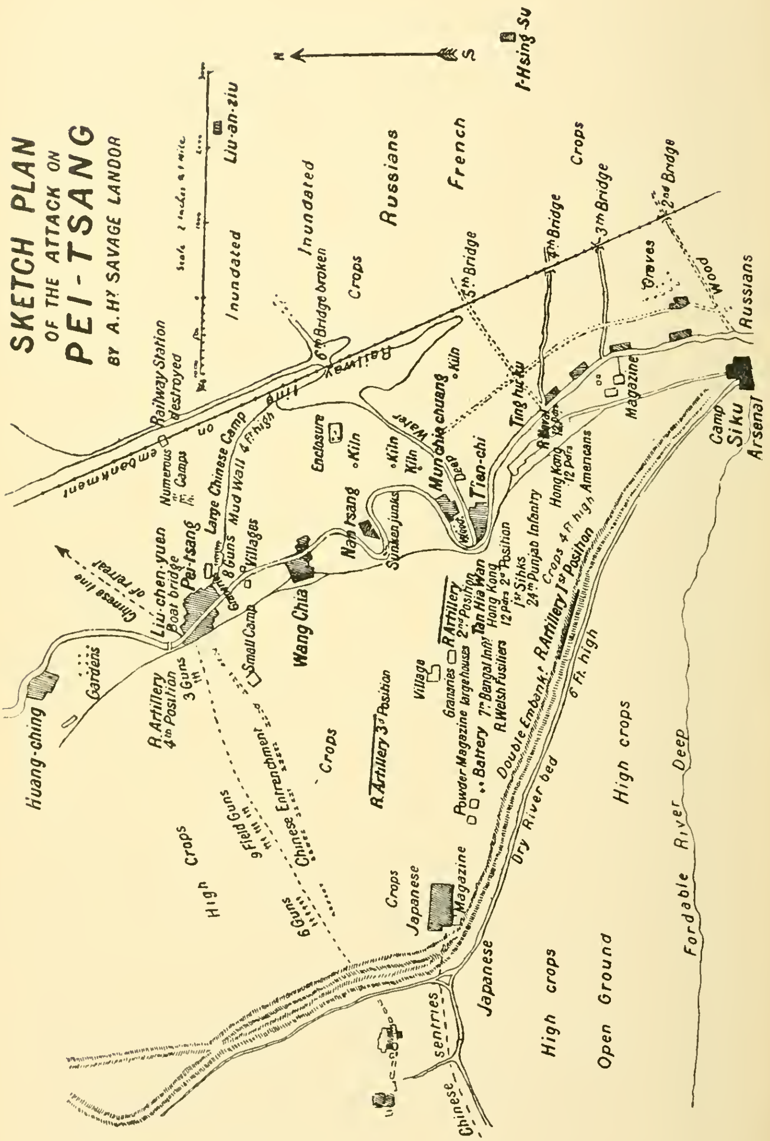
The night of the 4th was spent by the Allies at the Siku arsenal, the English and Russians acting as outposts. The Russians occupied the Siku arsenal itself, the British and Americans the right and left centre, and the Japanese the extreme left.

From the Siku arsenal a double embankment ran along in a north-westerly direction as far as a magazine, then turned almost north beyond it. Two buildings, a gunpowder magazine, a small village, and a few scattered houses and granaries, stood in the large triangular stretch of flat country, now covered with high Indian corn, and that was enclosed by the river on one side and the road embankment on the other, the Siku arsenal being the point of the triangle.

Pei-tsang, where the Chinese were reported in great force, was about six thousand yards north-west of Siku. The Chinese were very strongly entrenched behind several lines of earthworks stretching to the south-west from Pei-tsang and to the south-east along a mud wall.

There were several miles of trenches, skilfully laid out, and the enemy had placed behind them six guns at their extreme right, nine field guns in the centre of the line, three guns directly west of Pei-tsang, and eight guns near the granaries south-east of the village. It was a formidable position to attack.

During the night the Allies took up a position to the south of the embankment, the Japanese occupying the extreme left, close to the magazine. They brought up their artillery, under the command of Major-General Tskamoto, with the 21st Brigade of Infantry, the 5th Regiment of Cavalry, one company of engineers, the 5th Regiment of Artillery, and ambulances.


SKETCH PLAN
OF THE ATTACK ON
PEI-TSANG
BY A. HY. SAVAGE LANDOR
N
S
Railway Station destroyed
embankment
Railway
Huang-ching
Gardens
Liu-chen-yuen
Boat bridge
Pei-tsang
Chinese line of retreat
Numerous Camps
Large Chinese Camp
Mud Wall 4 Ft. high
8 Guns
Villages
Small Camp
Wang Chia
Nan Tsang
Sunken junks
Enclosure
Kiln
Mun chie chuang
Water
Deep
Tien-chi
Wood
6th Bridge broken
Inundated
Inundated
Crops
Liu-an-ziu
Russians
French
I-Hsing-su
5th Bridge
4th Bridge
3rd Bridge
2nd Bridge
Crops
Graves
Wood
Russians
Ting-tzu-ku
Magazine
Camp
Siku
Arsenal
Hong Kong 12 Pdrs
Americans
Crops 4 Ft. high
R. Artillery 1st Position
24th Punjab Infantry
1st Sikhs
12 Pdrs 2nd Position
Hong Kong
Tan Hia Wan
R. Artillery 2nd Position
Granaries
Powder-Magazine large houses
Battery
7th Bengal Infy.
R. Welsh Fusiliers
Village
R. Artillery 3rd Position
Double Embankt.
6 Ft. high
Dry River bed
High crops
Fordable River Deep
Open Ground
High crops
Japanese
sentries
Chinese
Magazine
Japanese
Crops
6 Guns
Chinese Entrenchment
9 Field Guns
High crops
Crops
R. Artillery 4th Position
3 Guns

To the right, under the command of Major-General Manabe, was the 9th Brigade, one company of cavalry, one battery of artillery and one company of engineers.

The reserve consisted of the 11th Regiment, taken out of the 9th Brigade and one company of engineers.

Next to the Japanese along the embankment were, under cover, the British forces.

The Americans lost their way and did not take part in the engagement.

At 4 a.m. on August 5, the Allies had taken up their position. The Japanese had pushed their way right up to the Chinese sentries near the magazine, and with the first rays of light, at 4.20, got the first glimpse of the enemy.

Ten minutes later, at 4.30, the magazine was in the hands of the Japanese. With this was also captured the first line of Chinese trenches. Three thousand Chinese troops were reported to be guarding the powder magazine, but they withdrew to their main defences. A Japanese battery was set to work at this spot.

I climbed with a friend over the embankment to obtain a better view, and I suppose that we placed ourselves in full sight of the enemy against the sky line, for directly three or four shells whizzed uncomfortably past us, exploding a little way beyond.

From this moment the Chinese, suspecting the whereabouts of the Allies, began to send shell after shell into our position.

The Japanese artillery was doing splendidly, but the Chinese had found the exact range of the Japanese guns, and were making their position very hot. The gunners were cool and composed, and the Japanese officers calmly smoked their cigarettes, cracking occasional jokes when shells burst too near. The moment any one was wounded he was bandaged up and carried away on an ambulance.

As I was looking on there was one soldier holding three horses. A Chinese shell dropped, and man and animals were killed, and gashed about in a fearful manner.

I thought it was wiser for me to go and see what the British artillery was doing, a little further back along the embankment. It had not yet come into action. This seemed a spot of comparative safety, as only occasional shells dropped here, instead of a regular hail of them. I was just talking to some soldiers, when a shell burst directly over our heads, wounding one man badly in the neck, and another slightly.

There was no prospect of an immediate advance, as long as the artillery duel continued, so I decided to go still further back, nearly as far as where the Bengal Lancers were in reserve.

I squatted on the ground, and was writing up my notes, when some Japanese Red Cross men approached and asked me whether this was a safe spot, as they wished to bring some wounded. On my answering in the affirmative, off they went, and presently returned with two stretchers, on which were two Japanese, severely wounded.

I went to help them to lay down the poor suffering creatures, when a shell exploded just above us, and again wounded one of the wounded men on the stretcher.

The Chinese were gradually slackening their artillery fire, and apparently withdrawing their guns, when General Fukushima sent word to the British, asking that the cavalry might immediately be despatched to co-operate with the Japanese in the advance on the Chinese position. Somehow or other, the British cavalry never arrived, and the Japanese—only one regiment of artillery—marched forward alone.

The 41st Regiment led the advance with one battalion on the left wing. The fighting was very severe, the Japanese suffering heavily. The Chinese made a stubborn resistance, but were gradually driven from their trenches.

Naturally the Chinese did not confine themselves to firing with one gun. Mauser, Mannlicher and *gingal* bullets were falling thickly. Moreover, the Chinese were using Maxims with considerable success.

The Chinese guns were still giving trouble, and the Royal Artillery had taken up a second position near the granaries (north of its first position), from which, as the Japanese were advancing so rapidly, it soon shifted again, and occupied a third position still further north.

In the meantime the Japanese cavalry charged the enemy, now retreating towards Pei-tsang village, and with great gallantry succeeded in capturing eight guns. The Chinese had withdrawn nearly all the artillery from their central position.

When once the retreat began, it was rapid, success after success being gained by the victorious army. One position after another fell, and the main body of the enemy was retreating even from Pei-tsang itself, but had left sufficient men to cover the retreat. The Chinese heavy artillery ceased to fire on us as we advanced, but they had some vicious Maxims which still poured lead into us.

One of these weapons was trained on a small bridge, over which we were bound to cross, and many a soldier was wounded in the hail of bullets that could not be escaped. The first soldiers who came unexpectedly into it fared badly.

A curious incident happened. I saw a number of Japanese coolies running along, following the soldiers, and I had just time to shout to them, "Abunai! Abunai!" (Look out! Look out!). The bullets made a noise just like hail on the wooden boards of the bridge, so the little fellows covered their heads with their blankets, as they would do in a hailstorm, and dashed across.

When we reached the place where the road crossed a Chinese trench, a commanding position, where the enemy had placed three guns, we found interesting sights. The earthworks and trenches, several miles in length, had been constructed with extraordinary skill, and in them stood the picturesque tents and sheds for the soldiers. We found many dead.

As the British cavalry was not forthcoming, the Japanese now filled the centre with their own infantry.

The enemy was driven out of all his positions except Pei-tsang village itself. The Russians and French, who, owing to inundations, had found great difficulty in advancing on the opposite side of the stream, were threatening them from the south-east.

Sharp fighting took place near the village itself, but eventually the Japanese entered it, and put the enemy in full retreat.

The right wing (Japanese) was ordered to pursue and cut off the flight of the Chinese, but the fighting had been very hard—nearly eight long hours of it—and the enemy had a good start. The Chinese were well ahead, with twelve flags and six guns. Their number was estimated at 6000, and they were falling back on Yangtsun.

At Pei-tsang itself, the fight was over at about noon. Sniping continued for some time afterwards from the fields on the opposite side of the Pei-ho. The Japanese and British, followed later by the others, pushed on directly to the second village, finding no further resistance.

The Chinese troops which had been fighting at Pei-tsang had been reported as 8000 in number, besides a great many well-armed Boxers, who had joined in the fighting.

There is no doubt that at this—the most important battle in the advance on Pekin—the Chinese troops received a blow from which they never recovered.

The battle of Pei-tsang will always remain a fine page in the history of Japan, for the Japanese alone did practically all the work, and won the victory for the Allies.

Some little distance beyond Pei-tsang the Japanese captured a pontoon bridge, leading to a deserted Chinese camp on the other side of the river.

The Japanese losses were heavy. According to the official list they had forty-two killed, eight missing, and twelve officers and two hundred and thirty-four soldiers wounded.

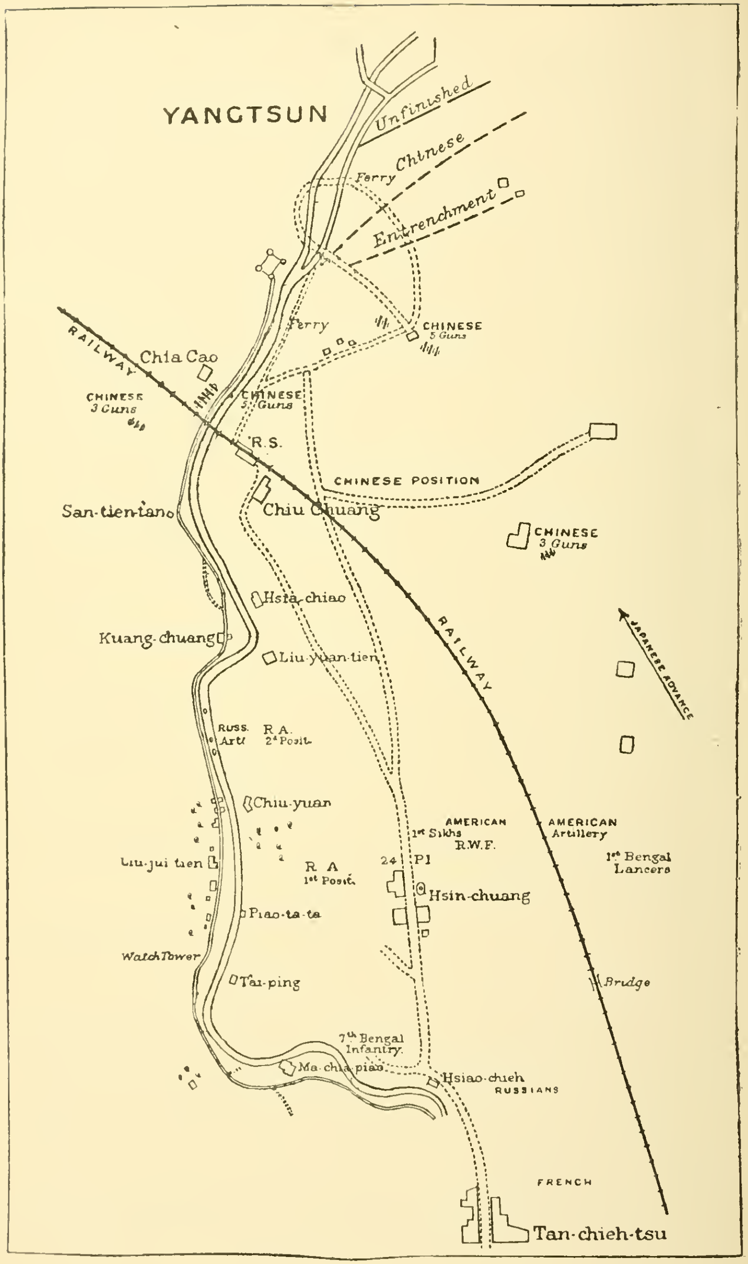
Eight guns were captured from the Chinese.

It was decided to follow up the Chinese at once to Yangtsun, and to give them no time to recover from the blow received at Pei-tsang.

The troops camped that night just beyond the pontoon bridge, and the Russians, French, and Austrians, being unable to deploy on their side of the river owing to the inundations, crossed over and joined the main body of the force on the west side of the stream.

One squadron of the 1st Bengal Lancers made a reconnaissance towards Yangtsun, discovered the enemy in strong force, and returned to camp during the night.

The troops began to march forward again at 6 a.m. on the 6th, and the Japanese (the Manabe Brigade), with the Russians, British, Americans, French, and Austrians, crossed by the bridge, and all marched this time on the east bank of the river.


YANGTSUN
Unfinished
Chinese
Entrenchment
Ferry
Ferry
CHINESE
5 Guns
RAILWAY
Chia Cao
CHINESE
3 Guns
CHINESE
5 Guns
R.S.
CHINESE POSITION
San-tien-tan
Chiu Chuang
CHINESE
3 Guns
Hsia-chiao
Kuang-chuang
Liu-yuan-tien
RAILWAY
JAPANESE ADVANCE
RUSS.
Artl
R.A.
2d Posit.
Chiu-yuan
AMERICAN
1st Sikhs
R.W.F.
AMERICAN
Artillery
Liu-jui tien
R A
1st Posit.
24
P1
1st Bengal
Lancers
Hsin-chuang
Piao-ta-ta
Watch Tower
Tai-ping
Bridge
7th Bengal
Infantry
Ma-chia-piao
Hsiao-chieh
RUSSIANS
FRENCH
Tan-chieh-tsu

And here, with the rough roads, began the first and serious troubles arising from hastily made transport arrangements. The heavily laden carts sank deep into the road; the teams of Chinese mules, unaccustomed to foreign drivers, stampeded, kicked, and smashed harness and carriages, and a considerable amount of strong language, in many different tongues, was consequently used on all sides.

Acting on the information collected by the Bengal Lancers, the British and Americans led the advance, marching about ten miles before coming in touch with the enemy.

The Cossack cavalry discovered the enemy commanding a strong wedge-shaped position formed by the railway embankment and the river, and intersected by a road. The Chinese left flank was protected by three guns near a building, and four hundred cavalry some distance beyond the railway embankment. There were also five guns to the north. Five more stood further back on the opposite bank of the river, directly across the iron railway bridge; and three others on the south side of the railway.

The railway embankment, being very high, and provided with a long platform near the station, furnished a commanding position, with most excellent protection.

On the side of the Allies the line of battle was formed as follows:—

On the left, along the river, were the Russian infantry and artillery (4 guns). Next to them, on the south, and to the right, came the British Royal Artillery, the 1st Sikhs, supported by the 14th United States Infantry on their right on the west side of the track, and the 9th Infantry, supported by marines. Reilly's battery, six guns, and the Bengal Lancers, were on the east side. The Tskamoto Japanese Brigade was held in reserve, and occupied the extreme right of the advance; while more Russians, with the 7th Bengal Infantry to their left, were near the Hsiao-chieh houses, and the French infantry behind them.

At about 1500 yards the line began to deploy with no very great opposition. There was fair cover from trees, undulations in the ground, and stray houses; but when only at nine hundred yards the advance became very slow, and was made under a terrific fire with no cover at all.

As can be seen by a glance at the map, the wedge formed by the embankment of the road and that of the railway becomes gradually narrower, and eventually forms a point at its northern portion. It was at this point that the 1st Sikhs and the 24th Punjab Infantry were forced forward in close formation, with K and M Companies of the 14th United States Infantry by their side.

The 1st Sikhs advanced well until they found themselves in the narrow depression shown in the photograph, where they got penned in and were exposed to very heavy fire. They held fast to their position, while the Americans came along in skirmishing order.

The 14th, which was ahead, came under the fire of the gun which the Chinese had placed on the embankment near the water-tower, and also to that from the rifles of the Chinese infantrymen in houses and behind trees.

The Chinese, furthermore, were lining the whole of the Station platform, whence they kept up a hot fusillade. Their force consisted of Imperial troops in the centre, and well-armed Boxers at the sides.

The Russians, advancing from the same direction, fired volley after volley into the Chinese, and, having brought up their artillery, shelled the enemy with great effect.

The Sikhs were for one moment under such heavy fire that they could not advance. Those few of the Americans who were not exhausted by fatigue and the terrible heat, were ordered by Colonel Daggett, when at eight hundred yards, and under a withering fire from front and flank, to rush the Chinese position.

A handful of them, led by brave Lieutenant Murphy, of the 14th Infantry, and a handful of plucky Sikhs, with Major Scott at their head, stormed the embankment, the Chinese bolting at their approach.

Lieutenant Murphy was the first to reach the position where the Chinese gun had been; then, a second later, came Scott with six Sikhs. Captain Martin, with six men of Company M (United States Infantry) and one man of Company I, arrived next. The Chinese were very smart, and dragged away their battery when the enemy was only three hundred yards off.

So narrow was the wedge when the Americans passed the Sikhs that they actually formed two single lines. When double time was ordered the Americans were so exhausted from the long march in the morning—the attack began at 11 a.m.—and from hunger and thirst, that many dropped on all sides and became delirious, or went clean out of their minds.

When once the enemy had been dislodged from the high embankment, the victory became easy. Captain Taylor, of the 14th United States Infantry, was the first of the Allies to enter the village to the left, and Colonel Daggett reached the platform just in time to see the Chinese withdraw in good order up the river.

In their rear to the north the Chinese had one line of trenches on the road, one line at the bank of the river, and two lines across the plain. When the Allies had seized the top of the embankment, the Chinese infantry, having occupied their first line of trenches, about eight hundred yards from it, opened fire principally from their left, to protect the retreat of their artillery.

They then leisurely withdrew, gaily flying their standards. The American 9th, on the right flank, had a splendid opportunity of firing into them at short range; but as the Chinese were dressed in blue, and flew white, red and blue flags, they were mistaken for Frenchmen and so escaped. Later, the French mistook the Americans for Chinese, and fired into them! Fortunately, they did not hit anybody. When

the first error was discovered it was too late to pursue the enemy effectively.

A worse mistake happened. Either the Russian or the British gunners (nobody seemed to know for certain) sent a few shells among E Company of the 14th United States Infantry, killing eight and wounding nine.

The first Chinese trench was taken by a regiment of "supports," and the others were evacuated.

Two squadrons of the 1st Bengal Lancers went in pursuit of the enemy in the evening, and succeeded in killing fifty. One of General Ma's flags was captured, and five standards.

The Yangtsun battle was over at 2.30 p.m.

The Americans and British, who had borne the brunt of the fighting, had a heavy list of casualties.

American.—21 killed and 54 dangerously wounded.

British.—46 killed and wounded.

On August 9, the Allies were half-way between Tientsin and Pekin.

The Japanese advance guard, when 2500 yards from the town of Ho-si-wu, discovered the enemy in the south end of the village. The Chinese opened fire, but the Japanese stormed the position, and the enemy ran away in confusion.

The town had been ransacked by Boxers and Imperial soldiers prior to our arrival, and this was the case with nearly every village we passed through.

Ho-si-wu was captured at 8.50 a.m., and from some of the inhabitants it was understood that the enemy had been there ten thousand strong, under the supreme command of Generals Ma and Lü. They had abandoned the position at the approach of the Allies, after making a half-hearted defence.

After resting here a while, the march was continued towards Matao, and the enemy was now reported ready to fight us at the walled town of Shan Matao.

I was then with the Japanese advance guard, composed of light cavalry. We started the next morning, the 10th, at 3.30, and at 4.30 the Tskamoto Brigade followed. At Matao itself we had a skirmish with the enemy, and easily succeeded in putting them to flight.

The right and left wing had come together again in one body, at An-ping, and spent the night at Shan Matao.

The Allied cavalry started again at 3.30 the next morning (the 11th), followed by the Tskamoto Brigade and the other Allies. When the advance guard reached Kao-tchan, south of Chang-chia-wan, we suddenly came in for a surprise. We were riding gaily through a narrow street of the suburbs, and had arrived at the bridge, when we were received with a few well-aimed shells, which compelled us quickly to turn back and get under cover of the houses.

Some Japanese infantry came up, and at the same time the left wing reached the gate of Chang-chia-wan, while we galloped down

the side of the canal under a thick fusillade and occasional shells. The enemy, however, withdrew his artillery in time, covering the retreat of his guns with rifle fire.

A halt was called here that the Allies might make preparations for an assault on the large town of Tung-chow, which they believed to be strongly garrisoned.

On the march, the Americans and the British possessed inadequate maps, but somehow or other the British seemed to have a knack of finding their way about and taking care of themselves. The Americans were constantly losing their way, and, exhausted as they were through heat and sickness, were on many occasions given extra suffering by being made to march several miles more than was necessary.

The American soldier is a splendid soldier, although possibly he is physically not very strong. A great many of them fell out of the ranks on the march to Pekin.

The Japanese went steadily and well, but looked somewhat overladen. Some men dropped off, but the little fellows possessed such strong will that when their physical strength failed them their pride made them keep up with the rest.

The Britishers were taking things in a calm fashion, sprawling along in a pretty easy way. They were well fed and properly looked after, and did not seem to suffer quite so much as some of the other troops. They generally marched in the cool of the morning and evening, which saved the men considerably, instead of doing like the Americans, who were made to march in the hottest hours of the day.

The thin-legged Indian troops stood the march very well. There was, however, some fever and dysentery among them, and even more so among the British white troops.

The Russians stood the march in a magnificent manner. I never saw one single man fall out of the ranks, and although, of course, they felt the heat, they undoubtedly proved themselves to be, physically, the strongest soldiers of the Allies.

While the other troops took advantage of the day's rest at Chang-chia-wan, the Japanese advance guard pushed on ahead, and at 1 p.m. was again fighting the enemy, with whom they had caught up, and who was running before them. In this race they had reached within 3000 yards of Tung-chow, when they perceived with spy-glasses a great number of Chinese soldiers on the city wall as well as outside the town. The Japanese artillery was brought up 1000 yards from the city, and shelled the enemy till four o'clock in the afternoon. There seemed, however, to be no sign that an effective resistance would be offered.

When a sufficient number of troops had arrived at Tung-chow, the Japanese commenced the attack on the town at midnight. They were fired upon from the wall, the Chinese actually using some of their home-made guns, over a hundred years old. They had spread a quantity of these primitive guns along the wall. Most of them had not even a gun-carriage, and were merely resting on the

parapet of the wall. In firing them, one or two of these guns actually fell off the wall on to our side.

At 3.30 a.m. on August 12, the Japanese advance guard reached the city gate, while the other troops were deploying. No resistance was offered. One company of engineers blew up the gate with dynamite.

At 4.30 the whole army of the Allies entered the city by the south and south-west gates. A deputation had been received saying that no fighting would take place if the lives and property of the people were safeguarded.

As far as this point the Allies had kept in touch with their transport, the communication being to a considerable extent by water. From Tung-chow, however, the Pei-ho had to be abandoned.

The allied generals held a conference, at which it was decided to march at once on Pekin.

A distance of only fourteen miles separated us.

The Allies would attack the city in the following order :—

The Japanese at the extreme right along the paved road ; the Russians in the centre on the north bank of the canal; while the Americans and British marched to the south of the canal.

It was understood that the troops would encamp for a day some three miles from Pekin, in order to give our soldiers a rest, for through heat, dust, thirst and sickness, the men were in a pitiable condition. Hundreds had already fallen off the ranks—sunstroke, dysentery and typhoid fever playing havoc among them.

During the whole afternoon of August 13, we of the Relief Expedition heard terrific firing in the direction of Pekin. It was so continuous that it resembled thunder. The sky was gloomy, and many thought that it was only an approaching storm.

This, however, did not seem to be the impression prevailing in the Russian camp, where a final and determined attack on the Legations was suspected and feared. Russian scouts had already pushed to within two hundred yards of the wall, and were only fired upon when close to the city gate. They were pursued by a few soldiers, and brought back the news that, as far as the wall itself was concerned, no resistance would be encountered.

Acting upon this information, in the evening of Monday the 13th, one battalion of infantry and half a battery, under the command of Major-General Vassielevsky, set out on a reconnoitring expedition towards Pekin. The object of this was to prepare the way for the attacking force, which was to follow the next morning. The night was very dark, and at 11 o'clock it rained so heavily that the Russians were able to extend their reconnoitring much further than was originally intended. They actually reached the Tung-Pien Men gate of Pekin without being discovered. Finding the enemy unprepared, General Vassielevsky decided not to lose his chance of making a bold stroke.

Along the wall there was a moat with water, which could be

crossed by a small bridge. Vassielevsky ordered his men to creep silently over the bridge and make an attempt to force the gate. The Chinese soldiers at the guard-house, awakened, gave the alarm. There were some thirty of them, and all came to an untimely end.

The Chinese on the wall immediately opened a fusillade on the Russians. Two guns were brought up close to the gates, and firing at once commenced to smash them open. After some twenty shots had been fired, an aperture had been cut large enough for a man to squeeze through.

Two fearless men, General Vassielevsky and Mr. Munthe (a Norwegian acting as guide on the staff of the Russian General), rushed in—the two first men of the Allies to enter the Chinese city of Pekin—and gave order to the soldiers to follow. Once inside, they were under terrific fire in the small walled court which is found between the outer gate and the inner.

The Russian infantry crept in through the small aperture, and answered as best they could the rifle fire poured upon them from the wall. The fusillade on both sides was terrific. The savage yells of the Chinese from above, the flashes of musketry playing along the edge of the wall and everywhere, the deafening din of their gingals and of the Russian rifles, drowned the moaning of those unfortunates who in scores fell wounded and dying.

The side gates having been forced open, three guns were pushed through and carried along the cluster of houses inside the wall. The infantry came in with them, and, in fact, walked ahead of the guns. The Chinese had by now retired little by little from the lower wall of the Chinese city to the adjoining higher wall of the Tartar city, from where they kept a heavy fire on the Russians. But not for long. Some twenty minutes later the firing ceased.

It was now decided to take the inner road close to the wall towards the Ha-ta gate, more especially as the Chinese guides and prisoners declared the outer wall was only weakly defended by the Chinese. The main force, they stated, was guarding the wall of the Tartar city.

The Russian infantry, escorting three guns, started on this road, and had no sooner passed in the vicinity of the higher wall with tower on the corner, than a murderous fire was opened from all along the wall against the advancing force. In a few minutes ten out of eighteen horses of the batteries were down, the officer leading the advance guard was severely wounded, and the majority of his men were killed.

It was impossible to advance under such deadly fire. All the horses of one battery had been shot, and fears were entertained that one gun must be abandoned. It would have been, but for the bravery of the infantrymen, who succeeded, amidst the general enthusiasm, in rescuing the gun, but the loss of life was appalling.

The Russians, unable to proceed, concentrated on the top of the wall at the gate. General Vassielevsky decided to hold the position until reinforcements arrived.

At daybreak the Chinese still occupied the higher wall, on which they were well under cover, and were therefore able to inflict great damage on the Russians.

To the south of the gate occupied by the Russians, the top of the wall was studded with mat sheds, which had been used as tents by the Chinese soldiers. Three Chinese flags were still seen flying on the wall. As it was of the utmost importance to ascertain whether the position was still occupied by the enemy, a few volunteers, led by the brave Munthe and a sergeant, under very heavy fire rushed the position. The flags were captured and the party returned to the gate.

From the Tartar wall the firing was now becoming more and more violent, and the Russian position was furthermore shelled from the city. All the Russians could do was to keep quiet, it being impossible for them to return the fire from the exposed position they held.

General Vassielevsky was all the time in the most exposed place on the wall—a splendid example of valour to his men, as well as a first-class target to the Chinese riflemen. In fact, a Mannlicher bullet went through his chest, and he fell, dangerously wounded. He behaved with much fortitude, and ordered his men to continue the defence. It was impossible to carry the General down from the perilous place in which he lay, and, in two attempts that were made, two Cossacks who carried the stretcher were mortally wounded.

It was not till ten o'clock in the morning that Russian reinforcements could be seen approaching, but instead of advancing immediately—and unaware of the position occupied by their advance guard—they stopped to bombard the high tower on the south-east corner of the Tartar wall. They did so, as from this tower, still occupied by the Chinese, a stout resistance and continuous fusillade was kept up.

Along the Chinese city wall occupied by the Russians, to the south, was seen approaching a large force of Mahommedan soldiers, waving their flags and standards. They advanced courageously towards the Russians in such masses that the latter found themselves in a very precarious position; but they held their own, and fired volley after volley into the swarm of fanatics, keeping them at bay.

At eleven o'clock, reinforcements commenced to arrive, and soon an American flag was seen waving from the wall itself in the position where Munthe and his brave companions had at sunrise captured the three Chinese flags. Later still, a number of plucky American soldiers scaled the wall and reached the Russian position. They were about twenty, under Captain Crozier.

More reinforcements arrived. In fact, the whole American main force proceeded through the gates burst open by Russian artillery.

The Russians called a halt to attend to their dead and wounded. They had twenty-six killed, and one hundred and two lay with mortal wounds, besides a large number of lighter casualties.

An hour later, towards noon, white flags were seen hoisted all along the Tartar wall, and firing practically ceased, except from the south-east corner tower, which persistently continued firing all day, notwithstanding that it was in return heavily shelled by the Russian and later by the American artillery.

The troops, unopposed, marched along the Tartar wall as far as the Ha-ta Men gate, and others as far as the "Sluice" (Water gate), through which, as we shall see, at about two o'clock p.m., an entrance had already been made by the British.

While this was taking place, the brunt of the fighting was borne by the Japanese, who had come up by the paved road leading from Tung-chow to the Chi-ho gate (East gate) of the Tartar city.

On the evening of the 13th, they had encamped some three miles from the East gate of Pekin. Their advance guard was a quarter of a mile in front of the main force. I selected as my own camping ground an open space between the advance guard and the main column. Colonel Mallory, attached to the Japanese force on behalf of the American Government, and Mr. Bass, correspondent of the *Herald*, were with me.

During the night the rain was torrential, and we had to cover ourselves with Colonel Mallory's poncho, when shots were heard in close proximity, and presently a very smart fusillade was opened from in front and behind us. Bullets were hissing over our heads. It was pitch dark, and we did not know exactly what was happening.

There seemed to prevail great excitement in the Japanese camp, and in the meantime the shooting in front could not have been more than forty or fifty yards from us.

At sunrise we discovered that the Chinese had boldly attacked the Japanese advance guard, which had to fall back on the main body, hence the exciting skirmish. They were eventually beaten off.

We marched briskly on Pekin, meeting with no resistance until we got within four hundred yards of the gate, which we reached at about eight in the morning of the 14th. A rush was made on the gate by the infantry—a scene of the wildest enthusiasm.

We were met by a fearful fusillade from the gate and wall, and a sudden shower of shells burst among us. We still advanced for some time, seeking shelter along the road-side, until the firing abated.

Four more guns were sent to shell the Tung-chih gate, where the wall was broken down and reported scaleable. The Chinese made quite a stout resistance on this side of the city. Probably they only expected to be attacked from the east side, and had accordingly made preparations, so that, although British, Russians, and Americans were already inside the Chinese and Tartar city walls since the afternoon, the Japanese did not succeed in blowing up the gate till nine o'clock in the evening. Their losses during the day were two hundred killed and wounded.

The British, in the meantime, had been steadily marching on

from Tung-chow. They found the country clear of the enemy, and entered the wall of the Chinese city of Pekin by the Shan-huo gate. They met with no opposition, the heavy gates being opened with the help of the Chinese from inside.

An immediate advance was made towards the south Tartar gate.

Scouting parties were despatched in every direction to look for Chinese soldiers, but none were found; so General Gaselee, with his staff, and half a company of the 7th Rajiputs, made a reconnaissance through a lane leading towards the wall, about half-way between the Ch'ien gate and the Ha-ta.

They discovered that the portion of the wall near the Legation was held by foreigners, and three flags—the Russian, the British and the American—were flying together on it, the British in the centre. This was a pre-arranged signal, communicated to General Gaselee at Tung-chow, by which he was to understand that the Legations were still holding out.

They were signalled to come up by the Sluice—an arched outlet through the wall, and leading directly to the Legations. Hardly a shot was fired at them, except by snipers inside mud-houses along the road.

They were signalled by the besieged that the wall was clear of Chinese and was in the hands of the Legation guards. Taking advantage of this, a company of the 7th Rajiput Infantry forced a passage through the wall by the Sluice. Helped to no mean extent by besieged Chinese Christians and a couple of Europeans from inside, they cut an opening in the rotten wooden gate.

Some forty Rajiputs, followed later by a handful of British, made an entry, cheered by the crowd of Christian converts who had come to meet them, and soon after greeted by the frantic hurrahs of the white men, women and children, awaiting them with open arms at the gate of the Legation—only a few yards further up on the left side of the canal.

The Legation was triumphantly entered.

Considerable excitement prevailed when the Rajiputs, waving their rifles, rushed into the British Legation.

The enthusiasm reached its maximum when General Gaselee and the 24th Punjab Infantry came in, and then the 1st Sikhs and the Bengal Lancers and the 9th and 14th American Infantry, who had come unopposed along the Tartar wall by the Ha-ta gate.

In Pekin there are four cities in one—the Chinese, the Tartar the Imperial city and the Violet or Red Forbidden city.

In the very centre of Pekin, and within the wall of the Imperial city, stands the "Forbidden City," containing the Emperor's audience halls, his private palaces, and those for the court, officials and attendants.

The Allied forces had entered the Chinese and Tartar city, and captured the Imperial city; they had destroyed, burned and looted wholesale the houses of friends and foes alike; but the Forbidden

city, with all its palaces—that nest of infamy and corruption, where the plans for the massacre of foreigners in China had been conceived and organised—was respected and protected.

Russians, British, Japanese, Americans and French were left to guard each of the four gates, but for two whole weeks—until August 28th—the Forbidden city was left untouched and closed. The result?

The Chinese naturally concluded that we were afraid to take it, that we felt our inferiority, and therefore did not dare trespass on this sacred ground. Rumours spread that foreign devils were not strong enough to take the palace; that when we had reached the gates we were unable to go further. Hence, within the ten days after our entry into Pekin, several American, French and Russian soldiers were murdered by Chinese, while peaceably going about the streets.

The Boxers in the neighbourhood of Pekin, encouraged by what they believed to be our lack of courage and strength, again became threatening.

It was then decided by the Foreign Representatives, that a suitable way of maintaining our prestige and of giving China a lesson would be to make a formal entry into the Forbidden city, and that the entry should take the form of an International procession. Only one small detachment of each Power would be allowed to march through the Forbidden grounds, each nation to be represented by a number of men in proportion to the number of troops despatched for the relief of Pekin.

I immediately called on General Barrow (British) to obtain permission to accompany the British troops on this triumphant march. The general was not approachable, but referred me to an officer, who said that permits would be issued to no British subjects. A great mystery was made about the whole affair, and it seemed that it was the intention of the authorities to have the whole performance "on the sly," as it were.

The American general was no better than the British, and when interviewed by an American correspondent is reported to have struck a stage attitude and exclaimed: "There are things in this world that are sacred! The Imperial Palace is one of them!" He would see non-military men anywhere rather than let them into the palace to see the show.

Personally, fortune attended me. An invitation was given me to ride into the palace with General Linievitch, who, being the senior general, would be the first foreigner to enter the Forbidden city.

"Be here at 7.30 to-morrow morning, for I shall start at 8 to review the International troops," said General Linievitch, as I thanked him for his kindness.

When morning came I rode briskly to the Russian Legation, reaching it an hour, or even two, before the time appointed.

I was cordially received by the general and his staff, and we eventually started on our horses. We entered the first courtyard.

Here the troops that were to march through the palace were already in line. The various generals were on foot at the head of their respective columns, and as we rode past each contingent the *doyen* general reviewed them, each general and staff in turn saluting as we passed, and accompanying us to the end of his line.

As far as numbers went, the Russians were most prominent of all, then the Japanese, the British, the Americans, the French, the Germans, the Italians and the Austrians.

The review of the troops completed, General Linievitch, his staff, and myself, with an escort of Cossacks, rode up through the second courtyard, followed by the diplomatic body, the Russian marines, Russian line officers and infantry.

We passed through the two gates of the approaches to the palace, which had already been opened by the Americans, and were now before the last gate, that leading into the Forbidden city itself, upon which no foot of "foreign devil" had ever trespassed. The moment was impressive.

Three Chinese officials—two of them interpreters to the Yamên—in their long blue State robes and white hats, stood with mournful faces waiting for us at the closed gate. As we approached, they stooped and chin-chinned, joining their thumbs together.

General Linievitch and I dismounted, and walked up the incline to the gate.

Opened from inside, the huge wooden doors, studded with iron knobs, revolved slowly on their rusty hinges.

General Linievitch and I stepped into the Forbidden city. The British Artillery fired the first salute of one-and-twenty guns.

"Entrez dans le Palais de l'Empereur!" said to the general the Chinese official, Lien-fang, who spoke the most perfect French; and, having shown us the way with an extended arm and a grand bow, he joined his companion ahead of us.

They marched quickly, evidently in a hurry to get us through in all speed.

Behind us we had General Linievitch's staff, including the brave Munthe and Yanchevetsky.

Then came the third Chinaman, who kept with the diplomatic body—an extraordinary-looking set, dressed up in the quaintest of costumes, most of them hardly adapted to so grand an occasion.

In front was prominent the bony figure of Sir Claude MacDonald, in an ample grey suit of tennis clothes, and a rakish Panama slouch hat, which he wore at a dangerous angle.

To his right the Russian Minister seemed quite reposeful by contrast. He was clad in dark clothes, and bore himself with dignity.

Next to him came the Representative of the French Republic, in a garb that combined the requirements of the Bois de Boulogne on a Sunday with the conveniences of tropical attire on a week-day.

Mr. Conger, the American Minister, strode ponderously behind,

dressed in white cottons and military gaiters, while a horde of secretaries, students and interpreters, in various fancy garbs, made part of the distinguished crowd.

The march through the palace being a military affair, it seemed as if the Ministers were sulky, and attached no importance whatever to the occasion. In fact, some appeared quite bored.

The remarkable head of Dr. Morrison (correspondent of the London *Times*) could be seen among the crowd.

The chief buildings and the Emperor's audience halls occupied the central part of the palace grounds in a line. The troops were not made to march through these throne-halls or other buildings, but we skirted them to the right, through various courts of more or less magnificence, passing buildings of great age and out of repair, and going through quaint rock gardens, until we came to a centenarian tree of gigantic proportions. Its branches were so heavy that they had to be supported by strong beams. Beyond that, through delightfully artistic grounds, was to be found a hill with curious grottos. In the grottos were idols and statues of Buddha, besides a number of silver and jade cups, vases, images, candlesticks and bowls.

Each of the courts or passages through which we went had massive gates that were opened by attendants as we approached. These attendants, who had been besieged in the palace, seemed famished and worn. Although apparently submissive, even servile, any observant person could notice on their stolid faces an expression of hatred and contempt for us as we went by.

We reached the last and most northern court of the palace, and here came the most impressive part of the procession.

The Russian general with his staff, the diplomatic body, and Lady MacDonald, stood under the northern gate leading out of the palace.

The Russian band, an excellent one, took up a position in the courtyard, while a strong force of Cossacks and Russian infantry stood under the gate and round the wall of the court. Presently the *défilé* before the Russian general began, and with the first bars of the Russian National Anthem in marched the Russian marines, infantry, and a contingent from each of their regiments in Pekin.

A finer, healthier, sturdier, better or more sensibly-drilled or clad body of soldiers it is impossible to imagine. They marched manfully across the court amid the "hurrahs" and wild excitement of all present. Some marched out of the palace, but a number of them were ordered by the general to remain inside the court. The object of this was a charming politeness on the part of the Russians towards their Allies. The soldiers had been ordered to cheer themselves hoarse as the contingent of each nation went by.

Next to the Russians the Japanese marched through—these wonderful and absolutely perfect soldiers and officers who have astounded the world by their bravery. In their white uniforms and black and yellow caps, perfectly equipped, they marched slowly and sensibly—a delightful display of precision and neatness. Baron

Yamaguchi, their commander-in-chief, and General Fukushima, with their staff, marched proudly at their head, taking a place next to the Russian general when they reached him.

The soldiers marched by the sound of their own bugles, as, alas, the Russian band, excellent as it was, broke down when it came to play a Japanese air! The absence of what might have turned out an ear-rending performance was, however, more than compensated by the enthusiastic and prolonged cheering of the Russians, which was much appreciated by the Japanese, and which continued until the last of them was out of sight.

And now for the British. One could not help being struck by the fact that they wore better clothes than any other nation—everything made of the best material instead of the cheapest. They looked as smart and spick and span as if they had come out of a bandbox, the officers especially. They had a dashing, free and easy, and extremely manly and business-like way of marching along, with a graceful, unaffected swing that one could not help liking. You could see as they came along that they were men who belonged to a great nation. They knew it, and were proud of it.

As General Gaselee and his staff appeared in the courtyard the Russian band played " God Save the Queen," and the most frantic hurrahs and waving of hats and caps took place as the Marines and Welsh Fusiliers marched by.

In now came the Sikhs, with their bagpipes, which they seemed to play as well as any Scotchman. The pipers remained in the courtyard, playing as delightfully as Scotch tunes permit, until the 1st Bengal Lancers, the 7th Rajiputs, the Pathans, and at last the Wei-hai-wei Regiment, all passed through, all received with thundering cheers, moderated slightly towards the Chinese regiment, for it seemed to go against the grain, even with the Allies, that Chinamen should have been sent to fight against Chinamen. One felt rather sorry at their position, for as a regiment they were a wonderful body of men.

The Americans were ushered in with " The Star-Spangled Banner" blown through the brass instruments by powerful Russian lungs. The soldiers looked smart as they came through the gate. Officers and men were in khaki, except General Chaffee, who wore his blue uniform.

They, too, like the British, were most enthusiastically cheered, and they deserved it, for indeed they had done exellent work in the campaign.

On this particular occasion, when one could contrast and compare them with other nationalities, one was particularly struck by the individually intelligent appearance of each man, and by the matter-of-fact mien of the line officers. At the same time they presented quite as good a military appearance as soldiers of any other nation. The boys marched through with pride, and waved their flag as cheers were raised for them.

The German contingent came next. Splendid men, tall, heavy,

machine-like, and all so perfectly alike in height, build and shape, that they seemed made in the same mould. The contrast between them and the natural, easy-going Americans was great. It was so great that when they came in with their extraordinary parade march—as unnatural a way of locomotion as was ever invented—there was a general semi-suppressed laughter, drowned at once in "hurrahs." One could not but admire the way they were drilled, their training being absolutely perfect—that is, if a soldier who is a machine is to be taken as a model soldier.

If one received a mental shock at the contrast of the American and German warriors, we had now a greater one when the French marched in.

The poor fellows seemed so exhausted that they could scarcely walk. Their uniforms were in a dreadful state. They hardly showed the French army at its best—but how could they? These men had been for several years in the murderous climate of Saigon, whence they had been despatched to Pekin, where they had just arrived. It is to be regretted that France was not better represented, for we all knew she could easily have made a better show.

A curious incident, noticed by few, happened. The Russian band had been playing, at the full power of their lungs, the 'Marseillaise,' the Republican march of France, but a forbidden air in the monarchical neighbouring country of Italy.

As the French were meagrely represented, the Italians came immediately behind them, just as the 'Marseillaise' was in full swing. The Russian general discovered the *faux pas* at once, and tried in vain to signal the bandmaster to stop. The musicians were blowing their hardest when the general's aide-de-camp was despatched across the line to them. Just in time. In a hurry-scurry fashion the Republican march ceased abruptly, and the 'Inno Reale' of Italy was struck up, much to the reassurance and relief of the Italians, who seemed perplexed to march under an air foreign and distasteful to their ears.

They looked very manly and neat, well drilled, and carried themselves splendidly. They were much admired and cheered.

So was the small and last contingent—the Austrian Marines—which worthily ended this marvellous International parade through the Forbidden City.

[A. H. S. L.]

GENERAL MONTHLY MEETING,

Monday, June 3, 1901.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Henry J. Wood, Esq.
John T. Middlemore, Esq. M.P.

were elected Members of the Royal Institution.

The following Address to the University of Glasgow was read and adopted:—

The Royal Institution of Great Britain, which celebrated its Centenary two years ago, desires to offer to the University of Glasgow its congratulations on the completion of the Four Hundred and Fiftieth Year of its illustrious history.

Dedicated as the Royal Institution of Great Britain is to the diffusion of knowledge, and to teaching the application of science to the common purposes of life, it recognises with sympathy and admiration the great work done by the University of Glasgow in maintaining and extending the higher education in Scotland, and so in spreading a knowledge of Science, the Arts and Letters, throughout the world: for there is no shore touched by the commerce of the great and enterprising city of Glasgow where the influence of that seat of learning, which is its chief ornament, has not been felt.

The Royal Institution of Great Britain recalls with special interest that it was in the University of Glasgow that originated that distinctive Scottish Philosophy with which the names of Hutcheson and Reid must always be conspicuously associated, which, discarding mystical speculations, appealed to common sense and inductive methods, and thus carried into the sphere of mental phenomena the principles which Bacon had introduced into physical inquiry, and which has had a powerful practical bearing on national life and character. It recalls also with profound interest that it was in the University of Glasgow that Joseph Black made his discovery of latent heat, and laid the foundations of quantitative analysis; that James Watt nursed that mechanical genius that has remodelled our civilisation and transfigured the face of the earth; and that Adam Smith, in an epoch-marking work, expounded those economical laws by which the body politic is governed.

The Royal Institution of Great Britain, at the time of its foundation in 1799, drew its first Professor of Natural Philosophy, Dr. Garnett, from Glasgow, and since then a long succession of Professors and Graduates of the University of Glasgow have lectured in its Theatre on scientific and literary subjects. Lord Kelvin, while a Professor in the University of Glasgow, lectured at the Royal Institution to the edification and delight of its Members fifteen times; and of the Professoriate of Glasgow, one Member, Professor McKendrick, has held the office of Fullerian Professor of Physiology in the Royal Institution, while two others, Professors Raleigh and Gray, have contributed to its Friday Evening Discourses.

The University of Glasgow has in a gratifying manner recognised the value of the work now being done in the Laboratories of the Royal Institution by conferring an Honorary Degree on Professor Dewar—no unworthy successor of Davy, Faraday and Tyndall—whose achievements have reflected honour on the Institution which has afforded him facilities for carrying on his memorable researches.

The links of common aims and mutual obligation uniting the Royal Institution of Great Britain with the University of Glasgow are numerous and strong, and it is therefore with feelings of the utmost cordiality that the Royal Institution greets the University at this notable moment of its voyage, and expresses the hope that, unenfeebled by affluence in the future as it has been undaunted by obstacles in the past, it will strenuously and successfully pursue its beneficent career.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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WEEKLY EVENING MEETING,

Friday, June 7, 1901.

His Grace The Duke of Northumberland, K.G. D.C.L. F.R.S., President, in the Chair.

Professor Raphael Meldola, F.R.S. *M.R.I.*

Mimetic Insects.

The Lecturer commenced by describing the production of colour among insects by selective absorption due to pigments and by purely physical structure causing interference. Images of butterflies (*Callidryas* and *Morpho*) illustrating the two classes of colouring were thrown on the screen. The subject of insect coloration was not considered in relation to physical, chemical, or physiological causes, but rather with reference to its bionomic significance, i.e. its value so far as it concerned the welfare of the species. It had long been familiar to naturalists that the colour and pattern, form and habit were often adaptive. This adaptation to environment, apart from any hypothesis, is one of the most marvellous facts presented by living organisms when examined in detail. The effect of the adaptation is *concealment*, whether for the purpose of enabling the insects to escape from their foes, or in order to enable them to secure food by approaching their prey undetected. It was convenient to distinguish between *protective* and *aggressive* resemblance, although in all cases it was not possible to decide off-hand without a full knowledge of life-history to which class any particular case should be referred.

Illustrations showing the harmony of colour, pattern and form between insects and their surroundings were shown on the screen, many examples being taken from native species at different stages in their life-histories. The explanation of the adaptations considered was referred to the action of natural selection, the advantage of concealment in such cases being sufficiently obvious to warrant the application of the principles of selection as laid down by Darwin and Wallace.

The Lecturer next proceeded to the consideration of those cases in which the colours and markings were conspicuous, and no attempt at concealment was made. These were explained also by the principles of selection as applied by Wallace in his well known theory of "warning colours." With these two classes of facts, colour for concealment and colour for warning, it was easy to carry the mind forward to true mimetic resemblances in which the species

instead of resembling its inanimate or vegetable environment, bore an external resemblance to some other species, the latter often belonging to a quite different order. The association of protective and aggressive resemblance with mimicry as all coming under the domain of the Darwinian theory, was first suggested in 1861 by the late Henry Walter Bates, who was soon followed and supported by Wallace and Roland Trimen, these naturalists having observed similar cases among the butterflies of the Eastern tropics and South Africa respectively.

Beginning with the extreme cases in which insects belonging to different orders resembled each other, illustrations were shown in which a moth (*Ægeria bembeciformis*) mimics a hornet (England); a moth (*Scoliomima*) and a beetle (*Nothopeus*) mimic wasps (N. Borneo); flies (*Hyperechia*) mimic bees (*Xylocopa*; Mashonaland and Borneo), and a remarkable case in which a bug mimics a leaf-bearing ant. The groups of mimetic butterflies observed by Bates in the Amazon Valley and which suggested the first scientific explanation of the phenomenon were shown on the screen, the mimicry being between *Pierinæ* (*Leptalis*) and *Heliconinæ* (*Ithomia*, *Mechanitis* and *Methona*). The departure in type on the part of the mimicking Leptalids was strikingly shown by comparison with the Brazilian *Leptalis nehemia* which retained the normal type of colour and pattern. The more complex case recorded by Trimen from South Africa, in which a *Papilio* (*P. cenea*) retains the normal colour and pattern in the male while the female presents four different forms mimicking respectively *Amauris albimaculata*, *A. echeria*, *A. niavius* and *Danais chrysippus*, was also shown.

The Batesian theory was based on the supposition, well supported by evidence, that the imitated forms were more or less exempt from persecution by insect enemies, by virtue of the unpleasant taste or smell conferred by the acrid juices contained in them. Their gaudy colouring and disregard of concealment thus brought them under the category of "warning colours," and the advantage derivable from a resemblance to such species was sufficient in this case to warrant the application of the theory of selection. The application of the Darwinian theory by Bates carried with it the implications that the imitating forms or mimics were devoid of distasteful qualities, that they were fewer in individuals and that they inhabited the same districts and associated with their models. As far as the theory could be tested by the evidence available at the time of its promulgation it appeared to furnish a satisfactory explanation of the phenomenon. But facts began to accumulate—and Bates himself was among the first to call attention to this point—which seemed to indicate that there must be some other influence at work bringing about mimetic resemblances between the protected species themselves. In other words, the Batesian theory, although applicable to such cases as the mimicry between Leptalids and Heliconids, did not explain the resemblances among the Heliconids themselves. (A group was shown illustrating some of the resemblances between distasteful species of *Ituna*, *Methona*

and *Thyridia* together with the *Dismorphias* and moths mimicking these, the group being selected from Bates's original examples.)

The first step towards the necessary extension of the theory of mimicry was taken by Fritz Müller in 1879. He showed how the principle of selection could be applied also to these cases if it is admitted, as the evidence warrants, that the so-called "protected" groups are only relatively and not absolutely exempt from persecution. In other words, it is considered that young birds and other insect-eaters do not inherit a knowledge of distasteful forms, but have to acquire their knowledge by individual experience at the expense of a certain amount of sacrifice of life by such distasteful forms. Fritz Müller, who had been a keen observer in Brazil for many years, had frequently noticed that the requirements of the Batesian theory were not always complied with. In some cases the mimics appeared to be more numerous than their models; in other cases both mimic and model belonged to protected groups, and he finally suggested the above explanation with special reference to *Ituna* and *Thyridia*, which, although not so closely related genetically as Müller supposed at the time, are undoubted mimics belonging to protected genera. The fundamental requirement of the Müllerian theory, that even protected species have to suffer a certain amount of persecution, was supported by the observation that butterflies belonging to such groups were frequently caught with mangled wings, indicating an attack by birds. From the psychological side, it has since been proved by the observations of Lloyd Morgan on newly hatched birds, that the requirements of the theory are quite in harmony with the facts.

The advantage conferred upon protected species by bearing a superficial resemblance to each other is capable of being expressed (as Müller did express it) algebraically, and is sufficiently explained for ordinary purposes by the general statement, that if a number of individuals belonging to different species have to be sacrificed, the larger the number of species which resemble each other the smaller the number of sacrifices undergone by each species relatively to its whole number of individuals. Thus, while the Batesian theory might be considered an extension of the theory of ordinary protecive resemblance to cases in which the imitated object is another living organism, the Müllerian theory might be considered an extension of the theory of warning colours from individual species to groups of species ("common warning colours" of Poulton).

On the principles indicated, it becomes intelligible why, among protected groups, there should so frequently prevail a general uniformity of type in colour and pattern. It is in such cases no longer an individual type that insect enemies have by experience to learn to avoid, but a generalised type of colour and marking, common to whole groups. In illustration of this point, the Lecturer showed a group of the British Vanessids in which there is extreme dissimilarity of colour and pattern on the upper surface of the wings, there being in this case no particular advantage conferred by a superficial re-

semblance to each other. On the other hand, there were shown the following groups illustrating the general resemblances between the species of protected genera:—

Three genera of *Euplœinæ* from Malabar and Nilgiris, the mimetic resemblance between which had been recognised by F. Moore.

Five species of *Acræa* from Mashonaland, and another group of two species caught in one day by Mr. Guy A. K. Marshall.

A large group of butterflies belonging to different Heliconine and Danaine genera, and some moths all converging in general pattern round *Thyridia* and *Methona*.

A group from Guiana showing resemblances between *Danainæ*, *Erycinidæ* and certain *Pierinæ* (possibly a protected group) and between *Ithomiinæ* and *Heliconinæ*. The chief interest of this group was the prevalence of a local peculiarity of colouring, viz. a darkening of the colours of the hind-wing which was apparent in all the families and sub-families furnishing genera for the Müllerian group.

Pairs of *Melinæa* and *Heliconius* and of *Tithorea* and *Heliconius* from various parts of Central America, these pairs being selected for their close resemblance and as being centres of convergence for other Müllerian groups.

A group of specimens of *Anosia plexippus* from various parts of North America, together with the mimicking *Limenitis archippus* and another species of *Limenitis* preserving the normal type of colour and pattern of the genus, in order to show how great had been the divergence from the normal type.

A group showing Müllerian mimicry between the female *Hypolimnas bolina*, *Papilio castor*, *P. panope*, and the Danaine *Podemma kollari* (both sexes) and *Crastis core* (both sexes), from the western Ghats, taken by Mr. G. Keatinge.

The Müllerian principle of mimicry was thus well illustrated by the Lepidoptera both in its broad application to the explanation of general resemblance among protected groups as well as to the more special resemblances between local forms. Although observations concerning other orders of insects were not so numerous as in the case of the Lepidoptera, there was already good evidence of Müllerian associations among such orders. In illustration of this point a group of Aculeate Hymenoptera from Western Australia was shown, a general similarity of colour and marking being observable among species of *Abispa*, *Eumenes*, *Alastor*, *Odynerus* and *Bembex*. A still more remarkable Müllerian group comprising insects of various orders captured in Mashonaland by Mr. G. A. K. Marshall in January 1899, was also shown. The general similarity in this case extended to Coleoptera (*Lycidæ*, *Telephoridæ*, *Cantharidæ*, *Melytidæ*, *Phytophaga*, *Cerambycidæ*), Hymenoptera (*Aculeata*, 3 genera), Hemiptera (*Reduvius*), Lepidoptera (*Zygænidæ*, *Arctiadæ*), and Diptera. It was perhaps not possible at present to refer each particular case of mimicry to its proper category, Batesian or Müllerian. But if a group round which in some district a number of other groups

converged was in other districts the model imitated, it might fairly be assumed that the group in question was protected and that the mimicry was in such a case Müllerian. Thus among Coleoptera the *Lycidæ*, which appeared as it were to set the fashion in the group exhibited, were in other districts the subjects of imitation and so were probably more or less "protected."

The Lecturer in conclusion dwelt upon the objections that are sometimes urged against birds, lizards, &c. being the selecting agents. He considered that the difficulty in this case was of the same nature as that which attends the theory of selection by natural agencies in general. Although it is certain that a large amount of extermination is constantly going on in nature, in scarcely any case can it be said that the actual agent or agents have been observed or the process been seen in operation. Four groups of butterflies caught at large with damaged wings were shown on the screen, viz. a group of species of various genera showing indiscriminate attack, a group showing attack at the tips of the forewings, and two groups showing notches on the hindwings. One of the latter, a group of *Lycænidæ*, was particularly interesting as illustrating the deceptive adaptation of the tailed prolongation of the hind-wing with the eye-like spot above it, so as to resemble the head end of the insect with eyes and antennæ. This observation, which had been recorded before, had recently been confirmed quite independently by Mr. Champion Russell in a South African Lycænid, and the frequent capture of these butterflies with wings notched just about the eye-like spot lent support to the view that the deception was successful against the attacks of birds, the insect escaping vital injury by the sacrifice of a small portion of the wing.

One example was shown illustrating the difficulty of detecting selecting agents at work even where experimental conditions had been imposed which undoubtedly favoured selective extermination. In the course of a series of experiments by Professor Poulton having for their object the testing of the relative immunity of the variously coloured pupæ of *Vanessa urticæ* according as they were exposed on surfaces with which their colours harmonised or not, it was found that a large percentage of individuals out of harmony with their surroundings was picked off by some foe, most probably a bird, leaving only the tail attached to indicate where the chrysalis had been suspended. Although hundreds of pupæ had been removed in this way, the most persistent observation had failed to reveal the enemy which removed them; on one occasion only did the observers suspect that a bird was at work, but the tree on which the pupæ were exposed was too far off to be sure of the species.

The Lecturer expressed his thanks to those who had helped to furnish illustrations; to Dr. Alfred Russel Wallace for the loan of slides and to Professor Poulton for slides and for the free use of the materials in the Hope Museum. All the groups illustrating the later developments of the theory of mimicry had been arranged by Professor

Poulton at Oxford and photographed by Mr. Sanger Shepherd by his three-colour process with excellent results. Specimens illustrative of the subject of the lecture had also been grouped by Professor Charles Stewart for the Museum of the Royal College of Surgeons, and by the permission of the President and Council of the College many of these had been placed for exhibition on the lecture table and in the library.

[R. M.]

HODGKINS TRUST.

Essay by Miss Agnes M. Clerke.

Low-Temperature Research at the Royal Institution, 1893–1900.

Early in 1895 the late Mr. Thomas G. Hodgkins presented to the Royal Institution a sum of 100,000 dollars,* as a source of income to be employed in the "investigation of the relations and co-relations existing between man and his Creator." On the ensuing 6th of February the Managers resolved to carry out the intentions of the donor, by devoting the resources thus placed at their disposal to the work of the Institution, which, having for its aim the attainment of truth, constitutes an effective means of "directing thought to the source of all knowledge."

"In der Schöpfung," Kepler wrote, "greife ich Gott, gleichsam mit Händen"; and Faraday reflected, "with wondering awe," on the powers of interrogating nature "given by the Almighty to man." These great men were no mere transcendentalists. The lofty aspect under which they viewed physical research may indeed be temporarily forgotten, but cannot permanently be lost sight of, for it corresponds to an invincible human instinct. Law formulates intelligent purpose; and the laws of nature are an expression of the Will of God. In tracing them out, man seeks, more or less consciously, the infinite; and his capability of tracing them is derived solely from the analogy of his mind with the Creative Mind, which designed a universe assumed by the necessities of thought to be a "cosmos"—an orderly arrangement, such as his faculties can apprehend. Were it unplanned, or planned according to a method incomprehensible by human reason, science would have no *locus standi*: life should be conducted on purely empirical principles. As a fact, however, we find the world essentially intelligible; and, by striving to enlarge the limits of its intelligibility, we promote the purpose of the Creator in placing us there, and, following in the track of His primal conceptions, bring our inchoate ideas more and more into harmony with them. The object of the present paper is to show what has been done towards this end at the Royal Institution, during the last seven years, under the terms of the Hodgkins Trust.

Research at low temperatures has long been recognised as the characteristic task of the establishment. Resuming, after the lapse of a third of a century, the traditions of Davy and Faraday, Professor Dewar gave a new stamp to his operations by the enlargement of

* Investment in Consols amounts to 17,986*l.* 1*s.*

their scale. They have been conducted, not for the mere purpose of scoring experimental successes by liquefying intractable gases, but with a view to extensive and profound investigations of the properties of matter under conditions previously unattainable. The realisation of these conditions, however, demands a profuse outlay of labour, time, and money, to say nothing of the risks incurred through the rebellion of natural forces against rigid mechanical constraint. Hence, a colossal equipment, disposed of by an individual of untiring energy, united to courage and inventiveness of no common order, was indispensable for the furtherance of this important enterprise; and nowhere has the combination been rendered so thoroughly effective as in the laboratory of the Royal Institution. Its effectiveness, however, has not been due to the generosity of one benefactor alone. Many have contributed to it. The munificence of the Goldsmiths' Company has twice brought timely relief in financial straits; the donations of private individuals have furnished indispensable supplies, and merit emphatic and grateful acknowledgment. The coincidence is noteworthy that the only considerable sum received by the Royal Institution for the endowment of research has come from a fellow-countryman of its versatile and far-seeing founder. What Count Rumford had begun Mr. Hodgkins designedly continued, and by sharing his subsidies between the Royal Institution of Great Britain and the Smithsonian Institution of Washington, he evidently proposed to invite the cordial co-operation of the two great English-speaking peoples for the investigation of those subjects in which he was so deeply interested.

The stage of the campaign arrived at when the Hodgkins fund became available for its prosecution may be described in a few words. All the known gases, except hydrogen and fluorine, had been reduced to liquids in a statical condition, and of these, liquid oxygen alone had refused to solidify when evaporated under diminished pressure. A point of cold had been reached only 73° C. above the absolute zero (− 273°), and the altered electrical and chemical relations of various substances cooled to − 182° had been investigated. The observed progressive decrease, with increase of cold, in the electrical resistance of pure metals, seemed to betoken its total disappearance near the absolute zero, while alloys lost little of their resistivity, and carbon followed an inverse course of change. The curious individualities, in this respect, of different metals were also noted. Thus, the resistivity of iron is reduced to one twenty-third, that of copper only to one-eleventh, by lowering its temperature from + 100° to − 197°, at which point an iron wire actually conducts better than one of the finest copper when ordinarily warm. In these inquiries Professor J. A. Fleming co-operated with Professor Dewar. On December 10, 1891, the magnetic quality of liquid oxygen was discovered by Professor Dewar. Other well-known properties of the gas proved to be equally persistent after condensation. Liquid, like gaseous oxygen, is a bad conductor of heat and electricity, while

transparent to thermal radiations. Its absorption-spectrum, too, is virtually the same as that of the gas; so that it could be inferred that the molecular constitution of the element was little affected by change of state.

The practical difficulties impeding the preservation and observation of frigid liquids were, in great measure, overcome by Professor Dewar's invention of vacuum-coated vessels for storing them. By their means the access of heat, whether by convection or by radiation, was so effectually checked that evaporation shrank at once to one-fifth its former amount, and the deposition of a thin film of mercury on the surface of the containing glass still further lessened the waste. Added refinements of construction brought it down to a mere fraction of what it had been in unprotected receptacles, and the durability of volatile fluids thus obtained a thirty-fold extension. Tranquillity was besides given to them, ebullition ceased, and they could be manipulated with ease. This essential improvement was effected late in 1892. Thus, at the beginning of the septennial period we have now to consider, a steady and continuous advance was in progress, and much new territory had been annexed and explored. A tract, it is true, lay beyond, not wide, but most arduous to penetrate; yet there was a hope of its thorough conquest through the amelioration of methods and the experience acquired in their application.

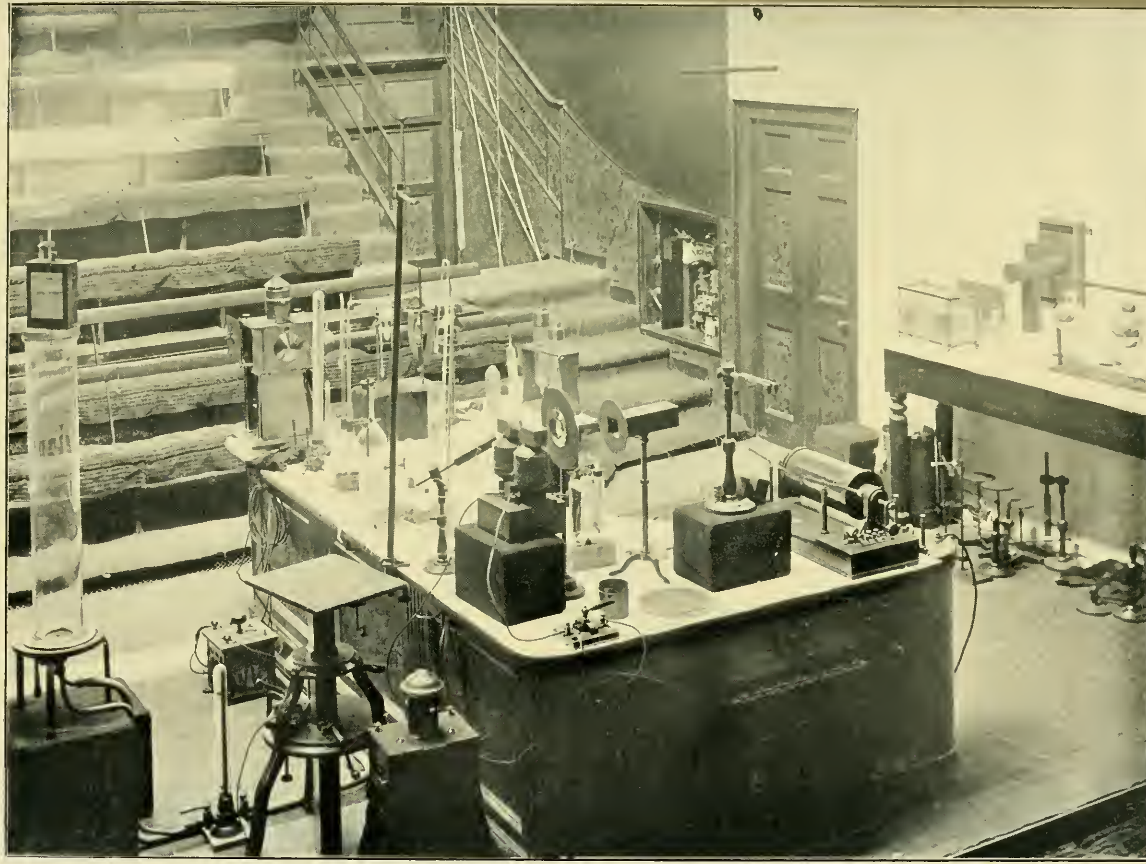
Liquefaction of Hydrogen.

The main outstanding problem was the condensation of hydrogen. This is the lightest of known substances, yet it resembles a metal in being strongly electro-positive; in being a conductor of heat and electricity; and in forming, with palladium, sodium and potassium, compounds possessing some of the properties of alloys. Upon these grounds Faraday based the forecast that solid hydrogen would show metallic lustre. Its liquefaction was first demonstrated by M. Wroblewski, of Cracow, in January 1884. A froth of hydrogen became momentarily visible when the gas, cooled to the temperature of nitrogen boiling *in vacuo*, was suddenly released from a pressure of 180 atmospheres. This evasive appearance was reproduced by M. Olszewski; but neither experimenter succeeded in obtaining the liquid in tangible form. This was accomplished at the Royal Institution, as the outcome of a long series of efforts, frequently baffled, and persistently renewed. The conditions that had to be met were approximately known through Wroblewski's determination, by the aid of Van der Waal's formula, of the critical constants of hydrogen. He fixed the temperature *sine quâ non* (as it may be called) at $-240°$ C., the corresponding pressure at 13 atmospheres, and the boiling point at $-250°$. With these conditions, Professor Dewar made in 1894 a preliminary attempt to grapple, by mixing a small percentage of air or nitrogen with hydrogen, and thus producing an artificial gas capable of liquefaction by the use of liquid air. This

gaseous blend, subjected to powerful pressure at a temperature of $-200°$ C., and then permitted to expand, gave rise to a degree of cold below any previously attained, and there resulted a deposit of solid air, together with a clear liquid of small density, too volatile for collection by any available device. This was, doubtless, the first sample of genuinely liquefied hydrogen ever exhibited to view.

Professor Dewar's object, however, was not merely to catch sight of liquid hydrogen, but to get hold of it. Enclosed in a glass tube, under pressure, it still remained comparatively inaccessible. Until it could be made to accumulate, at its boiling-point, in open vacuum-vessels, no satisfactory study of its nature was feasible. This goal was finally reached through the introduction of the regenerating coil. The principle of self-intensification had been, in 1857, applied by Siemens to the production of cold. In subsequent years its efficacy in this direction was turned to account for industrial purposes by Coleman, Solvay, Linde and others; while Dr. Kamerlingh Onnes had recourse to it in his cryogenic laboratory at Leiden in 1894. The prompt and copious liquefaction of refractory gases thus became an ordinary operation; but it was only at the Royal Institution that the facilities secured were made to serve for largely widening the scope of research.

In December 1895, Professor Dewar read a paper before the Chemical Society describing the mode of production and use of a liquid hydrogen jet. Owing to the rapid movement of the condensing gas, and the low specific gravity of the resultant liquid, attempts to collect it were fruitless; but, with better isolation and more perfectly adapted vacuum-tubes, their future success was anticipated. Meanwhile, research at some 20° or 30° above absolute zero was already practicable, by using a liquid-hydrogen spray as a cooling agent. Financial difficulties alone stood in the way, and they were not allowed wholly to bar progress. The type of regenerative apparatus employed in 1895 being satisfactory, it was resolved to develop it on a greatly enlarged scale in a liquid-air plant, combining special arrangements for dealing with hydrogen. Its construction took a year, and many months were occupied in testing its capabilities. That they were of the high order required to compel the unconditional surrender of the gas, was at last visibly attested by the dropping of the condensed fluid into a triply-coated vacuum-vessel, May 10, 1898. By a certain dramatic fitness, the first display before an audience of this, so to speak, preternatural substance was at Professor Dewar's lecture in commemoration of the Centenary of the Royal Institution, June 7, 1899. A spheroidal vessel, silvered and vacuum-protected, containing one litre of liquid hydrogen, stood on the table, immersed in a bath of liquid air, for the *savants* of two continents to see. With these precautions, evaporation from it was not inconveniently rapid. The removal, however, of a plug of cotton-wool from the mouth of the receptacle caused an immediate deposition of *air-snow*; and the clogging of



I.—Lecture Table of the Royal Institution. Centenary Commemoration Lecture on Liquid Hydrogen.

tubes with solidified atmospheric air forms a constantly-recurring embarrassment in the use and management of liquid hydrogen.

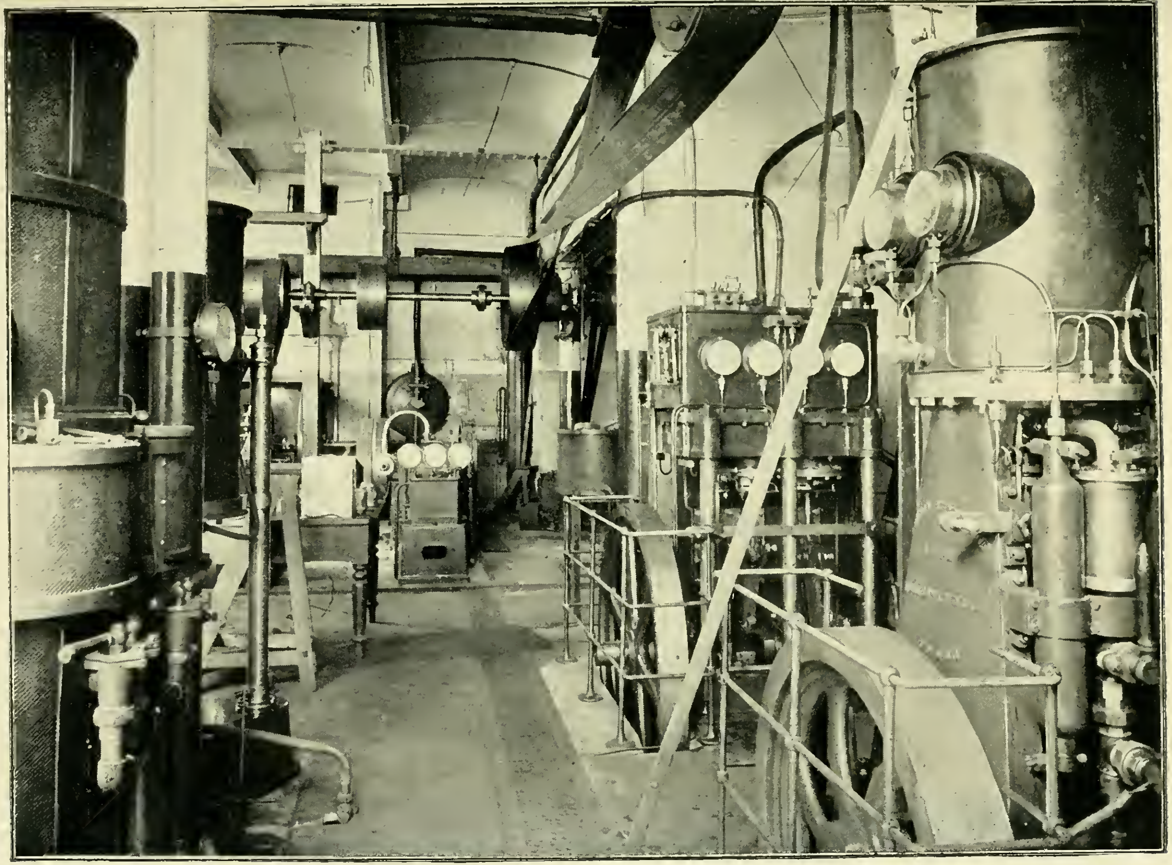
Low-Temperature Thermometers.

This brilliant result, achieved after a long series of discouragements and failures, was only the prelude to fresh enterprises. There is no finality in science. Truth still flies before, enticing its votaries to enter upon ground more and more difficult and broken. This emphatically applies to researches on the production of artificial cold. With every downward step the obstacles become more formidable, the circumstances more critical. Even the exact determination of the temperatures attained is encompassed with difficulties. Ordinary methods of heat-measurement collapse under extraordinary conditions. Hence the choice of a thermometer for fixing the boiling-point of hydrogen was a matter of the nicest delicacy. Those giving changes of temperature in terms of electrical resistance were the most readily available; but the law of their construction, being empirical, could scarcely be expected to hold good far beyond the limit of experience. Recourse was then had to gas-thermometers of the "constant volume" form, filled severally with hydrogen, helium, oxygen, and carbonic acid. The two latter were employed to resolve the doubt whether the rule of equable contraction with cold continued valid down nearly to the boiling-points of the fiduciary substances; and they gave reassuring replies. It appeared that either a simple or a compound gas might be depended upon for the determination of temperatures until liquefaction set in. The results obtained with the hydrogen and helium thermometers might accordingly be accepted with confidence, more especially in view of their close agreement. From them it was concluded that hydrogen boils under atmospheric pressure at $-252 \cdot 5°$ C., or $20 \cdot 5°$ above absolute zero; and that its critical temperature is $-241°$ C., the critical pressure being about 15 atmospheres. Liquid hydrogen shows no metallic affinities. It is a non-conductor of electricity, and freezes into an ice-like solid. Its lightness is extraordinary; water is fourteen times more dense. Perfectly transparent, it gives no absorption-spectrum, and is entirely colourless. Its specific heat, although not very different from that of liquid oxygen when volumes are compared, is twelve times greater for equal weights, and is six times that of water. Its scientific value depends, however, not only upon its exceptional qualities, but upon its power as a frigorific agent. In this capacity, it was rendered serviceable only after some months of severe experience, abnormally cold substances being as troublesome to deal with as abnormally hot ones. The obstacles surmounted in the case of liquid air were presented in an aggravated form by liquid hydrogen, with the further complications due to the solidification of all the ambient air. Its preservation thus offered a twofold problem. Not only the access to it of heat had to be

hindered, but the approach of an universally diffused element. Facts learned by its use are indeed to be reckoned among the *spolia opima* of nature; they represent the trophies of an arduous conflict.

Effects of Refrigeration.

Research into the effects of refrigeration is almost co-extensive with the whole realm of physics. It is concerned with the relations of matter to light, heat, electricity and magnetism. Questions as to the mode of action of cohesive and chemical forces come fully within its scope; nor is it possible to exclude from it the consideration of the intricacies of molecular structure, or even of the essential nature of material substance. These ultimate problems force themselves upon the attention of the least speculative "cryogenist." The nearer absolute zero can be approached, the more hopeful becomes the prospect of their definitive solution; and towards this "pole of cold" Professor Dewar has been pressing his way during two decades. Its actual attainment may perhaps be impossible; but the separating interval must be reduced to a minimum. The ground already won has meanwhile been carefully surveyed. Contemporaneously with unceasing efforts for the liquefaction of hydrogen, experiments were vigorously prosecuted at the temperature of liquid air. Resuming the subject of electrical resistance, Professors Dewar and Fleming carried out in 1893, and several subsequent years, an extensive series of inquiries, with more complete appliances than before for the production of refrigerating material in large quantities, with greater care in the preparation of the metallic wires submitted to trial, and with more delicate precautions in the physical measurements executed. The earlier result was confirmed that the resistivity of all pure metals falls off with increase of cold, but many abnormalities and peculiarities were brought to light. The various metals do not, in all cases, maintain the same relative places on the scale. At $-200°$ C. copper is a better conductor than silver, iron than zinc, aluminium than gold. The electrical eccentricities of bismuth cost the investigators protracted toil. They finally disclosed their origin from minute chemical impurities, by disappearing when electrolytic bismuth was employed. The further discovery was made that the known effect of a magnetic field in augmenting the resisting power of a bismuth wire is greatly intensified at the temperature of liquid air. That of so-called insulators, such as glass, ebonite, guttapercha, and paraffin, likewise gains as heat is subtracted. Alloys follow the opposite rule —that obeyed by pure metals—but in a half-hearted way, and with many perplexing incongruities. When liquid hydrogen was made amenable to control, it became possible to push these inquiries considerably further. At this lower depth of cold, the resistance of copper diminished to $\frac{1}{105}$th its efficacy at the temperature of melting ice, that of gold to $\frac{1}{30}$th, while $\frac{1}{8}$th the initial resistance of iron survived. The general upshot was besides most significant. It had been



II.—General View of Refrigerating Machinery of the Royal Institution.

logically inferred from the behaviour of pure metals down to $-200°$ C. that, at the absolute zero, they would entirely cease to dissipate the energy of an electric current transmitted through them. But at $-252°$ C., marked inconsistencies manifested themselves. Instead of continuing their straight downward course, the resistance-curves bent round, indicating the survival, at 0° absolute, of a finite value for this property. An emphatic warning was thus conveyed against trusting to the continuity of change.

Thermo-electric action had been studied by Professor Tait at temperatures above 0° C.; the modifications produced in it by cooling down to $-200°$ C. were ascertained by Professors Dewar and Fleming in 1895. They present no uniform character. The curves showing the fluctuation with temperature of the thermo-electric power of various metals, do not, in all cases, even approximate to straight lines. Some—notably those of iron and bismuth—show abrupt changes of direction, indicating reversals of the "Thomson effect" at those points. Others are inflected in a manner suggestive of a zero of thermo-electric power at the zero of cold. But these indications are most likely misleading. There is every reason to believe that the rate of diminution of thermo-electric power, as of electric resistance, would fall off notably before that extreme point was reached.

Another set of experiments served to test the influence of cold upon magnetisation. They justified the expectation that magnetic moment would gain strength proportionately to the deprivation of heat. Its value was usually increased, in the fixed state established after some alterations, to the extent of thirty to fifty per cent., by lowering the temperature from $+75°$ C. to $-182°$ C. Exceptions to this rule were, however, met with. A nickel-steel magnet, for instance, is acted upon oppositely to one of carbon-steel. A subordinate result of these experiments was to show that one of the best ways of *ageing* a magnet is to dip it several times into liquid air. This removes sub-permanent magnetism, and induces stable relations favourable to definite observation.

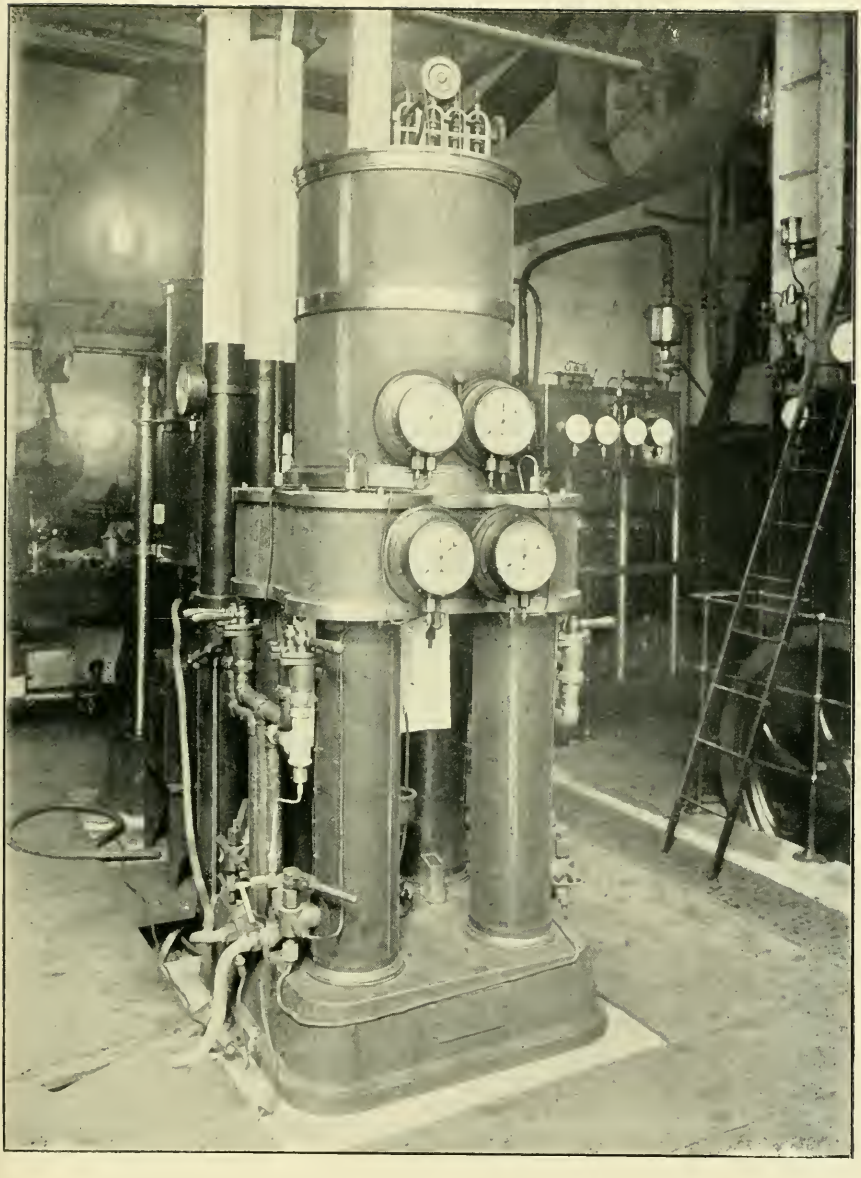
The magnetic permeability of iron over a descending range of temperature was made the subject of long and laborious comparisons. They showed it to be slightly diminished by immersion in liquid oxygen. That is to say, a greater magnetic force was needed to produce a given amount of magnetisation in the cooled material. As usual, however, apparent inconsistencies were recorded. Hardened iron reversed the behaviour of soft iron. Its permeability increased at low temperatures, and, for certain values of the magnetising force, as much as five times. "Hysteresis loss," or the dissipation of energy incidental to a cyclical process of magnetisation, was found, on the other hand, to vary little, if at all, with temperature. These diverse effects were attributed by Professor Fleming to the closer contiguity at extreme cold of the "molecular magnets," from the collineation of which external magnetic moment results. Their groupings and

mutual action might hence undergo modifications, the intricate consequences of which can only in part be divined.

An investigation of the dielectric constants, or specific inductive capacities, of frozen electrolytes was undertaken in 1897. It met with numerous difficulties, but led to some important conclusions. These may be summarised as follows. Such substances as ice and alcohol are capable, at low temperatures, of acting as dielectrics, notwithstanding that some of them possess, in the liquid state, relatively high electrolytic conductivity. They have dielectric constants of large value near their freezing-points, which are greatly reduced by cooling down to $-200°$ C. At the absolute zero, these values are probably equal, all alike being two or three times that of the dielectric constant of a vacuum. Near this point, too, all electrolytes tend to acquire infinite resistivity, or to become perfect non-conductors of electricity. Finally, at very low temperatures, frozen electrolytes are nearly perfect insulators, but they rapidly regain sensible conducting power at temperatures far below their melting-points.

Oxygen and air in the liquid state were inferred from their remarkable insulating quality to be dielectrics; and it was accordingly desirable to ascertain their dielectric constants, in terms of that of a vacuum taken as unity. They came out 1·493 and 1·495 respectively. Between the magnetic susceptibility of gaseous and liquid oxygen a significant difference was elicited. The ratio for equal volumes proved to be 1594 to 1. The magnetic susceptibility, in other words, of the gas was, for equal masses, nearly doubled by liquefaction; whence the inference was drawn that this property does not simply appertain to "the molecule *per se*, but is a function of the state of aggregation." Noteworthy, besides, was the verification for liquid oxygen of Maxwell's law connecting magnetic permeability, specific inductive capacity, and optical refractivity. Additional experiments on liquid oxygen made in 1898, on a different principle from that previously adopted, afforded a qualified confirmation to the law that magnetic susceptibility varies directly as the density of the paramagnetic body, and inversely as its absolute temperature.

So far from showing any tendency to disintegrate into "cosmic dust," matter grows continually more rigid with cooling. A metallic rod will sustain, for the same extension, four or five times the weight at $-182°$ that it would at $0°$ C. A coil of fusible metal wire, which the tension of a single ounce would pull out straight at the ordinary temperature, will support a couple of pounds and vibrate like a steel spring after immersion in liquid air. The most definite means, however, of determining the changes in cohesive force produced by cold, is to compare the breaking stresses of metals at moderate and very low temperatures. The necessary experiments, it is true, involve the expenditure of gallons of frigid and costly fluids; they were, nevertheless, carried out satisfactorily at the Royal Institution in 1893. They showed a large increase with cooling in the tenacity of all common metals and alloys. Exceptions presented by castings of zinc,



III.—Liquid Hydrogen Apparatus of the Royal Institution.

bismuth and antimony, were almost certainly more apparent than real; an explanation of them lay ready to hand in the crystalline structure of these bodies, the internal strains occasioned in which by extreme lowering of temperature, would naturally result in the weakening of some set of cleavage-planes, and comparatively easy rupture. Measurements of the elastic constant known as the "Young modulus," showed that it increased between four and five times by cooling from + 15° C. to − 182°, and balls of iron, tin, lead or ivory, rebounded much higher after the same treatment when dropped from a fixed height upon an iron anvil. The general outcome was to make it clear that cohesion gains effectiveness with added contiguity of the particles acted upon, in this resembling gravitation. Professor Dewar's experiments with liquid air thus lent countenance to Lord Kelvin's view that gravitation is adequate to account for cohesion.

They also disclosed marked alterations, at low temperatures, in the optical properties of certain bodies. Changes of colour, corresponding to changes in the specific absorption of light, were at once obvious. Vermilion and mercuric iodide paled from brilliant scarlet to faint orange; nitrate of uranium and the double chloride of platinum and ammonium turned white—the original hue returning in all cases with the restoration of warmth. Blues, however, remain unaffected by cold, and organic dyes are but slightly sensitive to it.

Temperature has long been known to play an important part in the phenomena of phosphorescence; their study, accordingly, under the intensely frigid conditions realised by Professor Dewar's work in liquefying intractable gases, seemed desirable. It yielded a variety of most interesting facts. On the whole, bodies gain greatly in phosphorescent capability by cooling to − 182° C. Gelatin, celluloid, paraffin, ivory, horn, indiarubber—in all of which the quality is ordinarily inconspicuous—emit a bluish luminosity, on being stimulated by the electric light, after immersion in liquid oxygen. Alkaloids forming fluorescent solutions invariably become phosphorescent at low temperatures. Glycerin, sulphuric, nitric and hydrochloric acids, and strong ammonia, are also very bright, as well as most substances containing a ketone group. Milk is highly phosphorescent, pure water but slightly so. An egg shone as a globe of blue light; and striking effects were obtained from many other organic products—from feathers, cotton-wool, tortoiseshell, paper, leather, linen, sponge, besides some species of white flowers; above all, from white of egg, which, with proper management, became vividly self-luminous. Complexity of structure was inferred to be one of the main conditions upon which the possession of this quality depends. The discovery that it belongs to oxygen alone among simple gases was accordingly unexpected. A current of oxygen flowing into an exhausted tube, after exposure to an electric spark, emits hazy white light, and the attendant formation of ozone attests the simultaneous progress of molecular change. The effect is completely stopped by the presence of hydrogen, or by the least trace of organic matter. At the tempera-

ture of liquid hydrogen, phosphorescent action is still further intensified; it may, although exceptionally, even at $-250°$ C. be set on foot by light deprived of its ultra-violet rays.

The electric stimulation of crystals by cooling brings about actual discharges between the molecules. In some platino-cyanides and in nitrate of uranium, the temperature of liquid air suffices to develop marked electrical and luminous phenomena, which are intensified and extended through the agency of liquid hydrogen. The importance of a systematic study of pyro-electricity under such conditions was pointed out by Professor Dewar in the Bakerian Lecture delivered June 13, 1901.

Chemical affinity is almost completely abolished by cold. Phosphorus, sodium, potassium, remain inert in liquid oxygen; and voltaic combinations, brought down to its temperature, cease to give electric currents. Photographic films, however, retain about one-fifth of their ordinary sensitiveness to light, nor does it wholly disappear even through the agency of liquid hydrogen. Possibly the decomposing force which comes into play under these circumstances is not chemical, but mechanical; if so, no trace of photographic action would be apparent were it possible to carry out development under the frigid conditions of exposure.

An elaborate course of experiments on thermal transparency, carried out in 1897–8, completely negatived Pictet's conclusion that, at a given degree of cold, non-conducting substances lose their faculty of insulation. They were proved to retain it unimpaired at the boiling-point of air, the abnormal transferences of heat observed at Geneva having been due, not so much to the materials themselves, as to the air contained in their interstices. The utility was thus rendered apparent of investigating problems of heat-transmission with the aid of liquid air. The comparative absorption of Röntgen-rays by various frigid bodies formed about the same time a subject of inquiry; and the view that the atomic weight of argon is double its density relative to hydrogen, obtained confirmation from the approximately equal opacities found for that substance in the liquid state, for liquid chlorine, and for potassium. This was the first use of the Röntgen radiation for the purpose of defining atomic weight.

Liquefaction of Fluorine.

The liquefaction of fluorine preceded by one year the liquefaction of hydrogen. That this peculiar substance would prove highly recalcitrant to condensation had long been known by sure indications derived from the character of its compounds. Thus, while chloride of ethyl boils at $+12°$ C., fluoride of ethyl boils only at $-32°$; and similarly, the boiling points of chloride and fluoride of propyl are respectively $+45°$ and $-2°$. Analogous values for the same constant are given by the various inorganic haloid compounds. The obstinate gaseity of fluorine was at length overcome through a com-

bination of forces. M. Moissan, a specialist in the practical chemistry of the element, brought his apparatus for its production to the Royal Institution. for the purposes of his lecture, on May 28, 1897, and it was used next day, in connexion with Professor Dewar's refrigerating plant, to obtain the first specimen of liquid fluorine. This is a clear yellow fluid of great mobility, boiling in open vessels at $-187°$ C., and refusing to solidify at $-210°$. The following are its chief ascertained properties. It is soluble in liquid air and oxygen; its density is 1·14 that of water; it has a capillarity less than that of liquid hydrogen; it gives no absorption-spectrum, and is devoid of magnetic quality. The energetic chemical affinities characteristic of the gas are almost entirely suppressed by the extreme cold needed for its condensation. The liquid rests harmlessly in glass bulbs; it is indifferent to oxygen, water or mercury; only hydrogen and hydrocarbons excite it to reaction with incandescence.

Solid Hydrogen, Oxygen, and Air.

The freezing of atmospheric air was first accomplished by Professor Dewar in 1893. A litre of liquid air, subjected to exhaustion in a silvered vacuum-vessel, yields about half that volume of colourless, transparent solid, which may last as such for half an hour. Under magnetic compulsion, liquid oxygen is drawn to the poles from the meshes of the "nitrogen-jelly," which forms the really solid part of air-ice. This substance can only be examined in a vacuum or in an atmosphere of hydrogen, since it instantly melts in contact with the atmosphere, giving rise, at the same time, to a further liquefaction of air. The interaction of the two processes can be watched, and is curious to discriminate. The difference between the conditions of freezing of oxygen and nitrogen depends upon the circumstance that the vapour-pressure of the former substance, boiling in an exhausted receiver, is inappreciable; that of the latter, quite considerable. Solid oxygen can only be obtained through the agency of liquid hydrogen. It is clear, blue ice. Hydrogen itself was solidified by Professor Dewar, not without much difficulty, in 1899. This final product of refrigeration has a fusing-point of about 15° absolute, at a vapour-pressure of 55 millimetres. It has the appearance of perfectly pure ice; there is nothing metallic about it. The exhibition to a crowded audience in the theatre of the Royal Institution, April 6, 1900, of a form of matter representing so much victorious exertion was a triumph tempered by the reflection that the hydrogen-route towards the absolute zero stopped with its production, leaving a trackless interval of narrow extent, but immense importance.

The era of "new gases" began in 1894, with the isolation of argon; helium was shortly afterwards extracted from clevite and other rare minerals; krypton, neon, and xenon were in 1898 spectroscopically identified as atmospheric constituents by Professor

Ramsay and Dr. Travers, using the method of fractionation at low temperatures. These successive discoveries suggested new and unexpected problems, and offered fresh opportunities for pioneering research. Argon, indeed, condenses with what may now be reckoned as tolerable facility. M. Olszewski reduced a sample of gas sent him by Professor Ramsay in 1895, to a colourless liquid, boiling under atmospheric pressure at − 187° C., and one-and-a-half times denser than water. It freezes into a transparent, glassy solid near − 190° C. Helium, on the other hand, is more volatile than hydrogen. Its liquefaction will accordingly give a lower temperature—*will give,* for it has not yet been accomplished. This strange and rare ingredient of our planet is the one accessible substance which remained invincibly gaseous at the close of the nineteenth century; there is no reason to doubt, however, that liquid helium will, in the twentieth, form one of the earliest trophies of research. Lord Kelvin's forecast of a substance by means of which the distance to absolute zero would be abridged from 15° to 5°, may then be realised.

Inert Components of Atmospheric Air.

The "inert" components of air may be ranked as a distinct class of bodies. They combine several exceptional peculiarities. They resemble mercury in being monatomic; the physical unit or molecule, is identical with the chemical unit, misnamed an atom; hence their density, as compared with hydrogen, is half their atomic weight. They stand apart from all other known substances in being devoid of chemical affinities; to a very limited extent they are capable of being dissolved by certain liquids, and absorbed by certain minerals; but they are strictly "non-valent"; they form no true compounds. For this reason, and because of the minute proportions in which they occur, ordinary tests fail to disclose their presence. They have no visible function in nature; they exist as if by a survival of an earlier order of things; their appointed part was perhaps played while the earth was still in the nebulous stage. They are wonderfully volatile considering their densities, and for this reason are of especial interest to cryogenists. The following little table gives the densities and boiling-points, according to Ramsay and Travers, of the five members of the group so far recognised:—

Element.	Density.	Atomic Weight.	Boiling-Point (Centigrade).
Helium	1·98	3·96	below − 262°
Neon	about 10·0	about 20	about − 239°
Argon	19·96	39·92	− 187°
Krypton	40·88	81·76	− 152°
Xenon	64	128	− 109°

Lord Rayleigh showed that the refractivity of helium was very small, being only 0·1238. On the same scale (refractivity of air = 1·0), the value of the constant for hydrogen is 0·469, or nearly four times greater, the opposite disparity of densities notwithstanding. The monatomic constitution of all these gases was established by finding 1·66 as the ratio between their specific heats at constant pressure and at constant volume. While exercising no appreciable absorption upon light, they glow brilliantly through the action of an electric discharge. An excited neon-tube flames with an orange-pink colour; krypton shines pale violet; xenon luminesces in sky-blue. The corresponding spectra are extremely vivid and characteristic. The progress of research at low temperatures led to anticipatory partial disclosures of them. In a paper 'On the Spectra of the Electric Discharge in Liquid Oxygen, Air and Nitrogen,' published in 1894 in the *Philosophical Magazine*, Professors Liveing and Dewar recorded the appearance, during the distillation and concentration *in vacuo* of liquid oxygen and air under diminished pressure, of two unknown bright lines at wave-lengths 557 and 555, the former coinciding approximately with the chief auroral ray. Both were subsequently, by Professor Ramsay and Dr. Travers, associated with krypton. Again, some strange bright lines, belonging to the then unidentified spectrum of neon, were derived by Professor Dewar in 1897 from a vacuum-tube filled with a residuum of gas from the King's Well at Bath, collected by the kind permission of the Corporation of that town; this is one of the most valuable among the available sources for the supply of scarce aërial constituents.

Low Temperature as an Analytic Agent.

What may almost be designated a new branch of pneumatic chemistry, the analysis of gases by cold, was set on foot by Professor Dewar in 1897. On the 4th of November in that year, he described before the Chemical Society an apparatus for determining the proportion of any atmospheric ingredient that remains uncondensed at −210° C. and is insoluble in liquid air under standard pressure. Preliminary experiments showed that one part of hydrogen in a thousand of air could just be detected by the newly devised method, and that liquid air can dissolve one-fifth of its own volume of hydrogen. Helium proved to be soluble, though in a less degree, in liquid nitrogen. With the powerful aid of liquid hydrogen, these researches were continued during four ensuing years. A striking illustration of its extraordinary effectiveness in refrigeration is afforded by the rapid production, through its means, of high vacua. It was computed that the pressure of air in sealed tubes, frozen out by immersion in liquid hydrogen, could not exceed one-millionth of an atmosphere, apart from any that might result from the survival, in minute quantities, of gases more refractory than oxygen or nitrogen. The exhaustion, in other words, due to the frigorific treatment of air-

bulbs, is theoretically about the same obtained by boiling out a space with mercury. Practically, it turned out to be even higher. In carefully prepared tubes, vacua so nearly perfect were produced that heat had to be applied before a spark could be got to pass. Their spectroscopic examination gave results of singular interest. Carbonic oxide bands were generally present, but might be traced to emanations from the glass; they were associated with lines of hydrogen and helium, and the distinctive yellow ray of neon. The path thus struck out was pursued further in August 1900, when, by an improved process, some tubes were filled at low pressure with the more volatile gases of the atmosphere. Traces of nitrogen, argon, and carbon-compounds, having been abolished by a bath of liquid hydrogen, sparking brought out prominently the spectra of hydrogen, helium, and neon, together with a number of less brilliant rays of unknown origin. Excited by continuous electric discharges, tubes thus prepared glow throughout with a strong orange light. The violet and ultra-violet sections of the spectrum given by it seem, nevertheless, to rival the strength of its red and yellow rays, so far as could be judged from spectrographic evidence. Sensitive plates received impressions of great intensity up to a wave-length of 314, despite the opacity of glass to such rapid vibrations. The photographs, it is true, were taken with a quartz calcite train, but there was still the glass of the tubes to be reckoned with.

The wave-lengths of nearly three hundred rays in the spectrum derived in this manner from residual atmospheric gases, uncondensed at the temperature of liquid hydrogen, were measured by Professors Liveing and Dewar, the spark-spectrum of iron serving for a standard of reference. Of these, 69 were identified, certainly or probably, as emanating from hydrogen, helium or neon, and it was noted, as a fact of no small significance, that among them were four members of the ultra-violet hydrogen series. For, under ordinary circumstances, they are emitted only by gas carefully purified; yet here they emerged with comparative facility on plates exposed to light from a heterogeneous mixture. An unexpected hint is thus afforded regarding the conditions that may tend to modify the hydrogen-spectrum in passing from star to star. Coincidences were diligently sought among the unknown lines with nebular, coronal and auroral rays, but with dubious or partial success. The possibility, however, was not excluded that "nebulum" might actually lurk, almost infinitesimally, in the earth's atmosphere, since, from one tube which, owing to its somewhat different treatment, preserved traces of nitrogen and argon, a faint additional ray was derived, agreeing approximately in position with the chief bright line of gaseous nebulæ at λ 5007. Further observations were contemplated for the verification of this curiously interesting suggestion. A good many subordinate lines in the tube-spectra fell very near the places assigned to radiations from the sun's corona; yet here, again, confirmation was needed before the terrestrial presence of coronium

could be regarded as even probable. Instances of agreement with the auroral spectrum were also adverted to; and some are likely to prove genuine. In this direction, certainly, lies the best hope of elucidating the baffling problem of the "Northern Lights."

By the use of liquid hydrogen as an analytic agent, neon can be spectroscopically distinguished through its yellow line at λ 5853, in 25 cubic centimetres of ordinary air. The searching nature of the method may be estimated from the consideration that the proportion of the gas present is only one in 40,000. The fundamental neon-line, indeed, predominates in the spectrum of the atmospheric residuum, very much as the adjacent ray of helium does in the prismatic light emanating from the more volatile portion of the Bath gas. Both rays shine in each spectrum, but with reversed brilliancy. Professor Dewar's inquiries thus confirmed the status of helium as an invariable atmospheric constituent. They, moreover, demonstrated the association with it of hydrogen. In every sample of air there is a percentage of hydrogen. The ratio by volume, according to M. Armand Gautier's recent determination, is 1 to 5000. If, then, as Dr. Johnstone Stoney maintains, the velocities of its molecules are, in the long run, uncontrollable by gravity, the leakage thence ensuing must be compensated either from within or from without. Subterranean sources perhaps supply the deficit; or interplanetary space itself gives back as many vagrant molecules as it receives. A balance, at any rate, is evidently struck somehow.

In a later communication to the Royal Society (read June 20, 1901), Professors Liveing and Dewar dealt with the least volatile, as they had previously dealt with the most volatile of the atmospheric gases. Separated from liquid air by careful processes of distillation, xenon and krypton were submitted to spectroscopic examination, in the course of which the variations of their spectra with the character of the electric discharge attracted particular attention. The rays of xenon measured and tabulated numbered 257, those of krypton, 182.

Low Temperature and Vital Phenomena.

Low-temperature research is of extreme importance to the study of vital phenomena. Our ideas as to the nature of life, and our conjectures regarding the course of its history on this planet, must be largely regulated by experience of its capability to resist extremes of heat and cold. Now the upper limit of endurance is easily reached; it is never above, and is usually considerably below $+100°$ C.; but germicidal cold has not yet been produced. Warm-blooded animals, to be sure, necessarily perish, and perish promptly, under frigid conditions. The power of resistance, however, increases with simplicity of organisation; and the ultimate atoms of life (so to call bacteria) bear with impunity an indefinite amount and degree of freezing. Professor M'Kendrick found, in 1893, that sterilisation did not result from an hour's exposure to a temperature of $-182°$ C. Samples of

blood, meat and milk, sealed in glass tubes, underwent putrefaction in the ordinary course after prolonged immersion in liquid oxygen. Nor was the germinating power of seeds impaired by subjection to the ordeal. Seven years later, Dr. Allan Macfadyen carried out an extensive series of experiments of this nature at the Royal Institution, under the personal supervision of Professor Dewar. The action of liquid air on bacteria was first tested. It proved entirely innocuous. After twenty hours at $-190°$ C., no diminution in their powers of growth, or in any of their functional activities, was perceptible. Phosphorescent organisms supplied a striking illustration of the alternate suspension and renewal of vital processes by freezing and thawing. Cooled down in liquid air they become non-luminous, but the intra-cellular oxidation producing the phosphorescence recommenced with full vigour when the temperature was raised. The sudden cessation and rapid renewal of the shining faculty of the cells, despite extreme changes of temperature, were eminently instructive. Seven days in liquid air proved no more deleterious to bacterial life than twenty hours. The temperature of liquid hydrogen was tried, with the same upshot; the much-enduring series of organisms dealt with suffered no injury. At 21° absolute, then, and probably much nearer to the zero-point, life can continue to exist. It can continue to exist, that is to say, under conditions bringing about an entire cessation of chemical, and an approximate cessation of molecular, activity. These facts, as Dr. Macfadyen remarked, "afford new ground for reflection as to whether life, after all, is dependent for its continuance on chemical reactions." Biologists, he added, "therefore follow with the keenest interest Professor Dewar's heroic attempts to reach the absolute zero"; while his success so far has already thrown open to them a new realm of experimentation, and placed in their hands an agent of investigation from the effective use of which they may "hope to gain a little further insight into the great mystery of life itself." The speculative interest of these researches supplies, indeed, the keenest incentive to their prosecution. The transmissibility from planet to planet, for instance, of living germs or spores has often been debated; it is now known that the cold of space would be unlikely to stand in the way. There are, however, other difficulties less easily removable; nor, even if a cosmic community of bacterial species were established, should we find ourselves any nearer to the heart of the creative mystery of life's origin.

The development of low-temperature chemistry is one of the most striking features of scientific history during the last decade of the nineteenth century. Many questions of profound interest have been answered through its means, and a partial insight has been gained into some of the most recondite secrets of nature. The unique condition attends it, that the *ne plus ultra* cannot recede as it advances. The absolute zero forms an irremovable landmark, a boundary-line that may not be transgressed, an asymptote, as it were, to the curve of future progress. And every step nearer to it is

harder to take than the previous one. Among many causes of augmenting difficulty is the circumstance that the molecular latent heats of vaporisation diminish with the absolute boiling-point. Hence, a continually more lavish expenditure of frigorific material is necessitated, and of material the price of which, in money and labour, rises rapidly with its frigorific efficacy. Still, although the bottom of the temperature-scale may never be actually reached, the intervening space will surely be much abridged. But we shall never, it is safe to predict, assist at the "death of matter." At the stage arrived at, there is no sign of its being moribund. Forces still act within and upon it. Gravity and cohesion maintain their normal power. It sensibly impedes the passage of electricity in the purest and most highly conducting metals. Its minute particles can take up and modify luminous vibrations. Only chemical affinity seems to be extinct; the various species of matter cease to react upon each other. The next cryogenic achievement, it is true, may alter the situation as we now see it. Our present standing-ground may be subverted, for the inquiry is just now in a critical phase. The liquefaction of helium, for example, may prove decisive of many things—it may set at rest some doubts, and raise unlooked-for issues.

The conditions for its accomplishment were clearly set forth in the Bakerian Lecture. They may be realised by the use of methods actually available. This last fortress of gaseity cannot be regarded as impregnable, although its capture will be at a high monetary cost. Gaseous helium, to begin with, is of the utmost scarcity; and what is scarce demands outlay to procure. Its condensation can be effected only by subjecting it to the same process that succeeds with hydrogen, substituting, however, liquid hydrogen under exhaustion for liquid air as the primary cooling agent. As the upshot, a liquid will be at hand, boiling at about 5° absolute, or $-268°$ C., but more expensive than liquid hydrogen, in a much higher ratio than liquid hydrogen is more expensive than liquid air. By comparison, "potable gold" would be a cheap fluid. Nor could the precious metal, in that, or any other form, be employed for a higher intellectual purpose than in promoting and extending researches of such boundless promise and commanding interest as those conducted at the Royal Institution.

List of Papers.

1893 "Liquid Atmospheric Air." Proc. Roy. Inst. vol. xiv. p. 1. (Discourse delivered January 20, 1893.)

1893 "The Electrical Resistance of Metals and Alloys at Temperatures approaching the Absolute Zero." (With Professor J. A. Fleming.) Phil. Mag. vol. xxxvi. p. 271. See also *Electrician*, vol. xxxi. p. 529.

1893 "Refractive Indices of Liquid Nitrogen and Air." (With Professor Liveing.) Phil. Mag. vol. xxxvi. p. 328.

1893 "Solid Atmospheric Air." Proc. Roy. Soc. vol. liii. p. 80.

1894 "Phosphorescence of Bodies at Low Temperatures." Proc. Roy. Soc. vol. lv. p. 340.

1894 "Experiments in the Liquefaction of Hydrogen." *Times*, September 1, 1894, and *Chemical News*, September 7, 1894.
1894 "Phosphorescence and Photographic Action at the Temperature of Boiling Liquid Air." Proc. Chem. Soc. vol. x. p. 171.
1894 "Relative Behaviour of Chemically Prepared and of Atmospheric Nitrogen in the Liquid State." Proc. Chem. Soc. vol. x. p. 222.
1894 "Spectrum of the Electric Discharge in Liquid Oxygen, Air and Nitrogen." (With Professor Liveing.) Phil. Mag. vol. xxxviii. p. 235.
1894 "Scientific Uses of Liquid Air." Proc. Roy. Inst. vol. xiv. p. 393. (Discourse delivered January 19, 1894.)
1895 "The Liquefaction of Gases." Phil. Mag. vol. xxxix. p. 298.
1895 "Thermo-electric Powers of Metals and Alloys between the Temperatures of the Boiling Point of Water and the Boiling Point of Liquid Air." (With Professor J. A. Fleming.) Phil. Mag. vol. xl. p. 95. (See also *Electrician*, vol. xxxv. p. 365.)
1895 "Refraction and Dispersion of Liquid Oxygen and the Absorption Spectrum of Liquid Air." (With Professor Liveing.) Phil. Mag. vol. xl. p. 268.
1895 "The Variation in the Electric Resistance of Bismuth when cooled to the Temperature of Solid Air." (With Professor J. A. Fleming.) Phil. Mag. vol. xl. p. 303. (See also *Electrician*, vol. xxxv. p. 612.)
1895 "Phosphorescence and Photographic Action at the Temperature of Boiling Liquid Air." Proc. Roy. Inst. vol. xiv. p. 665. (Discourse delivered January 18, 1895.)
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1896 "New Researches on Liquid Air." Proc. Roy. Inst. vol. xv. p. 133. (Discourse delivered March 27, 1896.)
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1896 "Changes produced in Magnetised Iron and Steels by Cooling to the Temperature of Liquid Air." (With Professor J. A. Fleming.) Proc. Roy. Soc. vol. lx. p. 57.
1896 "Electrical Resistivity of Bismuth at the Temperature of Liquid Air." (With Professor J. A. Fleming.) Proc. Roy. Soc. vol. lx. p. 72.
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1896 "Magnetic Permeability and Hysteresis of Iron at Low Temperatures." (With Professor J. A. Fleming.) Proc. Roy. Soc. vol. lx. p. 81.
1896 "Magnetic Permeability of Liquid Oxygen and Liquid Air." (With Professor J. A. Fleming.) Proc. Roy. Soc. vol. lx. p. 283.
1896 "Dielectric Constants of Liquid Oxygen and Liquid Air." (With Professor J. A. Fleming.) Proc. Roy. Soc. vol. lx. p. 358.
1896 "Electric and Magnetic Research at Low Temperatures." (By Professor J. A. Fleming.) Proc. Roy. Inst., vol. xv. p. 239.
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1897 "Electrical Resistivity of Electrolytic Bismuth at Low Temperatures and in Magnetic Fields." (With Professor J. A. Fleming.) Proc. Roy. Soc. vol. lx. p. 425.
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1897 "Dielectric Constants of Pure Ice, Glycerine, Nitro-Benzol and Ethylene Dibromide at and above the Temperature of Liquid Air." (With Professor J. A. Fleming.) Proc. Roy. Soc. vol. lxi. p. 316.
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1897 "Properties of Liquid Fluorine." (With Professor Moissan.) Proc. Chem. Soc. vol. xiii. p. 175.
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1898 "The Boiling Point and Density of Liquid Hydrogen." Proc. Chem. Soc. vol. xiv. p. 146.
1898 "The Liquefaction of Hydrogen and Helium." Proc. Chem. Soc. vol. xiv. p. 129.
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1898 "Application of Liquid Hydrogen to the Production of High Vacua and their Spectroscopic Examination." Proc. Roy. Soc. vol. lxiv. p. 231. *Nature*, January 19, 1899. Annales de Chimie, vol. xvii. p. 12.
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1898 "Magnetic Susceptibility of Liquid Oxygen." (With Professor J. A. Fleming.) Proc. Roy. Soc. vol. lxiii. p. 311.
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1901 "The Boiling Point of Liquid Hydrogen, determined by Hydrogen and Helium Gas Thermometers." Proc. Roy. Soc. vol. lxviii. p. 44. Annales de Chimie, July, 1901.
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1901 "The Nadir of Temperature and Allied Problems." (Bakerian lecture by Professor Dewar, Royal Society, June 13, 1901.) Proc. Roy. Soc. vol. lxviii. p. 360.
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GENERAL MONTHLY MEETING,

Monday, July 1, 1901.

His Grace The Duke of Northumberland, K.G. D.C.L. F.R.S., President, in the Chair.

Ashton Charles Allen, Esq.

was elected a Member of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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GENERAL MONTHLY MEETING,

Monday, November 4, 1901.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Mrs. Crowdy

was elected a Member of the Royal Institution.

The Special Thanks of the Members were returned to Mr. Charles Hofman for the munificent Gift of a Bust (by Sir Francis Chantrey, R.A.) of Thomas Harrison, F.R.S., who was Honorary Secretary of the Royal Institution from 1813–1824, presented in Memory of his Grandson, Thomas Edward Harrison, who recently died in South Africa while on Service with the Imperial Yeomanry.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Secretary of State for India—Report on the Kodaikanal and Madras Observatories for 1900–1901. 4to.
The Governor-General of India—General Report, 1900–1901. 8vo. 1901.
Memoirs, Vol. XXXI. Part 1. 8vo. 1901.
Palæontologia Indica, New Series, Vol. I. Part 3. fol. 1901.
The British Museum Trustees—Catalogue of the Schreiber Playing Cards. 8vo. 1901.
Catalogue of Hebrew and Samaritan MSS. 4to. 1899.
Facsimiles of Biblical MSS. 4to. 1900.
Catalogue of the Greek Coins of Lycaonia, Isauria and Cilicia. 8vo. 1900.
Catalogue of Sinhalese MSS. 4to. 1900.
"*A British Field Officer*" (*the Author*)—The Army and the Press in 1900. 8vo. 1901.
Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta : Rendiconti. 2° Semestre, Vol. X. Fasc. 5°–7°. 4to.
Amsterdam, Royal Academy of Sciences—Verhandelingen, 1e Sect. Deel VII. Nos. 1–7; 2e Sect. Deel VII. Nos. 1–7. 1899–1900.
Zittingsverslagen, 1900–1901, Vol. IX.
Jaarboek, 1899, 1900. 8vo. 1900.
Verslag, Deel VIII. 8vo. 1899–1900.
Proceedings of the Section of Sciences, Vol. II. III. 8vo. 1899–1901.
American Academy of Arts and Sciences—Proceedings, Vol. XXXVI. Nos. 24–29. 8vo. 1901.
American Geographical Society—Bulletin, Vol. XXXIII. No. 3. 8vo. 1901.
American Philosophical Society—Proceedings, Vol. XL. Nos. 165, 166. 8vo. 1901.
Aristotelian Society—Proceedings, New Series, Vol. I. 8vo. 1901.
Asiatic Society, Royal—Journal for July–Oct. 1901. 8vo.

Asiatic Society of Bengal—Proceedings, 1901, Nos. 3–8. 8vo.
Journal, Vol. LXIX. Part 3; Vol. LXX. Part 1, No. 1, Part 2, No. 1. 8vo. 1901.
Astronomical Society—Monthly Notices, Vol. LXI. Nos. 8, 9. 8vo. 1901.
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Baylis, T. H. Esq. K.C. (the Author)—On the Office of Reader or Lector. 8vo. 1901.
Belgium Royal Academy of Sciences—Bulletins, 1899–1900. 8vo.
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Mem. cour. et des savants étrang., Tomes LVII. LVIII. 4to. 1900.
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Berlin Academy of Sciences—Sitzungsberichte, 1901, Nos. 23–38. 8vo. 1901.
Boston Public Library—Monthly Bulletin for July–Oct. 8vo. 1901.
Boston Society of Medical Sciences—Journal, Vol. V. No. 11. 8vo. 1901.
Forty-ninth Annual Report of the Trustees. 1901.
British Architects, Royal Institute of—Kalendar, 1901–1902. 8vo.
Journal, 3rd Series, Vol. VIII. Nos. 18–20. 4to. 1901.
British Astronomical Association—Memoirs, Vol. XI. Parts 9, 10. 8vo. 1901.
British South Africa Company—Reports on the Administration of Rhodesia, 1898–1900.
Brymner, D. Esq.—Report on Canadian Archives, 1899–1900. 8vo.
Cambridge Philosophical Society—Proceedings, Vol. XI. Part 3. 8vo. 1901.
Camera Club—Journal for July–Oct. 1901. 8vo.
Canada, Department of Marine Fisheries—Report of the Meteorological Service of Canada, 1898. 8vo. 1901.
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Canadian Institute—Transactions, Vol. VII. Part 1. 8vo. 1901.
Chemical Industry, Society of—Journal, Vol. XX. No. 9. 8vo. 1901.
Chemical Society—Proceedings, No. 240. 8vo. 1901.
Journal for July–Oct. 1901. 8vo.
Chicago, Field Columbian Museum—Publications, Nos. 55–59. 8vo. 1901.
City of London College—Calendar, 1901–1902. 8vo.
Civil Engineers, Institution of—Minutes of Proceedings, Vols. CXLV. CXLVI. 1901.
Clinical Society—Transactions, Vol. XXXIV. 8vo. 1901.
Colonial Institute, Royal—Proceedings, Vol. XXXII. 8vo. 1901.
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Colacurcio, Prof. S. G.—Sull' Opera Istituzioni Bibliche del Prof. Giovan Giacinto Cereseto. 12mo. 1901.
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Cornwall Polytechnic Society, Royal—Sixty-eighth Annual Report. 8vo. 1900.
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Ducretet, E. Esq.—Catalogue Raisonné, 3me partie: Électricité. 8vo. 1900.
Editors—Aeronautical Journal for July–Oct. 1901. 8vo.
American Journal of Science for July–Oct. 1901. 8vo.
Analyst for July–Oct. 1901. 8vo.
Anthony's Photographic Bulletin for July–Oct. 1901. 8vo.
Astrophysical Journal for July–Oct. 1901.
Athenæum for July–Oct. 1901. 4to.
Author for July–Oct. 1901. 8vo.
Brewers' Journal for July–Oct. 1901. 8vo.
Chemical News for July–Oct. 1901. 4to.
Chemist and Druggist for July–Oct. 1901. 8vo.
Electrical Engineer for July–Oct. 1901. fol.
Electrical Review for July–Oct. 1901. 8vo.

Editors—continued.
Electricity for July–Oct. 1901. 8vo.
Electro-Chemist and Metallurgist for July–Oct. 1901. 8vo.
Engineer for July–Oct. 1901. fol.
Engineering for July–Oct. 1901. fol.
Homœopathic Review for July–Oct. 1901. 8vo.
Horological Journal for July–Oct. 1901. 8vo.
Invention for July–Oct. 1901.
Journal of the British Dental Association for July–Oct. 1901. 8vo.
Journal of State Medicine for July–Oct. 1901. 8vo.
Law Journal for July–Oct. 1901. 8vo.
Lightning for July–Oct. 1901. 8vo.
London Technical Education Gazette for July–Oct. 1901.
Machinery Market for July–Oct. 1901. 8vo.
Motor Car Journal for July–Oct. 1901. 8vo.
Nature for July–Oct. 1901. 4to.
New Church Magazine for July–Oct. 1901. 8vo.
Nuovo Cimento for July–Oct. 1901. 8vo.
Pharmaceutical Journal for July–Oct. 1901. 8vo.
Photographic News for July–Oct. 1901. 8vo.
Physical Review for July–Oct. 1901. 8vo.
Popular Science Monthly for July–Oct. 1901.
Public Health Engineer for July–Oct. 1901. 8vo.
Science Abstracts for July–Oct. 1901. 8vo.
Travel for July–Oct. 1901. 8vo.
Tropical Agriculturist for July–Oct. 1901. 8vo.
Zoophilist for July–Oct. 1901. 4to.

Electrical Engineers, Institution of—Journal, Vol. XXX. Nos. 151, 152. 8vo. 1901.

Fleming, Prof. J. A. F.R.S. M.R.I. (the Author)—A Handbook for the Electrical Laboratory and Testing Room, Vol. I. 8vo. 1901.

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Harlem, Société Hollandaise des Sciences—Archives Néerlandaises, Série II. Tome IV. Livr. 3. 8vo. 1901.
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Harvard College, Astronomical Observatory—Annals, Vol. XXVIII. Part 2. 4to. 1901.

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Imperial Institute—Imperial Institute Journal for July–Oct. 1901.

Inner Temple, The Honourable Society of the—Masters of the Bench of the Society, Supplement, 1883–1900. 8vo. 1901.

Iron and Steel Institute—Journal, 1901, No. 1. 8vo.

Johns Hopkins University—American Journal of Philology, Vol. XXII. No. 117. 8vo. 1901.
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Langley, S. P. Hon. Mem. R.I. (the Author)—The New Spectrum. 8vo. 1901.

Leighton, John, Esq. M.R.I.—Journal of the Ex-Libris Society for Sept. 1901. 8vo.

Linnean Society—Journal, Nos. 182, 243. 8vo. 1901.
Transactions: Botany, Vol. V. Parts 13–15, Vol. VI. Part 1; Zoology, Vol. VIII. Parts 1–4. 4to. 1900–1901.

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Manchester Museum, Owens College—Publications, No. 34. fol. 1901.
Mechanical Engineers, Institution of—Proceedings, 1901, No. 2. 8vo. 1901.
Metropolitan Asylums Board—Annual Report, 1900. 2 vols. 8vo. 1901.
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Middlesex Hospital—Reports for the year 1899. 8vo.
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Sitzungsberichte, 1899, Heft 3; 1900, Heft 1; 1901, Heft 2, 3. 8vo.
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New York Academy of Sciences—Annals, Vol. XIII. 8vo. 1900–1901.
Memoirs, Vol. II. Part 3. 4to. 1901.
Norfolk and Norwich Naturalists' Society—Transactions, Vol. VII. Part 2. 8vo. 1901.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XLIX. Part 6; Vol. L. Parts 2–5. 8vo. 1900–1901.
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Paris, Société Française de Physique—Séances, 1900, Fasc. 4; 1901, Fasc. 1. 8vo. 1901.
Pharmaceutical Society of Great Britain—Journal for July–Oct. 1901. 8vo.
Philadelphia, Academy of Natural Sciences—Proceedings, Vol. VIII. Part 1. 8vo. 1901.
Phipson, Dr. T. L. (the Author)—Researches on the Past and Present History of the Earth's Atmosphere. 8vo. 1901.
Photographic Society, Royal—Photographic Journal for June–Oct. 1901. 8vo.
Physical Society—Proceedings, Vol. XVII. Part 6. 8vo. 1901.
Quesneville, G. Esq. (the Author)—Théorie nouvelle de la Dispersion. 8vo. 1901.
Recherches sur les Réseaux. 8vo. 1899.
Queensland Government—North Queensland Ethnography Bulletins, Nos. 1, 2. fol. 1901.
Righi, Prof. A. (the Author)—Sui Campi elettromagnetici. 8vo. 1901.
Rio de Janeiro, Observatory of—Bulletins for May–Sept. 1900.
Annuario, 1901.
Rochechouart, La Société les Amis des Sciences et Arts—Tome X. Nos. 5, 6; Tome XI. No. 1. 8vo. 1900–1901.
Rome, Ministry of Public Works—Giornale del Genio Civile, March–June, 1901. 8vo.
Royal Irish Academy—Transactions, Vol. XXXI. Parts 8–11. 4to. 1900.
Proceedings, Third Series, Vol. VI. No. 2; Vol. VII. 8vo. 1901.
Royal Society of Edinburgh—Proceedings, Vol. XXIII. No. 4. 8vo. 1901.
Royal Society of London—Philosophical Transactions, A Nos. 287–289, 291–293; B, Nos. 98, 194, 199. 4to. 1901.
Proceedings, Nos. 447–450. 8vo. 1901.
Sanitary Institute—Journal, Vol. XXII. Parts 2, 3, and Supplement. 8vo. 1901.
Saxon Society of Sciences, Royal—
Mathematisch-Physische Classe—
Berichte, 1901, Nos. 1–3. 8vo.
Abhandlungen, Band XXVI. Nos. 5–7. 8vo. 1901.

Saxon Society of Sciences, Royal—continued.
Philologisch-Historische Classe—
Berichte, 1901, No. 1. 8vo.
Abhandlungen, Band XXI. No. 1. 8vo. 1901.
Selborne Society—Nature Notes for July–Oct. 1901. 8vo.
Smithsonian Institution—Miscellaneous Collections, No. 125. 8vo. 1901.
Annual Report, 1899. 8vo. 1901.
Annual Report, U.S. National Museum, 1899. 8vo. 1901.
Annals of the Astrophysical Observatory, Vol. I. 1901. 4to.
Society of Arts—Journal for July–Oct. 1901. 8vo.
Statistical Society, Royal—Journal, Vol. LXIV. Parts 2, 3. 8vo. 1901.
Tacchini, Prof. P. Hon. Mem. R.I. (the Author)—Memorie della Società degli Spettroscopisti Italiani, Vol. XXX. Disp. 7–9. 4to. 1901.
Toulouse, Société Archéologique du Midi de la France—Bulletin, No. 27. 8vo. 1901.
United Service Institution, Royal—Journal for July–Oct. 1901. 8vo.
United States Department of Agriculture—Report of the Chief of the Weather Bureau, 1899–1900. 4to. 1901.
Experiment Station Record, Vol. XIII. No. 2. 8vo. 1901.
North America Fauna, Nos. 20, 21. 8vo. 1901.
Monthly Weather Review for June, July, 1901. 4to.
United States Department of the Interior—Annual Reports, 1899, 7 vols.; 1900, 5 vols. 8vo.
Geological Survey, Twenty-eighth Report, 1899–1900, Vols. I.–VI. 4to.
United States Geological Survey—Twentieth Annual Report, Parts 2–5 and 7. 4to. 1900.
Monographs, Nos. 39, 40. 4to. 1900.
Bulletins, Nos. 163–176. 4to. 1900.
Geological Atlas, Folios Nos. 59–71. fol. 1901.
United States Patent Office—Annual Report for 1900. 8vo. 1901.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1901, Nos. 6–8. 8vo.
Vienna, Imperial Geological Institute—Verhandlungen, 1901, Nos. 7–10. 8vo.
Jahrbuch, Band L. Heft 4. 8vo. 1901.
Washington Academy of Sciences—Proceedings, Vol. III. pp. 217–272, 297–370. 8vo. 1901.
Western Society of Engineers (U.S.A.)—Journal of the Western Society of Engineers, Vol. VI. Part 3. 8vo. 1901.
Wisconsin Academy—Transactions, Vol. XIII. Part 1. 8vo. 1901.
Woodhouse, A. J. Esq. M.R.I.—The Life and Poetical Works of J. Woodhouse (1735–1820). Edited by Rev. R. J. Woodhouse. 2 vols. 1896.
Yorkshire Archæological Society—Journal, Part 63. 8vo. 1901.
Zoological Society—Proceedings, 1901, Vol. II. Parts 1, 2. 8vo.
Transactions, Vol. XVI. Parts 2, 3. 4to. 1901.

GENERAL MONTHLY MEETING,

Monday, December 2, 1901.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Sir Thomas Barlow, Bart. M.D. F.R.S.
K. M. Chance, Esq.
Sir Walter G. F. Phillimore, Bart. D.C.L.
Hon. C. S. Rolls

were elected Members of the Royal Institution.

The Honorary Secretary reported that the following Address t M. Berthelot, on the occasion of the Jubilee of his Researches, had been presented by Dr. J. H. Gladstone on behalf of the Members, and that Professor Cornu and Professor Mascart, as Honorary Members, had represented the Royal Institution at the Celebration in Paris on November 24.

"To M. Marcellin Berthelot, F.R.S. Grand Officier de la Légion d'Honneur, Membre de l'Institut, Secrétaire perpétuel de l'Académie des Sciences, Paris; Honorary Member of the Royal Institution of Great Britain.

"The Members of the Royal Institution of Great Britain desire to offer you their cordial congratulations on the occasion of the Jubilee of your Scientific Researches, and desire to express their great appreciation of the conspicuous services you have rendered in the extension and diffusion of scientific knowledge.

"The Members of the Royal Institution of Great Britain recognise the extraordinary originality of your Researches in the varied fields of Organic and Biological Chemistry, Physical Chemistry, Organic Synthesis, Thermo-Chemistry, together with your Philosophical and Alchemystical Studies, and the supreme importance of labours which have produced such valuable results and opened up new fields for Science.

"The Members of the Royal Institution of Great Britain earnestly hope that you will be long spared to continue such splendid researches and to further enrich your generation with sterling knowledge."

The Special Thanks of the Members were returned for the following Donation to the Fund for the Promotion of Experimental Research at Low Temperatures:

Dr. Ludwig Mond, F.R.S. . . . £100

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India, Geological Survey of India—Memoirs, Vol. XXX. Parts 3, 4; Vol. XXXI. Parts 2, 3; Vol. XXXII. Part 1; Vol. XXXIV. Part 1. 8vo. 1901.

The Meteorological Office—Meteorological Observations at Stations of the Second Order for 1898. 4to. 1901.

Accademia dei Lincei, Reale, Roma—Classe di Scienze Morali, etc. Serie Quinta. Vol. X. Fasc. 7, 8. 8vo. 1901.
Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. 2° Semestre, Vol. X. Fasc. 8, 9. 8vo. 1901.
Allegheny Observatory, Pa. (U.S.A.)—Scientific Reports. N.S. Nos. 1–3. 8vo. 1901.
American Geographical Society—Bulletin, Vol. XXXIII. No. 4. 8vo. 1901.
Bankers, Institute of—Journal, Vol. XXII. Part 8. 8vo. 1901.
Basel Naturforschende Gesellschaft—Verhandlungen, Band XIII. Heft 2; Band XIV. 8vo. 1901.
Boston Public Library—Bulletin for Nov. 1901. 8vo.
British Architects, Royal Institute of—Journal, Third Series, Vol. IX. Nos. 1, 2. 4to. 1901.
British Astronomical Association—Journal, Vol. XII. No. 1. 8vo. 1901.
British South Africa Company—Directors' Report and Accounts, 1899–1900. 8vo. 1901.
Camera Club—Journal for Nov. 1901. 8vo.
Chemical Industry, Society of—Journal, Vol. XX. No. 10. 8vo. 1901.
Chemical Society—Proceedings, Nos. 241, 242. 8vo. 1901.
Journal for Nov. 1901. 8vo.
Devonshire Association—Report and Transactions, Vol. XXXIII. 8vo. 1901.
Editors—American Journal of Science for Nov. 1901. 8vo.
Analyst for Nov. 1901. 8vo.
Anthony's Photographic Bulletin for Nov. 1901. 8vo.
Astrophysical Journal for Oct. 1901.
Athenæum for Nov. 1901. 4to.
Author for Nov. 1901. 8vo.
Brewers' Journal for Nov. 1901. 8vo.
Chemical News for Nov. 1901. 4to.
Chemist and Druggist for Nov. 1901. 8vo.
Electrical Engineer for Nov. 1901. fol.
Electrical Review for Nov. 1901. 8vo.
Electricity for Nov. 1901. 8vo.
Electro Chemist and Metallurgist for Nov. 1901.
Engineer for Nov. 1901. fol.
Engineering for Nov. 1901. fol.
Homœopathic Review for Nov. 1901. 8vo.
Horological Journal for Nov. 1901. 8vo.
Invention for Nov. 1901. 8vo.
Journal of the British Dental Association for Nov. 1901. 8vo.
Journal of Physical Chemistry for Oct. to Nov. 1901. 8vo.
Journal of State Medicine for Nov. 1901. 8vo.
Law Journal for Nov. 1901. 8vo.
Lightning for Nov. 1901. 8vo.
London Technical Education Gazette for Nov. 1901. 8vo.
Machinery Market for Nov. 1901. 8vo.
Motor Car Journal for Nov. 1901. 8vo.
Musical Times for Nov. 1901. 8vo.
Nature for Nov. 1901. 4to.
New Church Magazine for Nov. 1901. 8vo.
Nuovo Cimento for Nov. 1901. 8vo.
Photographic News for Nov. 1901. 8vo.
Physical Review for Nov. 1901. 8vo.
Popular Science Monthly for Nov. 1901. 8vo.
Telephone Magazine for Nov. 1901. 8vo.
Terrestrial Magnetism for Nov. 1901. 8vo.
Travel for Nov. 1901. 8vo.
Zoophilist for Nov. 1901. 4to.
Electrical Engineers, Institution of—Journal, Vol. XXX. No. 150. 8vo. 1901.

Franklin Institute—Journal for Nov. 1901. 8vo.
Geographical Society, Royal—Geographical Journal for Nov. 1901. 8vo.
Geological Society—Quarterly Journal, No. 228. 8vo. 1901.
Glasgow Philosophical Society—Proceedings, Vol. XXXII. 8vo. 1901.
Goppelsroeder, F. Esq. (the Author)—Capillaranalyse. 8vo. 1901.
Imperial Institute—Imperial Institute Journal for Nov. 1901.
Johns Hopkins University—American Chemical Journal for Nov. 1901. 8vo
Johnston, Messrs. W. & A. K. (the Publishers)—The Navy League Map of the British Empire.
Leighton, John, Esq. M.R.I.—Ex-Libris Journal for Oct. 1901. 8vo.
Linnean Society—Journal, No. 183. 8vo. 1901.
Proceedings, Oct. 1901. 8vo.
Liverpool School of Tropical Medicine—First Progress Report of the Campaign against Mosquitoes in Sierra Leone. 8vo. 1901.
Meteorological Society—Quarterly Journal, No. 120. 8vo. 1901.
Record, No. 80. 8vo. 1901.
Navy League—Navy League Journal for Nov. 1901. 8vo.
New South Wales, Royal Society of—Journal and Proceedings, Vol. XXXIV. 8vo. 1900.
New Zealand, Agent-General—Notes on New Zealand. 8vo. 1901.
Odontological Society—Transactions, Vol. XXXIV. No. 1. 8vo. 1901.
Paris Exhibition, Royal Commission for the, 1900.—British Official Catalogue. 8vo. 1900.
Report of His Majesty's Commissioners, Vols. I. II. 8vo. 1901.
Pharmaceutical Society of Great Britain—Journal for Nov. 1901. 8vo.
Photographic Society, Royal—Photographic Journal for Oct. 1901. 8vo.
Quekett Microscopical Club—Journal, Series 2, Vol. VIII. No. 49. 8vo. 1901.
Rome, Ministry of Public Works—Giornale del Genio Civile, August, 1901. 8vo.
Royal College of Surgeons—Calendar. 1901. 8vo.
Royal Irish Academy—Proceedings, Vol. VI. No. 3. 8vo. 1901.
Royal Society of Edinburgh—Transactions, Vol. XL. Part 1. 4to. 1901.
Royal Society of London—Philosophical Transactions, A. Nos. 294–297; B. Nos. 201–203. 4to. 1901.
Proceedings, Nos. 451, 452. 8vo. 1901.
Selborne Society—Nature Notes for Nov. 1901. 8vo.
Society of Arts—Journal for Nov. 1901. 8vo.
United Service Institution, Royal—Journal for Nov. 1901. 8vo.
United States Department of Agriculture—Monthly Weather Review for August, 1901. 8vo.
United States Geological Survey—Twenty-first Annual Report, Parts 1 and 6. 4to. 1900–1901.
Victoria Institute—Journal, Vol. XXXIII. 8vo. 1901.
Washington Academy of Sciences—Proceedings, Vol. III. pp. 371–486, 487–539. 8vo. 1901.
Western Society of Engineers—Journal, Vol. VI. No. 5. 8vo. 1901.
Zoological Society of London—A Record of the Progress of the Zoological Society of London. 8vo. 1901.

WEEKLY EVENING MEETING,

Friday, January 18, 1901.

His Grace the Duke of Northumberland, K.G. D.C.L. F.R.S. President, in the Chair.

Professor Dewar, M.A. LL.D. D.Sc. F.R.S. *M.R.I.*

Gases at the Beginning and End of the Century.

(Abstract.)

It is interesting to review in broad outline the century's progress in some limited field of research. The subject of the Gases in its widest sense is too large a province to cover in a single discourse, but if the sketch is confined more especially to the growth of our knowledge of the change of state in gases, then the compass of the review becomes restricted to practicable dimensions.

At the beginning of the century the doctrines put forth in Lavoisier's 'Elements of Chemistry,' held overwhelming sway. The various states of matter were explained as arising from variations in the amount of caloric with which the body was penetrated. The term caloric must be interpreted as meaning the repulsive cause, whatever that may be, which separates the particles of matter from each other. According as the repulsive power is equal to, stronger, or weaker than, the attraction of the particles the substance became liquid, gaseous or solid. As a general principle it was assumed that every body in nature was susceptible of taking on solid, liquid or aeriform states.

The elastic aeriform fluids were now characterised for the first time under the generic term gases. A clear distinction was, however, drawn between the caloric which might be said to act like a solvent and the substance or base of the gas with which it was combined.

The particles of bodies, according to Lavoisier's view, do not make contact with each other, the intervals between them varying according to the figures and magnitude and the existing proportion between their inherent attraction and the repulsive force exerted in them by the caloric. The presentation of caloric, sometimes as a real material or very subtile fluid, did not prevent Lavoisier from generalising with profound sagacity on the fundamental similarity of all known gases. Thus, some thirty years before the definite liquefaction of any gas at that time regarded as permanent, in speculating on what would occur provided the earth were suddenly transported into a very cold region, he said, "In this case, the air, or at least some of the aeriform fluids which now compose the mass of our atmosphere,

would doubtless lose their elasticity for want of a sufficient temperature to retain them in that state; they would return to the fluid state of existence, and new liquids would be formed of whose properties we cannot at present form the most distant idea."

Dalton, in his paper 'On the force of steam or vapour from water and various other liquids both in a vacuum and in air,' published in 1802, arrived at the conclusion that, "there can scarcely be a doubt entertained respecting the reducibility of all elastic fluids of whatever kind into liquids; and we ought not to despair of effecting it in low temperature and by strong pressure exerted upon the unmixed gases." Here we notice Dalton introduces the novelty of suggesting the application of combining high pressure and low temperature on the pure gases, which led in 1823 to the successful experiments of Northmore, and then of Faraday and Davy. Thomas Young, in the lectures he delivered in this Institution early in the last century, and subsequently published, in 1807, under the title 'A Course of Lectures on Natural Philosophy and the Mechanical Arts,' after a careful analysis of all the existing experimental data, strongly supported the view that heat was a quality of the particles of bodies, and that that quality could only be motion. In explanation of the action of heat he says, "The effects of heat on the cohesive and repulsive powers of bodies have sometimes been referred to the centrifugal forces and mutual collisions of the revolving and vibrating particles; and the increase of the elasticity of aeriform fluids has been very minutely compared with the force which would be derived from an acceleration of these internal motions." (Young, 1807.) In the 'Elements of Chemical Philosophy,' published in 1812, Davy, in order to guard against the supposition that the doctrine of a specific fluid of heat was a necessary part of the principles of philosophical chemistry, threw out the following suggestions as the basis of a rational theory of the causes inducing change of state in matter; following the dictum, that the immediate cause of the phenomenon of heat is motion. Thus he says, "It seems possible to account for all the phenomena of heat if it be supposed that in solids the particles are in a constant state of vibratory motion, the particles of the hottest bodies moving with the greatest velocity and through the greatest space; that in fluids and elastic fluids, besides the vibratory motion, which must be conceived greatest in the last, the particles have a motion round their own axes with different velocities, the particles of elastic fluids moving with the greatest quickness; and that in ethereal substances the particles move round their own axes, and separate from each other, penetrating in right lines through space. Temperature may be conceived to depend upon the velocities of the vibrations; increase of capacity on the motion being performed in greater space, and the diminution of temperature during the conversion of solids into fluids or gases, may be explained on the idea of the loss of vibratory motion in consequence of the revolution of particles round their axes at the moment when the body becomes fluid or aeriform, or from loss of rapidity of vibration in consequence of the motion of the particles through greater

space." Later investigators, while altering the details of Davy's theoretical explanations of the gaseous state, in substance acknowledged the legitimacy of his ideas by extending and perfecting his hypothesis. A short time after the publication of Dalton's 'New System of Chemical Philosophy,' Leslie and Wollaston made their memorable experiments on the freezing of water by inducing rapid evaporation of the isolated liquid in a receiver by chemically absorbing or condensing the vapour as quickly as it is produced in a separate part of the same vessel. Thus originated the process of reaching lower temperatures and maintaining them by the continuous distillation of a fluid, or what is now called distillation *in vacuo* or under reduced pressure. Gay Lussac succeeded in showing that by such means a temperature as low as the freezing-point of mercury could be reached. Cagniard de la Tour in 1822 made his most startling experiments, proving that the dilatation of liquids has a limit beyond which, in spite of compression, they become gases. He succeeded in vaporising ether in a space twice the volume of the liquid, and noticed that before disappearing the liquid seemed to occupy the whole space; the tube finally looking empty, until on cooling a thick mist appeared for a minute, and then suddenly the liquid in the old state. This change in the case of ether from the liquid to the gas he showed took place at 160°, the pressure then being 37 to 38 atmospheres. He made similar experiments with alcohol, bisulphide of carbon and water, showing that the same state could be brought about in each case, only at different temperatures and pressures for each substance. This beautiful investigation, involving the use of sealed tubes together with manometric observations —the temperatures and pressure being very considerable—was indeed of first-rate importance, but its real value was not appreciated until many years later.

In the experiments of Faraday and Davy high pressures were obtained by generating the gases in sealed tubes bent into the shape of an inverted U, so that one leg containing the re-acting chemicals might be heated if necessary, and the other cooled to form a condenser for the liquefied gas. Davy, who was always alive to the possibility of practical utility resulting from scientific discovery, had the idea of using liquid gases as agents for the production of motive power and as the means by which great reductions of temperature could be effected, owing to the rapidity with which they could be rendered aeriform. Although Davy's mechanical suggestion has not been generally adopted, there can be no doubt about the successful application of such liquids to command steady low temperatures by their evaporation.

The next great advance was the production of large quantities of liquid carbonic acid by Thilorier in 1835, and the remarkable discovery that when ejected into the air its rapid evaporation reduced the temperature to such an extent as to cause its own solidification into the well-known snow. He further observed that the coefficient of expansion of the liquid gas is greater than that of any aeriform

body. No wonder that Thilorier should say that inside a Faraday tube was a new world in which totally unexpected phenomena occurred.

Faraday, by evaporating the carbonic acid snow in an exhausted receiver, succeeded in lowering the boiling point from $-78°$ C. to $-110°$ C. Combining the action of this low temperature with pressure, all gases by the year 1844 were liquefied, with the exception of the three elementary gases, hydrogen, nitrogen, oxygen, and the compound gases, carbonic oxide, marsh gas and nitric oxide. Andrews, some twenty-five years after the work of Faraday, attempted to induce change of state in the uncondensed gases, by using much higher pressures than Faraday employed. Combining the temperature of the solid carbonic acid bath with pressures of 300 atmospheres, Andrews found that none of the gases exhibited any appearance of liquefaction even in such high states of condensation; so far as change of volume by high compression went, Andrews confirmed the earlier work of Natterer, showing that the gases become less and less compressible with growing pressure. While such investigations were proceeding, Regnault and Magnus had made refined observations on the laws of Boyle and Gay Lussac, but none had made a complete study of the isothermals of a liquefiable gas through wide ranges of temperature. This was accomplished by Andrews in 1869, and his Bakerian Lecture 'On the Continuity of the Gaseous and Liquid States of Matter' will always be regarded as an epoch-making investigation.

During the course of this research Andrews observed that liquid carbonic acid raised to a temperature of 31° C. lost the sharp concave surface of demarcation between the liquid and the gas, the space being now occupied by a homogeneous fluid which exhibited, when the pressure was suddenly diminished or the temperature slightly lowered, a peculiar appearance of moving or flickering striæ, due to great local alterations of density.

At temperatures above 31° C. the separation into two distinct kinds of matter could not be effected even when the pressure reached 400 atmospheres.

This limiting temperature of the change of state from gas to liquid Andrews called the critical temperature. He showed that this temperature is constant, and differs with each substance, and that it is always associated with a definite pressure peculiar to each body. Thus the two constants, the critical temperature and pressure, which have been of the greatest importance in subsequent investigation, came to be defined, and a complete experimental proof was given that "the gaseous and liquid states are only distinct stages of the same condition of matter, and are capable of passing into one another by a process of continuous change."

The fundamental idea that gaseous pressure was the result of a succession of strokes of bombarding particles was first put forward by Bernouilli about the middle of the eighteenth century. Later the same suggestion was employed by Lessage, of Geneva; and Herepath,

in his 'Mathematical Physics,' published in 1847, made a considerable advance in the application of the theory. Joule made a great step in 1821 by calculating the mean translational velocity of the particles of hydrogen required to produce in a closed space the pressure of one atmosphere at the melting point of ice; but the great advance in the application of the theory was due to Clausius, ably supported later on by Maxwell, Boltzmann, Meyer and Van der Waals.

A very important series of experiments was made by Joule and Kelvin 'On the Thermal Effects of Fluids in Motion,' about 1862, in which the thermometrical effects of passing gases through porous plugs furnished important data for the study of the mutual action of the gas molecules.

Such experiments, along with a knowledge of the specific and latent heat, together with the rate of diffusion, viscosity and thermal conductivity, furnished material for a complete thermo-dynamical treatment of the gaseous state. Professor Van der Waals entered upon this difficult inquiry in 1873 by publishing an essay 'On the Continuity of the Gaseous and Liquid States,' full of new and suggestive ideas.

The equation of continuity Van der Waals developed involved the use of three constants instead of one, as in the old law of Boyle and Charles, the latter being only utilised to express the relation of temperature, pressure and volume when the gas is far removed from its point of liquefaction. Of the two new constants, one represents the molecular pressure arising from the attraction between the molecules, the other four times the volume of the molecules.

Given these constants for a gas, Van der Waals showed that his equation not only fitted into the general characters of the isothermals, but also gave the values of the critical temperature, the critical pressure and the critical volume. In the case of carbonic acid the theoretical results were found to be in remarkable agreement with the experimental values of Andrews. This gave chemists the means of ascertaining the critical constants, provided sufficiently accurate data derived from the study of a few properly distributed isothermals of the gaseous substance were available. Such important data came into the possession of chemists when Amagat published his important paper on the isothermals of oxygen, nitrogen, hydrogen, ethylene, etc., in the year 1880. It now became possible to calculate the critical data with comparative accuracy for the gases oxygen and nitrogen. This was done by Sarrau in 1882, and the subsequent static liquefaction of oxygen by Wroblewski in 1883 confirmed the theoretical conclusions. No doubt a great impulse had been given to research in this department by the suggestive experiments of Pictet and Cailletet in 1878.

The theory of Van der Waals has been of the greatest importance in directing experimental investigation in attacking the difficult problem of the liquefaction of the permanent gases. In the space of an hour's lecture it is impossible to do justice to all the workers who have contributed materially to the advance of this department.

The following table of the names of investigators who have contributed to the experimental, theoretical, or practical study of gases at once suggests much deserving work that otherwise ought to have been discussed had time permitted. All we can do is to make in rapid succession the fundamental experiments illustrative of the great advances made during the century. [Experiments here.]

Table of Investigators.

Dalton, Gay Lussac, Faraday, Davy, Avogadro, Caignard de la Tour, Regnault, Magnus, Thilorier, Natterer, Deville, Graham, Joule, Kelvin, Andrews, Herepath, Wollaston, Clausius, Rankine, Maxwell, Boltzmann, Stoney, Tait, Van der Waals, Mendeleef, Amagat, Rayleigh, Crookes, Pictet, Cailletet, Wroblewski, Olszewski, Kundt, Warburg, Witkowski, Onnes, Young, Ramsay, Leduc, Mathias, Siemens, Kirk, Coleman, Linde, etc. etc.

It is unnecessary to enter into any detailed discussion of the progress made since liquid air came to be an instrument of scientific research, as this has been done in previous Friday Evening Discourses; but recent improvements in apparatus and methods of manipulation may be worthy of consideration. The facility and ease of handling, storing and working with liquid gases is dependent on the use of vacuum vessels, many types of which may be seen in Diagram 1.

Fig. 8 of the diagram is a copy of the highly exhausted calorimeter used in 1875 in a research 'On the Physical Constants of Hydrogenium,' * and which for all intents and purposes was a vacuum vessel made of brass instead of glass. The use of such an arrangement to guard against gain or loss of heat was a natural deduction from the early work of Dulong and Petit on radiation. Many convenient forms may be given to such vessels, and several varieties are represented in the diagram. The types 3 and 9, containing a spiral-tube arrangement to relieve the contraction when the inner vessel has to be joined to the outer by a tube, are of special importance when regenerative methods have to be employed, because, while isolating the metallic coil, it allows the liquid gas as it is formed to drain away from the interior, and be collected in another vacuum vessel outside of the main apparatus. This device, developed after many unsuccessful attempts to construct such a vacuum vessel, was found to be essential for the easy production and collection of liquid hydrogen, and as all the Royal Institution designs for such special vessels have been made in Germany, they have been supplied to and utilised by other workers, unconscious, it may be, where or how they originated. Such vessels may be silvered in whole or in part, or the vacuum may be nothing but mercury vapour. When liquid hydrogen has to be kept naturally, the vacuum vessel in which it is collected is placed in another vessel full of liquid air, so that the external wall of the hydrogen vacuum vessel is kept at about $-190°$ C.,

* Trans. Royal Society of Edin. vol. xxvii.

or better at a lower temperature by exhausting the surrounding air. Many combinations of vacuum vessels can be arranged, and the lower the temperature at which we have to operate the more useful they become.

As the great object in producing liquid gases is in the first place scientific utility in opening up new fields of research, the application of liquid hydrogen as an agent by means of which the more volatile gases contained in atmospheric air may be separated is of some interest.

The diagram, Fig. 1, will make the process of separation intelligible. *A* represents a vacuum-jacketed vessel, partly filled with liquid air, in which a second vessel *B*, was immersed. From the bottom of *B* a tube, *a*, passed up through the rubber cork which closed *A*, and from the top of *B* a second tube, *b*, passed through the cork and on to the rest of the apparatus. Each of these tubes had a stopcock, *m* and *n*, and the end of the tube *a* was open to the air. A wider tube also passed through the cork of *A* and led to an air-pump, whereby the pressure above the liquid air in *A* was reduced, and consequently the temperature of the liquid by resultant evaporation. To keep the inner vessel, *B*, covered with liquid, a fourth tube, *r*, passed through the cork, and its lower end, furnished with a valve, *p*, which could be opened and closed by the handle *q*, dipped into liquid air contained in the vessel *C*. As the pressure above the liquid in *A* was less than that of the atmosphere, on opening the valve *p* some of the liquid air was forced through *r* into *A* by the pressure of the atmosphere, and in this way the level of liquid in *A* was maintained at the required height.

Since *B* was maintained at the temperature of liquid air boiling at reduced pressure, the air it contained condensed on its sides, and when the stopcock *n* was closed and *m* opened more air passed in through the open end of *a*, and was in turn condensed. In this way *B* could be filled completely with liquid air, the whole of the most volatile gases being retained in solution in the liquid.

The tube *b*, passing from the top of *B*, was connected with a three-way stopcock *d*, by which it could be put in communication with the closed vessel *D*, or with the tube *e*, and by which also *D* and *e* could be connected. The tube *e* passed down nearly to the bottom of the vacuum-jacketed vessel *E*, and out again through the cork; and so on to a gauge *f*, and through a sparking tube *g* to a mercury pump *F*.* The stopcock *n* being still closed, the whole of the apparatus between *n* and the pump, including the vessel *D*, was exhausted, and liquid hydrogen introduced into *E*. The three-way cock *d* was then turned so as to connect *b* with *D*, and close *e*, and then *n* opened. *B* was thereby put in communication with *D*, which was at a still lower temperature than *B*, the air being at 63° absolute, while the hydrogen is at 21° absolute, and any gas dissolved in the liquid air in *B*, along with some of the more volatile nitrogen, distilled

* The Sprengel in figure is simply diagrammatic.

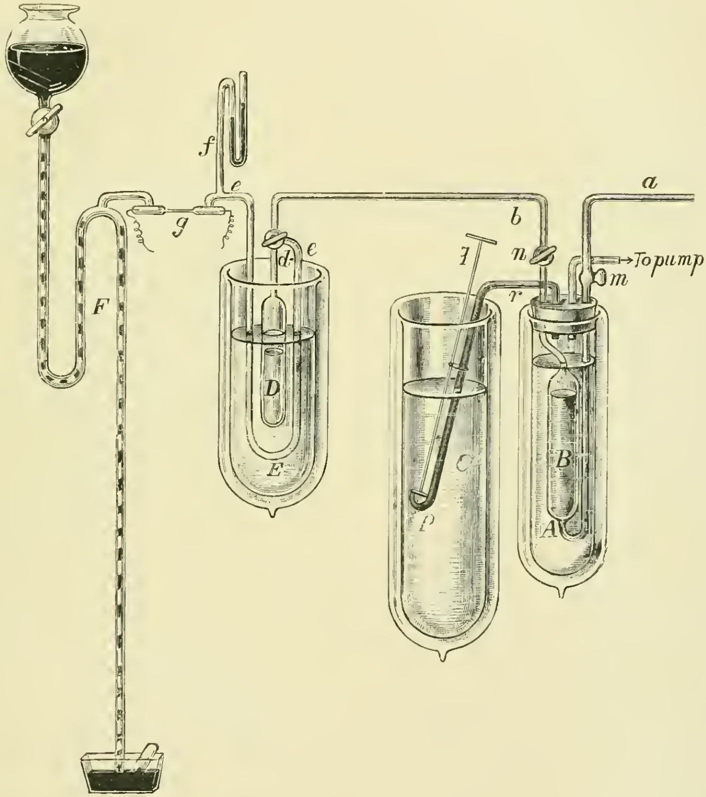

FIG. 1.

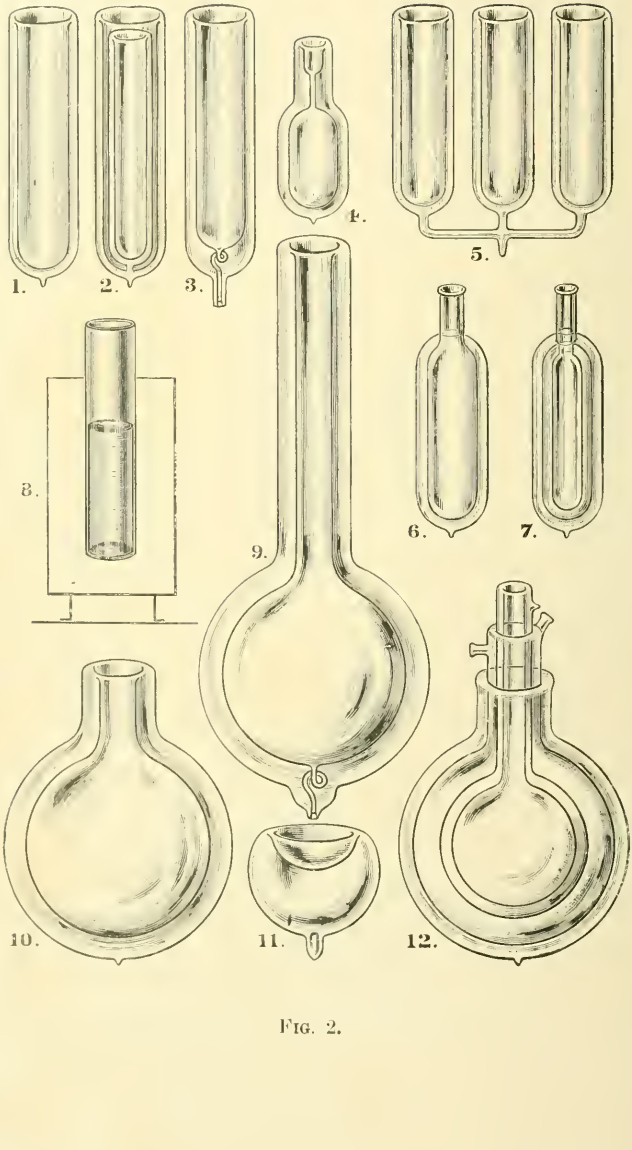

Fig. 2.

over, and the latter condensed in a solid form in *D*, while any gas incondensable at the boiling point of hydrogen filled the vessel at a low pressure, the latter being recorded by the manometer *f*. When a small fraction of the liquid in *B* had thus distilled, the stopcock *d* was turned so as to close the communication between *D* and *b* and open that between *D* and *e*. Gas from *D* passed into the sparking tube *g*, but in so doing it had to pass through the portion of *e* which was immersed in liquid hydrogen, so that condensable matter, like nitrogen or oxygen vapour, carried forward by the stream of gas, was frozen out.

On passing electric discharges through the tubes containing the most volatile of the atmospheric gases collected as above, they glow with a bright orange light, not only in the capillary part, but also at the poles, and at the negative pole in particular. The spectroscope shows that this light consists in the visible part of the spectrum chiefly of a succession of strong rays in the red, orange, and yellow, attributed to hydrogen, helium, and neon. Besides these, a vast number of rays, generally less brilliant, are distributed through the whole length of the visible spectrum. They are obscured in the spectrum of the capillary part of the tube by the greater strength of the second spectrum of hydrogen, but are easily seen in the spectrum of the negative pole, which does not include the second spectrum of hydrogen, or only faint traces of it. Putting a Leyden jar in the circuit, while it more or less completely obliterates the second spectrum of hydrogen, it also has a similar effect on the greater part of these other rays of, as yet, unknown origin. The violet and ultra-violet part of the spectrum seems to rival in strength that of the red and yellow rays, if we may judge of it by the intensity of its impressions on photographic plates.

As these gases probably include some of the gases that pervade interplanetary space, search was made for the prominent nebular, coronal and auroral lines. No definite lines agreeing with the nebular spectrum could be found, but many lines occurred closely coincident with the coronal and auroral spectrum. Before any final conclusion can be reached, larger quantities of the gases must be collected, but this will not be difficult now that the method of separation has proved a success. It may safely be predicted that liquid hydrogen will be the means by which many obscure problems of physics and chemistry will ultimately be solved, so that the liquefaction of the last of the old permanent gases is as pregnant now with future consequences of great scientific moment as was the discovery of the liquefaction of chlorine in the early years of the century.

INDEX TO VOLUME XVI.

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